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and Physical Sciences

Umberto Bottazzini
Jeremy Gray

Hidden Harmony— Geometric Fantasies

The Rise of Complex Function Theory



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Hidden Harmony— Geometric Fantasies

The Rise of Complex Function Theory

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*We dedicate this book to our respective
families for all their support over the years*

*– and to everyone who thought we would
never finish it*

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Introduction

I.1 Introduction to the Main Themes of the Book

This book is the first to be devoted to the history of analytic function theory since Brill and Noether published their Bericht über die Entwicklung der Theorie der algebraischen Functionen in älterer und neuerer Zeit in the *Jahresbericht der Deutschen Mathematiker Vereinigung* in 1894. Indeed, because that work leaves out many topics that belong to the theory of analytic functions but not algebraic functions, it can reasonably be argued that our book is the first ever to be written exclusively on this subject. This is rather strange given the importance of analytic function theory within mathematics and the attention that historians of mathematics have paid to the development of the theory of real functions in the nineteenth century.

It is indeed surprising that the rise of complex or analytic function theory in the nineteenth century from almost nothing to one of the dominant fields of mathematics has not been told before, because it is a story worth telling and analysing in its own right. The theory of functions, as it was generally referred to throughout the later half of the century, was much more concerned with complex than with real variables and functions. To tell this story is to redress the balance and to restore a family of overlapping perspectives on the mathematics of the day.

In fact, we provide here the first full treatment of the work of several major mathematicians in the context of complex function theory. Gauss's work has not been treated in this way since Schlesinger contributed his essay (1912) to the *Gauss Werke*, although Cox (1984) has written a good account of Gauss's work on elliptic functions. Ours is the first thorough analysis of Cauchy's contributions. They were described by Smithies (1997), but he stopped his account in 1831, after which the reader must consult Belhoste's biography (1985, 1991); Grabiner (1981) confined her attention to Cauchy's work on real analysis. Riemann's work was discussed quite fully by Laugwitz (1999), but he only skirted the central topic of Abelian functions. Weierstrass has also been studied only selectively by Dugac, Manning, and Ullrich; again, our chapter is the first to discuss all his work on

complex analysis. Although there are two good accounts of Kovalevskaya's life and work (Koblitz 1983; Cooke 1984), there is nothing on Schwarz, although the English translation of Arild Stubhaug's biography of Mittag-Leffler appeared in 2010. Our account of Poincaré's contribution is fuller than most but still only partial. Hadamard's work in this area has been written about very helpfully by Maz'ya and Shaposhnikova, but there is very little on the other French mathematicians of note, Borel and Montel. We too have written about several parts of the subject in one place or another, see for example Bottazzini (1986, 1992a,b, 2003) and Gray (1984a, 2000a).

1.1.1 Accounting for the Rise of Complex Function Theory

But we have not tried just to tell a story and certainly not one which is, or is solely, a history of ideas. We have tried both to describe the rise of complex analysis and to explain it. That is why a major and novel aspect of our work is the emphasis it places on elliptic functions as one of the principal impetuses for a theory of complex functions. The arrival by 1830 of elliptic functions in the hands of Abel and Jacobi caused tremendous excitement, and our history starts with that event and its immediate consequences because elliptic functions were such a spur to the development of complex function theory. They were in an essential way *complex* functions, and yet their foundation in the inversion of an integral containing a two-valued integrand was soon felt to be both inadequate and mysterious. Gauss felt this acutely and took considerable steps to create a properly complex theory of doubly periodic functions that did not start from an integral; Jacobi turned to the theory of theta functions.

But they were not the only spur: Cauchy was drawn to the subject as an analyst, someone who had taken profound steps towards a rigourisation of the calculus and knew very well the way complex issues had become entangled with problems in evaluating integrals. Cauchy, however, was not so strongly drawn to the theory of elliptic functions, and his insights into the integration of complex functions took over 20 years to suggest a rich, organised theory to him. It was left to others, Liouville and Hermite, to find a way to bring enough analysis to bear to re-found elliptic functions as doubly periodic complex functions.

As the years went by, complex function theory became steadily more involved with other branches of mathematics. We argue that these applications or interventions of complex function theory in both pure and applied aspects of mathematics are among the reasons why the theory became more and more highly regarded: it proved its worth in fundamental ways in number theory, in mechanics, in the theory of linear differential equations, and even in geometry, and it developed a most fruitful interaction with potential theory and the theory of harmonic functions. We follow these connections far enough, we believe, to establish our general claim that they helped advance the merits of complex function theory, and when we judge

that this claim has been established and nothing would be served by going further, we stop.

We have dealt differently with the subject of topology, because it was not a pre-existing discipline to be enriched by the new ideas. Rather, it was a new subject, created in part to meet the increasing need for rigour in complex function theory itself. From Riemann's intuitive vague and yet profound idea of a "Riemann" surface to the work of Harnack on connectedness and path-connectedness, complex function theory generated a number of basic concepts in both geometric and point set topology.

Finally, after a brief look at the early attempts to produce a theory of complex functions in several variables, we conclude our account of the rise of complex function theory with an examination of the textbook tradition and the emergence of what can be called the standard or canonical version of the theory, the one that came to be presented internationally down to the present day. This has involved us in analysing some 60 books written in nine different languages and published in about a dozen countries, one of first times this sort of work has been done for the history of any branch of mathematics.

1.1.2 Putting the Complex in Context

The rise of complex function theory cannot, however, be understood solely as a history of ideas, and we have not only traced it as a sequence of ideas, theorems, and theories, but we have also paid attention to the historical context, the biographies of the main actors, the different reactions and developments in various countries depending on the national traditions, and so on. One would expect that the balance would shift from France to Germany, from Paris to Berlin, because that is true of so many aspects of the nineteenth century. It took less than 60 years for the nation whose citizens filled the ranks of Napoleon's Army and watched Moscow burn to see a Prussian army at the gates of Paris, and no-one then or since has doubted that this says something profound about almost every aspect of life in those two countries. What, more precisely, it says remains heavily debated. In the context of the history of mathematics, it is true that the young ambitious person seeking the best education in the 1820s would go to Paris and in the 1870s would go to Berlin. But single individuals in small fields can buck trends, and the shift suggested by the names alone (Cauchy, but Riemann and Weierstrass) was in fact a real one. The strong presence of Italian mathematicians points up what historians of mathematics have been writing about for a generation: the revitalised Italian community after unification. The almost complete absence of British mathematicians for most of the nineteenth century is further melancholy evidence of the narrowly utilitarian instincts of the British when they managed to be intellectual at all.

We note below several occasions when the biographical, institutional, and more general social sides of the story predominate, but nowhere is this clearer than in the reception of the new ideas and the writing of text books.

In any history of ideas, the historian seeks to show how things once thought about in one way became thought about in another. As complex function theory developed many ideas were first introduced naively and only slowly refined. Definitions were lacking, and when provided were sometimes inadequate by later standards. Moreover precision, when it became available, could be misleading: mathematicians on occasion offer a clear definition with very few ideas about its deepest implications—as the example of continuity in real analysis shows. Sometimes these problems can be confronted directly, as with the very definition of an analytic function, but more often one has to ride out a long period of some vagueness. Let us note some specific issues: Cauchy, for example, often used the phrases “continuous” and “finite and continuous” very loosely to mean something like “complex analytic”. Similar problems occur with counting roots according to their multiplicities, with $\lim$ versus $\limsup$, and points of infinity and poles. Mathematicians throughout the eighteenth and nineteenth centuries spoke freely of many-valued functions (the simplest example being $\sqrt{z}$). It is in this spirit we have used the nineteenth century term “complex function”, for example, where a modern mathematician would use “analytic function”.

It is helpful here to recall André Weil’s remark:

The mathematicians of the eighteenth century were in the habit of speaking of “the metaphysics of the infinitesimal calculus” and the “metaphysics of the theory of equations”. They understood by this a set of vague analogies, difficult to grasp and difficult to formulate but which nonetheless seemed to play an important role at a given moment in research and mathematical discovery. [...] Nothing is more fertile, all mathematicians know this, than these obscure analogies, these cloudy reflections of one theory on another, these furtive caresses, these inexplicable misunderstandings; nothing gives more pleasure to the researcher. (Weil 1960, 52)

There is therefore no truly satisfactory way to represent the original ideas of mathematicians when they are like this. To say nothing is to produce confusion. To silently bring them into line with modern standards not only introduces anachronisms but also brings in historical falsehoods and nullifies the purpose of a history. To correct them in more than the most egregious cases is to encumber genuine advances with the admission of genuine blunders and thereby diminish the work of major mathematicians. The best policy is to read on in a spirit of dialogue with the earlier authors, aware, as one might be, of the limitations and false implications of their papers and books, and waiting to see when, if at all in the period, a better light was shone on the subject. In this way one can grapple with more of the complexity, and the drama, of the past.

1.1.3 Notation

Notation is also a problem in the history of mathematics. For Cauchy it was natural to let y be a complex function of a complex variable x , because he was engaged in exploring the consequences of letting variables and functions become complex.

Accordingly, in his writings we can find y treated as a function of the complex variable $x + iz$. Anyone who knows the old literature will know that there was never any agreement about subscripts versus superscripts, that the now-standard notation for partial derivatives came in only slowly, that summations might or might not have their range clearly indicated, and that integrals could be written in as $\int f(t)dt$ or $\int^t f(t)dt$ and mean what we would write today as $\int_a^t f(s)ds$ according to context. If we modernise and standardise the notation we alter the texts. Harmlessly, one might say, but it has the effect of making the originals harder to read. Leaving the notation unaltered can bring one closer to how these mathematicians actually thought, and in fact Cauchy's use of notation is not hard to adjust to. That said, we have tended quietly to modernise the appearance of integrals, to replace d with ∂ when it was clearly meant, and to tidy up subscripts and superscripts when it helps. We have kept the old word "infinity" for what would today be called a pole until it is historically sensitive to use the newer term. Above all, we have replaced the symbol $\sqrt{-1}$ with i whenever we believe that no harm is done thereby.

I.2 The Book Chapter by Chapter

The book opens with the triumphant emergence of elliptic functions. These functions have their origin in the study of elliptic integrals, and they, as their name suggests, go back to study by Newton and later workers on the motion of the planets. As with so many other matters of mathematical importance elliptic integrals were taken up and studied in depth by Euler, but for the purposes of this book it is Legendre's lengthy account of them that forms the starting point. Legendre computed tables of values for them, found differential equations for the so-called complete elliptic integrals as functions of the parameter upon which they depend, and described in detail the sorts of situations in mechanics where these integrals could be found. It is Legendre, modelling himself deliberately on Euler, who made elliptic integrals into a definite mathematical topic. His elliptic integrals provided one of the first new functions to enrich mathematics since the days of Euler.

Legendre's elliptic integrals were firmly real functions of their upper endpoint and they involved only a real parameter. Everything changed with the nearly simultaneous realisation by Abel and Jacobi that it is much more productive to invert the integral and to let the variables be complex. This step turned the elliptic integrals into elliptic functions strikingly analogous to the familiar trigonometric functions, explained some puzzling features of the previous study of elliptic integrals, and opened a wide door to new research. Their work was almost immediately recognised as significant with the joint award of the prestigious Paris Académie prize of 1830 to Abel (posthumously) and Jacobi. Their work provided the first examples, other than the trigonometric functions, of complex-valued functions of a complex variable.

But Abel and Jacobi were not the first to take this momentous step. Unknown to them both, Carl Friedrich Gauss had been confiding results about elliptic functions to his notebooks since 1797. He had indeed gone further than them; notoriously he

said of Abel’s work that Abel had “gone one third of the way”, a claim we discuss below (Sect. 1.5). Gauss had also connected their study to the hypergeometric differential equation, and in various other ways fully embraced the idea that the proper domain for the theory of functions was the complex domain. He had ideas about the nature of complex integration when Abel and Jacobi were still proceeding formally and several proofs of the fundamental theorem of algebra.

It is well known that Gauss did not publish much of his work, preferring, it is said, the motto “Few but ripe”.¹ His French peer in the next generation, Augustin-Louis Cauchy, had no such inhibitions. But whereas Cauchy’s *Cours d’analyse* of 1821 and *Résumé* of 1823 did so much to put real analysis on the map, we show in Chapter 2 that circumstances were to deny his contemporaries any such account of complex analysis. What eluded Cauchy for over 20 years was a good way to make precise the perception that within the growing collection of facts about maps from $\mathbb{R}^2$ to $\mathbb{R}^2$ there was a coherent collection of ideas that would make a theory of maps from $\mathbb{C}$ to $\mathbb{C}$. Indeed, that story will occupy us for many pages.

Cauchy’s work in the 1810s and 1820s is rooted in eighteenth-century methods for evaluating integrals, specifically those that used what was called the passage from the real to the imaginary. This involved mathematicians such as Clairaut, d’Alembert, and Euler in the study of complete differentials and their integrals, and here the Cauchy–Riemann equations naturally make their first (one might say, unannounced) appearance. In a purely formal way, complex terms appeared in many places when factorising expressions or making substitutions. Cauchy built on this work, and to explain some of its paradoxical conclusions made a detailed study of the introduction of imaginaries in his *Memoir* (Cauchy 1814, first published 1827). There he showed that the reduction of a double integral to a repeated integral can be affected by the presence of singular integrals, an insight that was eventually to be the key to the theory of integrating a complex function on a closed path.

As we show in detail for the first time in Chapter 3, at one time or another in the 1820s, 1830s, and early 1840s Cauchy had a theory of complex integration that included the calculus of residues (a term he introduced in 1826) and a theory of power series expansions of “complex” functions. But he had taken himself into exile for political reasons in 1830 and was away from France for almost all of the 1830s. This did not stop him publishing—apparently nothing could—but it deprived him of an opportunity to write up his ideas in a systematic way, and it robbed his numerous articles of some of their impact. By the time he was securely back in Paris in the late 1840s he was approaching 60, and this, and the rivalry of the younger generation, may have stimulated him to publish, for only now did Cauchy draw his ideas together and promote them. In a fast-flowing stream of papers he responded to the work of others, often to claim priority, to draw out new and forgotten conclusions from his published and unpublished work and to find new ideas (the logarithmic counter dates from this period). For the first time he found the right general setting

¹A practice he only broke when it came to astronomy.

for his ideas and settled on the geometric description of complex numbers as points in the plane, which he had hitherto resisted.

As for elliptic functions, which were never a topic close to Cauchy's heart, by the 1840s Liouville and Hermite, independently of Cauchy, had begun to anchor them in a theory of complex functions. In so doing, Liouville had come to the realisation that an analytic function which is defined and bounded in the entire complex plane reduces to a constant (the correctly named Liouville's theorem), which can be taken as one of the first major landmarks on the way to recognising analytic functions as a distinctive class of functions. The inevitable priority spat hints at the fact that only now could the concepts of analytic function theory ground the edifice of elliptic functions. However, it was also the case that Cauchy's uncertain grasp of the geometric approach to function theory meant that, Puiseux's memoir of 1851 aside, there was still little that could be said function-theoretically about algebraic functions beyond the elliptic case.

Chapter 4 begins with Jacobi's reformulation of the theory of elliptic functions. Throughout the 1830s mathematicians had tried to confront the unfortunate fact that an elliptic function was defined by inverting an elliptic integral, but an elliptic integral, when treated in the complex setting, has a two-valued integrand. One successful way forward was to restart their story with what had been one of its crowning successes: the representation of elliptic functions as quotients of theta functions. Jacobi saw that it was possible to reverse the argument, start with theta functions, and derive elliptic function theory from them, although, as Weierstrass was to point out, he did not fully extend the theory from the real to the complex setting in every respect. Other mathematicians, as well as Jacobi, looked at a graver problem still, extending the theory of elliptic integrals and elliptic functions to the situation where the integrand involves not the square root of a polynomial of degree 4 but one of degree 5 or more, what became called the hyperelliptic case. Even Jacobi stumbled here and seems to have suggested that it could not be done directly, but only by passing to the study of theta functions in two or more variables. This was done by Göpel and Rosenhain and extended by Hermite and Carl Neumann. In this way the hyperelliptic integral and beyond it the general algebraic integral became established as central problems in the emerging theory of complex functions.

Complex functions, and a measure of complex function theory, were beginning to find uses. Jacobi had shown that Poncelet's closure theorem could be tackled by means of them, and Gauss's work showed that they turn up naturally in number theory and the study of quadratic (and higher) reciprocity. Dirichlet also used elementary ideas about complex variables to establish the remarkable result that every arithmetic progression without common factors contains infinitely many primes.

Chapter 5 is devoted to the insights of Bernhard Riemann. The piecemeal discovery of complex function theory as a theory of analytic functions defined on the complex plane was disrupted so completely by Riemann that it was a long time before the implications of his ideas were fully understood, even though—contrary to the impression sometimes given—they were promptly and energetically studied. In his doctoral thesis of 1851 Riemann made the definition of complex differentiability

and its consequence the Cauchy–Riemann equations central to his definition of the relevant functions. He deduced from this that a complex function can be defined on any two-dimensional patch, thus opening the way to the study of complex functions on non-simply connected domains and in particular the study of elliptic functions as functions on a torus. He proposed the theorem that any two simply connected domains with boundaries—this rules out the entire plane—are equivalent for the purposes of complex function theory (the Riemann mapping theorem). In 1857 he pushed these ideas through to a resolution of the outstanding problem of the integrals of algebraic functions (Weierstrass had essentially done the hyperelliptic case in 1854 and 1856). He gave the first theory of algebraic functions on a complex curve and amplified it with a thorough-going theory of theta functions in any number of variables. It was a profound success for his geometric, and indeed topological, way of thinking. He followed it with his dramatic account of the hypergeometric equation in the complex setting, important consequences for the construction of minimal surfaces, and, famously in the twentieth century, his treatment of the zeta function as a complex function, with its deep implications for the distributions of the primes.

Riemann’s visionary presentation left much for his successors to do. Some of his bolder claims collapsed under careful criticism. For a generation his use of Dirichlet’s principle, which he had tried to prove, was held in great suspicion. His hope that his geometric analysis captured exactly what an approach based on infinite arithmetical expressions would capture was shown to be unfounded (arithmetical expressions can do more). But his elementary insight that the Cauchy–Riemann equations were the place to start and his use of geometric and topological methods to tackle advanced problems in complex function theory were widely regarded as decisive, albeit very difficult.

By the 1850s complex function theory was increasingly well established in France because of the work of Cauchy and in Germany through the work of Riemann. The next major figure with a vision for the subject was Karl Weierstrass, whose lifelong ambition was to create a theory of Abelian functions, as the functions obtained by inverting an arbitrary algebraic integral were called in honour of Abel. They would be functions of several complex variables, and so whenever possible Weierstrass preferred methods that worked in any number of variables and avoided methods that worked in just one. By the time he arrived in Berlin in his glory as the conqueror of the hyperelliptic integral, he had renounced the Cauchy integral theorem, and he distrusted the Cauchy–Riemann equations too; neither work satisfactorily when more than one variable is involved. So, as his famous letter to Schwarz attests (see p. 385) everything came down more and more to the use of algebraic methods and convergence arguments.

In his own way, however, Weierstrass thought as deeply about complex function theory as Riemann had done. He was not the first to observe the crucial distinction between finite poles and essential singularities (Casorati in Italy and Sokhotskii and Russia had noticed this independently), but he was the first to begin to understand it and to make real use of this distinction and thereby to clarify obscure features about the way a function “becomes infinite”. His representation theorem clarified

completely the question of what the zero set of a complex function can be, and his disciple Mittag-Leffler then did the same service for the polar set. His theory of elliptic functions evolved until in the 1870s it was based on his famous $\wp$ function, and rooted in a significant argument about what functions can satisfy an algebraic addition theorem. Ironically, it was only in the study of Abelian functions that Weierstrass made any serious mistakes and could not get the deep results he wanted, but that only showed how difficult that subject was.

One way the theory of complex functions advanced was because of the fruitful connections found for it with other, better established, fields of mathematics, and we look at some of these in Chap. 7. Weierstrass had, of course, disdained the intimate connection that Riemann had exploited between complex function theory and harmonic function theory, but others found it worth exploring. The first difficulty here, however, was that the fundamental theorems of potential theory were themselves in trouble. With Dirichlet's principle in disgrace, the Dirichlet problem had to be solved some other way, and among the first to do this was Schwarz, Weierstrass's most loyal and ambitious former student. Schwarz gave a rigorous account of how the Dirichlet problem can be solved for a large but obscure class of boundaries, and he was followed by Neumann, Harnack, and Poincaré. Schwarz in fact performed the function for Weierstrass of recapturing several of Riemann's theorems in a way that was more acceptable (and indeed more rigorous) than Riemann's first presentation of them. He solved special cases of the Riemann mapping theorem in this spirit, as did Christoffel independently, and likewise engaged in the study of minimal surfaces. He also followed Fuchs, another Berlin graduate much influenced by Weierstrass, in a study of the complex hypergeometric equation and investigated when all its solutions are algebraic functions.

Other problems in applied mathematics were enriched by the use of complex methods in surprising ways. Several stand out. Elliptic integrals had been identified by Legendre as important in the motion of the top, and the corresponding elliptic functions studied by Jacobi. In the 1870s the motion of the top was studied again by Kovalevskaya and later on by Klein and Sommerfeld. The three-body problem, although decisively reformulated by Poincaré in the late 1880s, still concealed answers about collisions that only yielded to Sundman's complex methods at the end of the century. The hypergeometric equation and the related study of the so-called special functions of mathematical physics were clarified and deepened by being made complex, as the discovery of Stokes's sectors and the theory of confluent differential equations show. Finally, the conformal character of a complex analytic map proved its worth in the reformulation of the theory of minimal surfaces, in conformal transformations of problems in two-dimensional fluid flow, and in the elucidation of a complex structure on a (Riemann) surface.

Chapter 8 takes up two complementary developments in the theory of complex functions that came about in the period from 1880 to around 1910. Geometric function theory is the use of geometric, chiefly Riemannian, ideas. It received a considerable boost when Dedekind used it in 1877 to illuminate the theory of modular functions, an important offshoot of the theory of elliptic functions. It was immediately taken up by Klein and a year or so later independently by Poincaré,

who made it into the first major application of non-Euclidean geometry. Also in 1879 Picard published his two theorems about the behaviour of a function with an essential singularity, which led to a great many attempts in France and Germany to exploit and deepen his unexpected discovery.

The work of Poincaré and Klein suggested strongly that every Riemann surface is the quotient of the appropriate simply connected surface by the action of a discrete group (this is the content of the uniformisation theorem). But it stopped well short of a proof, and proofs were only found by Koebe and Poincaré independently in 1907, after Hilbert had made the uniformisation theorem the subject of one of his Paris problems. Rigorous proofs of the Riemann mapping theorem also date from this time, when for the first time topological methods could be developed to deal with general boundaries.

In the same three decades, French mathematicians (Laguerre, Poincaré again, Hadamard, and Borel) had been at work investigating questions that seek to describe properties of a complex function from properties of its power series expansion. They also took up the theory of entire functions and the properties of *genre* and order of an entire function. Along with the Italian mathematicians Arzelà and Ascoli and the American Osgood, they looked at questions about sequences of functions, and out of this network of ideas, which includes Picard's theorems, came Montel's work on normal families. At the very end of our period, a remarkable amount of complex function theory was brought together by Fatou and Julia in their theory of the iteration of rational functions.

Chapter 9 discusses the uncomfortable fact that such rapid progress was not to be made in the theory of complex functions of more than one variable. Where the direction of influence had run from elliptic functions to complex functions, it was to run from complex functions of several variables to Abelian functions, and then only slowly. The zero set and the singular set of a function of several variables had to be elucidated, and even Weierstrass made mistakes. The theory of Abelian functions and their connection to the general theory of theta functions was again difficult (in fact the central problem, the Schottky problem, which asks for a characterisation of the theta functions that arise from Abelian functions, was not to be solved until the 1980s). Some insight came with Cousin's generalisation of the Mittag-Leffler theorem to the several variables case, and gradually Poincaré, Picard, and Appell produced the first rough but complete theory of the subject. Then, in the early years of the twentieth century, Hartogs and E.E. Levi were the first to explore the crucial novelties concerning the possible domains of functions of several variables, and Poincaré and Reinhardt showed how the Riemann mapping theorem could not be generalised to higher dimensions.

We have ended this book with a thorough look at the evolution of textbooks in the subject. Our analysis in Chapter 10 not only shows some unsurprising national features, but also a growing recognition that complex differentiability and the Cauchy–Riemann equations are the place to start, that a transition to the Weierstrass's power series methods should be made quite quickly, especially if the important subject of elliptic functions is to be taught, and that Riemannian methods are likely to be the right way to tackle deeper problems. A consensus could only

emerge, however, when the Weierstrassian school was played out, and it emerges, interestingly enough, in the lecture courses Hilbert gave in Göttingen and the books by Bieberbach (1921b and 1931) and Knopp (1913). These are among the classics that Lang hoped everyone would continue to consult.

I.3 The Reception of Complex Function Theory

The reception of a new body of mathematical ideas is not always discussed in the historical literature with the attention it deserves, and here our findings shed new light on the topic. Cauchy's presentation of his ideas was frankly poor until the last few years of his life. It has none of the focus of his two major accounts of real analysis, his *Cours d'analyse* and his *Résumé*, and consequently had much less of an impact. It was scattered between a number of journals and irregular publications, a problem his voluntary exile from Paris for most of the 1830s inevitably exacerbated. Only in the 1850s, when he attempted to pull together his many and varied presentations, did he give French mathematicians good accounts that they could respond to, and by then his ideas were being independently rediscovered and used.

Riemann's ideas, on the other hand, drew a much better response than is commonly realised. The lonely and obscure genius writing, in Ahlfors' accurate phrase, "cryptic messages to the future" must give way to that of the highly regarded man of ideas. He did indeed leave a body of work that needed major rewriting to become rigorous, but over twenty mathematicians contributed to its further development in less than a decade after his early death in 1866. Only his remarkable paper on the zeta function and the distribution of primes lay fallow for 30 years. This achievement is all the more striking because Riemann was unlucky in that so many of his own students and immediate followers died young. His great rival, Karl Weierstrass, paid him the compliment of suggesting that his own students take up aspects of Riemann's work and re-derive Riemann's results by better (i.e., Weierstrassian) methods.

Weierstrass himself had if anything less success in promoting his own approach, despite having the immeasurable advantage of teaching in Berlin University, the leading place in the world in his lifetime for the education of the mathematical elite. But Weierstrass put all his energies into the presentation of his 2-year, 4-semester cycle of lectures on complex functions, elliptic functions, Abelian functions, and their applications. He published very little of his own work in any journal, even some of his lectures to the Berlin Academy of Science were only read but not printed, and the seven volumes of his collected works are largely full of accounts that were being published for the first time. His present and former students had access to lithographed sets of notes, Halphen (1884, 69) recorded that he saw such sets on every mathematician's desk when he went to Germany, but not to books, reprints, or preprints. Instead, Weierstrass relied on the power of his lectures to spread his word, which he revised between each cycle and the next, and by giving few references to existing literature he took steps to shut his students off from alternative

versions. Some remained loyal, but those, even at other Prussian universities, who felt excluded from the process could do little else but go their own way.

The result was that naturally the French relied on a version of the theory that was largely that of Cauchy as described first by Briot and Bouquet in their book of 1859 and then by a succession of authors, and that German mathematicians turned either to more-or-less Riemannian accounts such as Durège provided in 1864 or to Weierstrassian approaches derived from lecture notes. Gradually the geometric versions that relied on the theory of the complex integral distilled into a Cauchy-style approach and a more advanced Riemannian one, the distinction being drawn according to the extent that the concept of Riemann surface was found to be necessary. Those attracted to a Weierstrassian approach valued it for its pedagogic clarity, its rigour, its numerous insights, and its coherence; but even here a crucial distinction emerged. Weierstrass himself disliked the integral and was at pains to avoid it and to rely on power series methods instead. English and American authors, for example, found this too extreme, and blended Cauchy and Weierstrass's methods as they saw fit. German authors, too, felt more free to pick and mix than is often suggested by the historical literature, which wants a tidy dichotomy.

Even if the elements of complex function theory were almost fully established by the triumvirate of Cauchy, Riemann, and Weierstrass, as the literature would often have it, much more was done before it was clear that the foundations had been laid. There was the vigorous series of investigations that form the body of geometric function theory, associated with the names of Carl Neumann, Dedekind, Klein, and Poincaré. There was the equally important body of work on understanding functions defined by power series that one might have expected to have been done by Weierstrass or his students but which was largely a French creation of the 1880s. But there were also significant gaps in the theory that were filled by other mathematicians, such as Laurent's theorem, Rouché's theorem, Morera's theorem, and the Schwarz reflection principle (so named by Carathéodory in 1913).

Curiously, Weierstrassian Berlin may be the only example of a school of function theorists in the period covered by this book. The concept of a school was originally introduced into the history of science and after its loose use in the history of mathematics for a number of years was recently refined by Parshall (2004). On her definition, a school involves a leader who actively pursues research, who has a characteristic approach to his or her subject, who trains students who then develop this approach further, and who collectively publish the results of their research, thereby demonstrating that it has external validity. If the research is particularly successful, the school's focus may produce a new subdiscipline of mathematics, into which the school dissolves. This concept of a school may be particularly well suited to the later nineteenth century and beyond, which is interesting in itself as an observation about the structure of the mathematics profession (it was, of course, constructed with the example of E.H. Moore at Chicago in mind). This fits the situation with Weierstrass in Berlin very well, for Weierstrass had a particular approach to the fundamentals of complex function theory, and he taught it with considerable success to many of his students. Riemann, by contrast, was a major influence, but he did not lead a school. Kronecker was also influential in Berlin, but

he did not build a school around him. Nor did one evolve in Paris—Hermite did not create one. The closest the French came was the group around Émile Borel, but that was more like a collection of equals.

These differences are reflected in the way the subject spread. The school around Weierstrass looked inward to the writings of the master and kept new members away from alternative treatments. Everyone else in France, Italy, or further afield, had much more independence, less support, and could be much more eclectic. By the time our story ends, a generation after the death of Weierstrass, it is clear that orthodoxy was no longer a virtue, and a new consensus was in the making, one that has largely survived to the present day.

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We were writing the final revisions of our book when one of us (JJG) was also reading the manuscript of *Early days in complex dynamics: A history of complex dynamics in one variable during 1906–1942* by Daniel S. Alexander, Felice Iavernaro, and Alessandro Rosa, which had been submitted to the American and London Mathematical Societies series in the history of mathematics. We gratefully acknowledge the opportunity to compare the results of our research with theirs in the areas of overlap. Their book is now published as (Alexander et al. 2011).

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Chapter 1

Elliptic Functions

Scarcely had my work seen the light of day, scarcely could its title have become known to scientists abroad, when I learned with as much astonishment as satisfaction that two young geometers, MM. Jacobi of Königsberg and Abel of Christiania, had succeeded in their own individual work in considerably improving the theory of elliptic functions at its highest points.

Legendre

1.1 Legendre

On 28 June 1830 the Académie des sciences in Paris, the leading scientific institution of the day, announced that its Grand Prize in mathematics of 3,000 francs devoted to work which “presents the most important application of mathematical theories ...or which contains a very remarkable analytical discovery” would be divided equally between Carl Gustav Jacob Jacobi in Königsberg and the family of the late Niels Henrik Abel of Christiania.¹ The Grand Prize was awarded regularly by the Académie, usually for meritorious answers to problems posed by the Académie itself. On this occasion they had decided instead to award the prize to two foreigners, Jacobi, then only 25, and Abel, who had died on April 6, 1829 at the age of 26, for work which had caused a sensation in the small but growing European mathematical community.

One reason for this decision lies behind the scenes with the elderly but still active and influential French mathematician Adrien Marie Legendre (Fig. 1.1). For 40 years he had published papers and books on what are called elliptic integrals. This work had not attracted the interest of others to the degree that he had hoped until unexpectedly in 1827 Abel and Jacobi had taken it up. However, they did not

¹*Procès-verbaux des séances de l'Académie* 9 (1921), 466. The Commission consisted of Poisson, Poincaré, Legendre, with Lacroix as rapporteur.

Fig. 1.1 Adrien-Marie Legendre (in Duren “Changing faces: the mistaken portrait of Legendre”, *Notices of the AMS*, 56, 2009, 1440–1443)



so much extend Legendre’s ideas as completely reformulate them. Each brought to the topic two remarkable new perceptions which enabled them to give it from the start a high degree of elegance, profundity, and power. These ideas were to invert the integrals, as we shall describe below, and to let them have complex endpoints, which is why their work was to be such a stimulus for later workers interested in the theory of complex functions. Of the two steps, Jacobi was often to remark later that the introduction of imaginaries on its own was enough to solve all the riddles of the early theory (see [Dirichlet 1852](#), 10). Moreover, if at first Abel and Jacobi had seemed to be working in complementary ways an evident rivalry soon developed. Legendre was kept in touch mostly by Jacobi, who prudently wrote to him often about his work, for after publishing only a few short papers Jacobi had decided to publish his new theory of elliptic integrals as a book. Abel, however, published prolifically in a new German mathematical journal, although he too corresponded briefly with Legendre.

Legendre was delighted with the attention his favourite subject was receiving and generous enough to welcome it in its new and much altered form. He ensured that the Paris Académie asked him and Poisson to report on it, and Poisson presented their report on Jacobi’s work on 21 December 1829.² This report, based largely on Jacobi’s book, which had been published that April, helped ensure that the German was elected a corresponding member of the Académie on the 8th of February

²*Procès-verbaux des séances de l’Académie* 9 (1921), 373. Poisson’s *Rapport* was published as his (1831).

1830.³ By that time Poisson was one of the outstanding French mathematicians, if not the most authoritative after Laplace's death. A former, brilliant pupil at the École Polytechnique, and a professor there from 1802, Poisson, with the support of Lagrange and Laplace, had risen very quickly to the top of Paris mathematical milieu. In 1812 he was elected to the Institut de France, where he played an increasingly influential role.⁴ Support like this undoubtedly helps explain why Abel and Jacobi's work was taken up when almost contemporary achievements of similar magnitude, such as the discovery of non-Euclidean geometry by Lobachevskii and Bolyai and the ideas of Galois, were not.

The widespread recognition of the importance of elliptic functions as a new but central domain in mathematics is worth documenting and explaining. It is one reason the subject of complex function theory was to grow so rapidly, for in the study of elliptic integrals, as reformulated by Abel and Jacobi, there was much to be done at every level from the deepest to the routine and superficial. Part of the explanation therefore lies in the traditional aspect of the new mathematics, its expression in terms of functions and their integrals. This "routine" mathematics was chiefly the creation of Euler and Lagrange. So far as questions in advanced calculus or analysis were concerned, it was a body of techniques highlighted by certain attractive results.⁵

The calculus of the eighteenth century was remarkably algebraic or formal. Functions had been defined quite generally by Euler on the input–output model but were at heart regarded either as closed expressions or as infinite series.⁶ The meaning of x in, say,

$$(1-x)^{-1} = 1 + x + x^2 + \dots$$

was deliberately vague. It is not true that Euler had no theory of convergence, or that he dealt with power series purely formally. He had, rather, several profound ideas, ranging from the now-standard elementary one to more sophisticated limit-processes to more formal ideas about series independent of their convergence, and he presented each of these views when he thought the context demanded it. The effect, nonetheless, was that he did not give convergence questions anything like the significance that Cauchy was to attach to them. The situation is more complicated with Lagrange. On the one hand it was a matter of definition for him that every function had a Taylor series, the coefficients of which were his definition of the derived functions of a function. On the other hand, he frequently gave estimates of the error involved in replacing such a series by a finite, polynomial expression such as its first n terms. As Grabiner has shown, Cauchy may well have drawn his

³*Procès-verbaux des séances de l'Académie* 9 (1921), 400.

⁴For Poisson's life and work, see [Métivier et al. \(1981\)](#).

⁵The work of Euler and Lagrange dominates eighteenth-century mathematics. On Euler, the reader may start with the online Euler Archive, which gives access to all of his works as well as many commentaries. Burckhardt's *Euleriana* updated to 1983 fills up pages 511–552 of [Euler \(1983a\)](#). As for Lagrange's life and work, see [Loria \(1913\)](#), [Burzio \(1963\)](#), and [Borgato and Pepe \(1990\)](#).

⁶For a recent discussion, see [Ferraro \(2008\)](#).

ideas about analysis in terms of the $\varepsilon - \delta - N$ methodology from yoking together these two apparently different ideas in Lagrange's work.⁷ For various reasons and in various ways power series expansions were a legitimate and common device in the study of functions, albeit of a formal kind to modern eyes. They retained much of this character, for example, in the work of Jacobi.

Power series methods were commonly used in the study of differential equations when more informative solution methods failed. There was, by 1800, an impressive literature on ordinary differential equations. Linear equations had been given an (admittedly formal) theory by Lagrange, and some, like the hypergeometric equation, had been the object of more detailed study (as will be seen below). It had been well understood, for example, since Euler's work in the 1740s, that an n th order linear ordinary differential equation had a general solution depending on n arbitrary constants, or, equivalently, was a sum of n distinct solutions.⁸ What was not at all understood was the nature of the singular points; this, as we shall see with Gauss, (see Sect. 1.5.2) had to wait for a firm grounding in the theory of a complex variable.

The contribution of Abel and Jacobi was to create a theory of functions at once traditional and novel; traditional in its algebraic aspect; novel in that it was, in ways that required to be understood, truly a theory of functions of a complex variable.

As for the topic of elliptic integrals, it had a long-established place in contemporary astronomy. Since Kepler's second law asserts that an elliptical orbit is parameterised by a satellite sweeping out equal areas in equal times, mathematicians were led straight away into questions involving the rectification of an ellipse and so to elliptic integrals. Newtonian theory then said that the orbit would be an ellipse only if the problem was a two-body one. For a 3- or n -body problem, the question was to compute the additional variation of that ellipse. The details of perturbation theory had called on some of the deepest ingenuity of eighteenth-century mathematicians such as Clairaut, Euler, Lagrange, and Laplace and acquired a new urgency in the early nineteenth century with the discovery of the asteroids. Moreover, since astronomical work is highly numerical, it lent some force to the whole thrust of Legendre's work, which was the simplification of the general elliptical integral, and the subsequent computation of the values of elliptical integrals as functions of the coefficients and their upper end points.

The computation of values of elliptic integrals proved to be far from the dreary task it might seem. The analogy between the trigonometric and elliptic integrals is helpful here as elsewhere. To compute tables of, say, the sine function, one would make repeated use of the addition formulae

$$\sin(u + v) = \sin u \cos v + \sin v \cos u,$$

$$\cos(u + v) = \cos u \cos v - \sin u \sin v,$$

⁷Grabiner (1981), see also Bottazzini (1992a).

⁸Publication is another matter and seems to have begun with Euler (1750).

with their important corollaries

$$\begin{aligned}\sin 2u &= 2 \sin u \cos u, \\ \cos 2u &= \cos^2 u - \sin^2 u,\end{aligned}$$

and their consequences

$$\begin{aligned}\sin(u/2) &= ((1 - \cos u)/2)^{1/2}, \\ \cos(u/2) &= ((1 + \cos u)/2)^{1/2},\end{aligned}$$

together with known values of cosine and sine such as $\cos 0 = 1$, $\sin 0 = 0$, $\cos \pi/2 = 0$, $\sin \pi/2 = 1$. Now, in terms of integrals,

$$u = \int_0^v \frac{dt}{\sqrt{1-t^2}}$$

may be taken to define the function $v = \sin u$, and the simplest and paradigmatic elliptic integral is

$$u = \int_0^v \frac{dt}{\sqrt{1-t^4}}. \quad (1.1)$$

It measures arc-length along the lemniscate $r^2 = \cos 2\theta$, which is a curve in the shape of a figure eight.⁹ This integral can be regarded as defining either u as a function of v or v as a function of u . In either case, one is led to look for analogues of the behaviour of the trigonometric functions.

A remarkable analogy had been discovered by an Italian mathematician, Count Fagnano, in 1714 and republished by him in 1750, on the occasion of his submitting his life's work to the Berlin Académie (see [Enneper 1876](#), Note III). His [Fagnano \(1750\)](#) was sent to Euler to read, with gratifying and dramatic effect. Fagnano had showed that a certain algebraic change of variable, which he gave explicitly, established that if

$$\int_0^v \frac{dt}{\sqrt{1-t^4}} = 2 \int_0^w \frac{dt}{\sqrt{1-t^4}} \quad (1.2)$$

then v and w are algebraically related, so although arc-length along the lemniscate is a transcendental function of the parameter, values of the parameter for an arc and an arc of twice the length are algebraically related. Fagnano went on to show

⁹Differentiating the equation for the lemniscate gives $rdr = -\sin(2\theta)d\theta$. The element of length is given by $ds^2 = dr^2 + r^2d\theta^2$, and eliminating θ gives this expression for arc-length: $ds^2 = \frac{dr^2}{1-r^4}$, whence the claim. The total arc length of the lemniscate is denoted 2ω .

that dividing the lemniscatic arc into 2.2^m , 3.2^m , or 5.2^m pieces was also possible algebraically. Euler was already interested in elliptic integrals and had studied of them in their setting of elastic beams.¹⁰ On December 20, 1738 he had written to Daniel Bernoulli to say that he had come across “a singular property of the rectangular elastica”¹¹ corresponding to

$$\int_0^1 \frac{dx}{\sqrt{1-x^4}} \cdot \int_0^1 \frac{x^2 dx}{\sqrt{1-x^4}} = \frac{\pi}{4}.$$

Euler was immediately excited and generalised Fagnano’s results to obtain a general addition theorem. He interpreted them as concerning solutions to the differential equation

$$\frac{dx}{\sqrt{1-x^4}} = \frac{dy}{\sqrt{1-y^4}}, \quad (1.3)$$

and by analogy with the trigonometric case

$$\frac{dx}{\sqrt{1-x^2}} = \frac{dy}{\sqrt{1-y^2}}$$

guessed correctly that the solution of this differential equation was

$$x^2 + y^2 = c^2 + 2xy(1 + c^4)^{1/2} - c^2 x^2 y^2, \quad (1.4)$$

where c is an arbitrary constant. This fascinated him because it is an algebraic equation connecting solutions to a differential equation. It also means that

$$\int_0^x \frac{dt}{\sqrt{1-t^4}} + \int_0^y \frac{dt}{\sqrt{1-t^4}} = \int_0^c \frac{dt}{\sqrt{1-t^4}}, \quad (1.5)$$

so the result of adding together any two arcs of a lemniscate is an arc whose parameter, c , depends only algebraically on the parameter values of the original arcs. Equation (1.4) is called Euler’s algebraic addition theorem for elliptic integrals. It makes explicit what Fagnano had also noticed, that doubling the lemniscatic arc

¹⁰The connection of elliptic integrals with elastic beams went back to Jakob Bernoulli who in his study of the elastica in the 1690s was led to consider the differential $dy = \frac{x^2 dx}{\sqrt{c^4 - x^4}}$ (c constant). After some attempts at expressing y in terms of exponentials he stated he had “weighty grounds” for believing that the integral could not be expressed by means of quadratures or rectifications of any conic section.

¹¹According to Truesdell (1960, 174) Euler seemed “particularly proud” of this result, “and he comes back to it again and again, until finally it reveals itself to him as only a special case of the addition theorem for elliptic integrals”.

yields a quartic equation, whereas doubling the circular arc yields only a quadratic equation.

This could have been the crucial step towards a theory of “lemniscatic integrals”

$$u = u(v) = \int_0^v \frac{dt}{\sqrt{1-t^4}} \quad (1.6)$$

and so it was to be, but not for Euler. The reason seems to have been, as Krazer argued (1912), that on this occasion Euler remained too close to the geometry of conics and lemniscates with which he began. Nor did anyone else pick up the problem for a generation. As Enneper pointed out ([Enneper 1876](#), 542) “the writings of Fagnano seem to have been very little known, and the writings of the great Euler too little read”. The man who first studied the lemniscatic integral from a purely functional point of view was Legendre.

Like Euler, Legendre began, in his “Mémoire sur les intégrations par d’arcs d’ellipse” (1788a) with geometrical questions.¹² But in his “Mémoire sur les transcendentes elliptiques”, which he read to the Paris Académie in 1792, his attention was directed towards the new functions the integrals define, as is made clear by the title and still more by the sub-title: “Containing easy methods of comparing and valuing these transcendental, which include elliptic arches, and which frequently occur in the application of the Integral Calculus”.¹³ In this paper he showed for the first time how any integral of the form $\int \frac{Pdx}{R}$, where P is a rational function in x and R is the square root of a quartic (with real coefficients), can be simplified to one of the form

$$\int \frac{Qdt}{\sqrt{(1-t^2)(1-c^2t^2)}}. \quad (1.7)$$

The natural substitution $t = \sin \phi$ further reduces this to

$$\int \frac{Qd\phi}{\sqrt{(1-c^2\sin^2\phi)}}. \quad (1.8)$$

The variable ϕ Legendre called the amplitude of the elliptic integral, the parameter c the modulus, and quantity b defined as $\sqrt{1-c^2}$ the complementary modulus. Writing Δ or $\Delta(\phi)$ for the square root $\sqrt{(1-c^2\sin^2\phi)}$, Legendre showed how this reduction led to one of the three distinct kinds, a classification employed by

¹²As he did in [Legendre \(1788b\)](#).

¹³According to the English translator, Lacroix added a note to his *Traité* (1797–1798) to say that this interesting memoir had become very rare, and the translation was published at the request of several eminent mathematicians; see Leybourn’s *Mathematical Repository*, (2) 2 (1809, 1).

everyone since and which he was rightly to say in his *Traité* (1825–1832) 33 years later was at the basis of his method. In his terminology:

Elliptic integrals

$$\int_0^x \frac{d\phi}{\Delta} \quad (1.9)$$

are of the first kind;

elliptic integrals of the form

$$\int_0^x \Delta d\phi \quad (1.10)$$

are of the second kind; and elliptic integrals of the form

$$\int_0^x \frac{d\phi}{(1 + n^2 \sin^2 \phi) \Delta}, \quad (1.11)$$

where n may be real or complex, are of the third kind.

All the integrals were regarded as functions of their upper end point x . He wrote F for a typical integral of the first kind and E (or G) for one of the second kind. The complete integrals $\int_0^{\pi/2} \frac{d\phi}{\Delta}$ and $\int_0^{\pi/2} \Delta d\phi$ he denoted F^1 and E^1 , respectively, or $F^1(c)$ and $E^1(c)$ when he wanted to think of them as functions of the modulus c . He said it was indispensable that the modulus c and the amplitude ϕ were real, and that c be less than 1. Amongst other results, he showed that Euler's differential equation $\frac{dx}{R(x)} = \frac{dy}{R(y)}$, where R is the square root of a quartic, reduced to

$$\frac{d\phi}{\sqrt{1 - c^2 \sin^2 \phi}} = \frac{d\psi}{\sqrt{1 - c^2 \sin^2 \psi}}, \quad (1.12)$$

and that its integrals satisfy $F(\phi) + F(\psi) = F(\mu)$, where μ is an arbitrary constant. But when $\phi = 0$, $\psi = \mu$, so a comparison with Euler's result shows that

$$\cos \phi \cos \psi - \sin \phi \sin \psi \Delta(\mu) = \cos \mu. \quad (1.13)$$

This is his functional form of Euler's addition theorem (1.4) for elliptic integrals. The bulk of the paper was given over to a sketch of how tables of the values of $F(\phi)$ could be calculated for specified values of the modulus c .

That Legendre had thoroughly adopted the function-theoretic point of view is even clearer in his *Exercices de calcul intégral* (1811–1817) which begins with his study of “Des fonctions elliptiques”. He meant $F(\phi)$, $E(\phi)$, and related functions, but to avoid confusion with their inverses, which we nowadays call elliptic functions, we shall call Legendre's functions elliptic integrals. The book

is dominated by the spirit of Euler, and Legendre thought of himself as enriching mathematics as Euler had done with whole families of new, interesting functions. To this end he showed in Vol. II (1817) how they can be calculated numerically, then how the new functions can be used in geometrical and mechanical problems (see Sect. 4.6 below), and then he wrote extensively on Eulerian integrals. He devoted Vol. III (1816, sic) to the production of several sets of tables, which were also published independently, and computed tables for the Beta and Gamma functions which Euler had introduced (see Sect. 2.2 below) and shown, inter alia, to be closely connected to elliptic integrals.

The *Exercises* is a summary of his life's work on the subject, and it often builds on what he had written earlier. From his study of elliptic integrals (1792) he now deduced by differentiating under the integral sign that the complete elliptic integrals satisfy linear differential equations

$$(1 - c^2) \frac{d^2 F^1}{dc^2} + \frac{1 - 3c^2}{c} \frac{dF^1}{dc} - F^1 = 0 \quad (1.14)$$

(an equation first given by Euler in 1750)¹⁴ and

$$(1 - c^2) \frac{d^2 E^1}{dc^2} + \frac{1 - c^2}{c} \frac{dE^1}{dc} + E^1 = 0, \quad (1.15)$$

whence he obtained power series expansions for the complete integrals $F^1(c)$ and $E^1(c)$. He also established a strikingly attractive result connecting complete integrals of the first two kinds with complementary moduli (c and b):

$$\frac{\pi}{2} = F^1(c)E^1(b) + F^1(b)E^1(c) - F^1(b)F^1(c). \quad (1.16)$$

In this work he also showed (Vol. I, Sects. 17–22) how to calculate values of F by showing how to give accurate approximations when c is nearly 0 or 1, and how to reduce the general case to this one by a transformation. He had done this earlier, in his (1788a, b) and his (1792).

By analogy with the trigonometric case, he defined ϕ_n to be an amplitude such that $F(\phi_n) = nF(\phi)$ and sought to find $\sin(\phi_n)$ and $\cos(\phi_n)$ in terms of $\sin \phi$ and $\cos \phi$. He pointed out that this was easy when the modulus was 0 or 1 because then the elliptic integral can be evaluated explicitly. For example, when $c = 0$, $F(\phi) = \phi$, which implies that $\phi_n = n\phi$. When c lies between 0 and 1 the addition formula gave him expressions for $\cos(\phi_2)$, $\sin(\phi_2)$ and $\tan(\phi_2)$. Identities between $\sin(\phi_{n+1})$, $\sin(\phi_{n-1})$ and $\sin(\phi_n)$ enabled him to obtain formulae for $\sin(\phi_3)$, $\sin(\phi_4)$, and so on. The formulae struck him as unexpectedly complicated. The trisection formula, for example, relating $\sin(\phi_3) = a$ to $\sin(\phi) = x$, is

¹⁴See Euler, *O.O.* (1) 20, 40.

$$\frac{(3 - 4(1 + c^2) + 6c^2x^4 - c^4x^8)x}{1 - 6c^2x^4 + 4c^2(1 + c^2)x^4 - 3c^4x^8} = a \quad (1.17)$$

It is of degree 9 in x , not of degree 3 as a naïve analogy with trigonometry suggests. Quite generally, to divide ϕ into n parts leads to an equation of degree n^2 , although the complexity of the formula was to invite later authors to fruitful reflection.

This approach to constructing a table of values therefore seemed blocked, but Legendre showed that $\tan \frac{1}{2}(\phi_{n+1} + \phi_{n-1}) = \Delta \tan \phi_n$. Since Δ does not depend on n this formula can be iterated to give successive values of ϕ_n in terms of n . Legendre, however, preferred a second method, which altered the value of the modulus. The elliptic integral is trivial when $c = 0$, for then it reduces to a linear function. As he had observed in his first paper, (1788a), it was already traditional in evaluating arc lengths along ellipses to make a substitution that transformed a given ellipse into another, more circular, one. Indeed, as Enneper later showed, Legendre's transformation is his re-working of Landen's transformation and is very close to some ideas of Lagrange, although neither are mentioned by name in Legendre's paper.¹⁵

In his paper Legendre considered the transformation $c' = \frac{2\sqrt{c}}{1+c}$. Iterating this transformation gives a series of values $c, c', c'', \dots$ tending rapidly to the value 1. So iterating it backwards gives a series of values $c, c^o, c^{oo}, \dots$ tending towards the value 0. Explicitly,

$$c^o = \frac{1 - \sqrt{1 - c^2}}{1 + \sqrt{1 - c^2}},$$

or, in terms of the complementary modulus, $c^o, c^{oo}, \dots, c^{(n)}, \dots$ can be found from the sine tables by a quick use of the addition formulae.

Legendre applied this idea as follows. He defined ϕ^o by the equation

$$2\sin^2 \phi = 1 + c^o \sin^2 \phi^o - \Delta^o \cos \phi^o,$$

where $\Delta^o = \sqrt{1 - (c^o)^2 \sin^2 \phi^o}$, and found that $F_c(\phi) = \frac{1+c^o}{2} F^o(\phi^o)$, where

$F^o(\phi^o) = \int_0^{\phi^o} \frac{d\phi^o}{\Delta^o}$. He then deduced the useful little result that $\tan(\phi^o - \phi) = b \tan \phi$.

So evaluating F at ϕ reduces to evaluating F^o at ϕ^o , which is easier to do because c^o is less than c , and by iterating he found that

$$F_c(\phi) = \frac{(1 + c^o)(1 + c^{oo}) \dots (1 + c^{(n)})}{2^n} \cdot F^{(n)}(\phi^{(n)}).$$

¹⁵See [Enneper \(1876, 353 and 358\)](#). Landen's transformation, introduced by him in his (1775), is equivalent to this transformation of the moduli: $k_1 = \frac{1-k'}{1+k'}$.

When the value of $c^{(n)}$ was negligibly small, which Legendre observed it would be after a small number of steps, $\Delta = 1$ and $F(\phi) = \phi$, so he let $\Phi = \lim_{n \rightarrow \infty} \frac{\phi^{(n)}}{2^n}$ and found, on setting the product $(1 + c^o)(1 + c^{oo}) \dots = \alpha$ (it is a constant depending only on the choice of c , not the angle ϕ) that

$$F = (1 + c^o)(1 + c^{oo}) \dots = \alpha \Phi. \quad (1.18)$$

This meant that $F^1(c) = \frac{\pi}{2} \alpha$ and was easy to find from logarithm tables. When $\phi = \frac{\pi}{2}$ the limit $\Phi = \frac{\pi}{2}$, and the corresponding value of F is $\alpha \frac{\pi}{2}$.

Moreover, the value of c tended rather rapidly to zero, so that the convergence was quite rapid. Legendre illustrated this with an example. Starting from the value

$$c = \frac{\sqrt{2}}{2} \left(\frac{1 + \sqrt{3}}{2} \right) = \sin 75^\circ,$$

which he noted was unfavourable to calculation because c was close to 1 (it is in fact 0.9659258262), Legendre showed that four iterations were enough to show that c^{oooo} vanished to seven decimal places, and $\log \alpha = 0.2460561$. He also showed that when c is small

$$c^o = \frac{1}{4}c^2 + \left(\frac{1.3}{2.4} \right) c^4 + \left(\frac{1.3.5}{2.4.6} \right) c^6 + \dots \quad (1.19)$$

from which it followed that it was often enough to use just the first two terms. In the *Exercises* he gave a table of values of elliptic integrals to 14 decimal places.

Legendre also sought to show how useful his new functions would be in various parts of mathematics. In his (1792) he had mentioned the oscillations of a simple pendulum, which are given by the equation

$$dt = \frac{\sqrt{l} d\psi}{\sqrt{1 - c^2 \sin^2 \psi}}, \quad (1.20)$$

where l is the length of the pendulum, $c^2 = \frac{h}{2l}$ where h is the height of the pendulum due to its speed at its lowest point (in units where the acceleration due to gravity = 1) and the angle ψ is related to the angle of displacement from the vertical by the formula $\sin(\frac{\phi}{2}) = c \sin \psi$. So $t = F(\psi)$, which means that the familiar approximate equation for small arcs under-estimates the time needed to make a complete swing. Legendre was surprised to notice that he was the first to show that there were algebraic relations connecting the times of swings of a circular pendulum, just as there were for divisions of circular arcs.

In Vol. II of his *Exercises* he dwelt at length on three problems: the rotation of a solid about a fixed point; the motion either in plane or space of a body attracted to two fixed bodies; and the attraction due to an homogeneous ellipsoid. In the first volume of his *Traité* he added four more examples: motion under central forces, the surface area of oblique cones, the surface area of ellipsoids, and the problem of

determining geodesics on an ellipsoid. The result was that Legendre's final book, surely intended by him as the definitive treatment of a topic that had engaged him all his life, was a systematic presentation of new functions of a real variable that embraced their definition, their fundamental properties, and tabulated their values, while displaying their utility in solving significant problems in applied mathematics.

At the time of the publication of the *Exercices* Legendre seems to have felt his work was not getting the attention it deserved. Once Abel and Jacobi had taken it as their point of departure, its fame was assured. In the first supplement to his *Traité*, dated 12 August 1828, Legendre wrote that¹⁶

Until then geometers had taken almost no part in this kind of research, but scarcely had my work seen the light of day, scarcely could its title have become known to scientists abroad, when I learned with as much astonishment as satisfaction that two young geometers, MM. Jacobi of Königsberg and Abel of Christiania had succeeded in their own individual work in considerably improving the theory of elliptic functions at its highest points.

And Dirichlet, in his memorial address on Jacobi (see [Dirichlet 1852](#), 9), said that

It is Legendre's eternal glory to have discovered the kernel of an important branch of analysis and by the work of half a lifetime to have erected on these foundations an independent theory Only with the continued determination that enabled the great mathematician to return again and again to the subject was he able to overcome difficulties that, with the means he had at his command, must have seemed scarcely subduable.

Legendre's is firmly a real theory of functions of a real variable, and the obvious analogy Euler had pointed out between $\int \frac{dt}{\sqrt{1-t^2}}$ and $\int \frac{dt}{\sqrt{1-t^4}}$ never fruitfully suggested to Legendre that he should invert his new-found functions. Inversion in this context is to take an integral, say $y(x) = \int_0^x f(t)dt$, and to regard it not as a function, y , of its upper end point, x , but inversely, as defining a function $x = x(y)$. The familiar motivation for doing this is the trigonometric functions, as Jacobi spelled out in a paper of 1832, after the first rush of discovery was over.¹⁷ Inverting the integral $y(x) = \int_0^x \frac{dt}{\sqrt{1-t^2}}$ gives the function $x = x(y) = \sin y$. This is a much more tractable function than $y(x) = \arcsin(x)$; it is periodic, whereas the arcsin function is infinitely many-valued (and for that reason not strictly a function at all in the modern sense of the term).

As Krazer pointed out, Legendre regarded the inversion problem as essentially solved by his tables, which in a sense it is—for real functions of a real variable (see [Krazer 1909](#), 55). In view of the great successes of Abel and Jacobi, it is interesting to speculate on why Legendre never did in 40 years what they did almost at once, and there is surely much justice in Jacobi's opinion that¹⁸

¹⁶Quoted in [Ore \(1957, 210\)](#).

¹⁷See [Jacobi \(1832b, 5\)](#). Partial English translation in [Birkhoff \(1973, 207–212\)](#).

¹⁸Quoted in [Koenigsberger \(1904b, 54\)](#) and [Krazer \(1909, 55n\)](#).

The fear that the mathematician most chiefly concerned with the determination of numerical values had of the imaginary, was the reason that Legendre was prevented from taking the most important step in modern analysis, the introduction of doubly periodic functions.

We would add that in many of the applied problems that Legendre discussed the elliptic integral arises naturally, and the problem and its solutions make sense at that level. Moreover, in Legendre's time the distinction between a function and a multi-function was not sharply made and indeed, would one throw the logarithm out of mathematics in favour of its inverse the exponential function on the grounds that it is not a proper function?

The crucial breakthrough in the study of elliptic integrals proved to be the simultaneous recognition of inversion and complexification as governing ideas. As we shall see, the integral $\int_0^v \frac{dt}{\sqrt{1-t^4}} = u(v)$ is more tractable when regarded as defining not $u = u(v)$ but, inversely, $v = v(u)$, and it is seen even more clearly when v is allowed to be complex. Without this second step, the first one loses much of its force. Although Legendre knew that his tables of values for elliptic integrals solved the inversion problem, he did not appreciate the fact. Abel, Jacobi, and Gauss almost immediately took up the question as one involving complex u and v , and for them inversion burst with the full force of a revelation.

Another stimulus to the work of Abel was the cryptic hint that Gauss had dropped in his *Disquisitiones arithmeticae* (1801, Sect. 335) about the principles of his theory of cyclotomy: "Not only can they be applied to the theory of circular functions, but also to many other transcendental functions, e.g. those which depend on the integral $\int \frac{dt}{\sqrt{1-t^4}}$ ". But although Gauss promised a treatment of this and related topics, he never provided it, and it was the young Abel who, with difficulty, made it yield its secrets.

1.2 Abel

Niels Henrik Abel was born on the 5th of August 1802 on the island of Finnøy, off the southern coast of Norway, where his father was a pastor and his mother the daughter of a local shipowner, and educated at home until he was 13, when he was sent to school in Christiania (now Oslo). In his final year there his extraordinary mathematical talents were discovered when a new mathematics teacher, Bernt Michael Holmboe, himself only 22, came to the school.¹⁹ Soon Holmboe was giving Abel, not yet 17, private tuition in the works of Lagrange; not surprisingly he wrote in Abel's school report that the youth was "an excellent mathematical genius". Abel

¹⁹Abel's short life has been well told in many places, for example [Ore \(1957\)](#) and most recently [Stubhaug \(2000\)](#). Stubhaug's fascinating account is much fuller on Abel's life and times, Ore's remains more reliable on the mathematics. The richest account of his mathematics is [Houzel \(2004\)](#).

Fig. 1.2 Niels Henrik Abel
(Portrait by Gorbitz, in
Stubhaug, *Niels Henrik Abel
and his times*, Springer
©1996, p. 437)



graduated from school and went to Christiania university, where he was encouraged by Hansteen, an expert in terrestrial magnetism, to think of a career in mathematics (Fig. 1.2). When in 1823 Abel thought, erroneously, to have shown how to solve the quintic by radicals, Hansteen sent him to Copenhagen to talk to Degen, who was better qualified than anyone in Norway. Degen could not find the flaw, but his advice, to test the method on examples, soon revealed that there was one. Writing to Holmboe about the trip, Abel commented for the first time on record about elliptic integrals and on Degen's reaction to some of Abel's ideas on a work he showed him²⁰:

You remember the little paper which treated the inverse functions of the elliptic transcendents, where I proved something impossible; I asked him to read it from one end to the other; but he could not discover any false conclusion nor understand where the fault might be; God knows how I will get out of it.

Krazer (1909, 56), following many other commentators, observed that the “mistake” may only have been a surprising novelty, for example that the division equation is of order n^2 and not order n . Be that as it may, Abel's letter reveals that he had already taken one of the two crucial steps in the transformation of Legendre's work and inverted the integrals.

In 1825 Abel was funded by the university to continue his studies abroad, and he left with three friends on September 7th. They first went to Berlin, where Abel was fortunate enough to meet Privy Councillor Leopold Crelle. Forty-five years old, Crelle was an influential engineer who had built the railway from Berlin to nearby

²⁰Quoted in Ore (1957, 65).

Potsdam, but he was also an exponent of the German philosophy of neo-humanism. Thus he laid great emphasis on the study of mathematics for its own sake, believing that worthwhile applications of new mathematics would arise naturally, so that the subject need not be shackled, as had previously been the case in many parts of Germany, to a narrowly utilitarian view. His success as an engineer, allied to his skills as an organiser, made him a very powerful supporter of pure mathematics. Even more valuably for Abel he was in the process of establishing a new journal for mathematics, the first of its kind in Germany. Crelle hoped thereby to raise what he saw as the poor standards of German mathematics, and he invited Abel to contribute to it. This journal, the *Journal für die reine und angewandte Mathematik*, rapidly became the leading journal devoted to mathematics in Germany, and after Crelle's death ever more securely the journal of mathematics as Berlin saw it. Despite its title it concentrated on pure mathematics, becoming what wits called the *Journal für die reine unangewandte Mathematik*, the Journal for pure unapplied mathematics.

In Abel, Crelle gained a prolific contributor who brought fame to his journal. Through Crelle, Abel gained access to an up-to-date mathematical library. He also gained a lifelong admirer, and a translator of the papers which he began to produce at a great rate—no less than seven in the first volume of Crelle's *Journal* alone. His interest in the solvability of equations by radicals deepened. Before leaving Norway he had succeeded in showing, independently of [Ruffini \(1799\)](#), that the general polynomial equation of degree 5 is not solvable by radicals. Now he began to consider those of higher degree which nonetheless are solvable by radicals and to investigate when this occurred. He also became aware of Cauchy's critiques of divergent series in analysis and wrote his own paper on the convergence of the binomial series. The divergence of certain Fourier series struck him most forcefully.

He decided against visiting Gauss in his lair in Göttingen, perhaps on the advice of Crelle. Crelle, like everyone in Berlin, thought highly of Gauss, but regretted that everything the great man wrote was “an abomination—gruel—so obscure that it is almost impossible to understand anything” and Hansteen added that Gauss “is like the fox who covers his tracks in the snow with his tail”.²¹ Abel wrote with evident surprise to Hansteen on 5 December 1825 (in [Abel 1902](#), 11–12) that

The degree to which young mathematicians here in Berlin and, I hear, all over Germany almost worship Gauss is extraordinary. For them he is the epitome of mathematical perfection, but if he is indeed a great genius it is also certain that he writes badly.

Instead Abel went first on a holiday and then to the centre of the mathematical world, Paris, where he arrived on July 10, 1826. But he did not find the French mathematicians as approachable as the Prussians had been, indeed his best contact was with the young Lejeune Dirichlet who called on Abel under the mistaken impression that he was a fellow German. He did meet Cauchy but found him mad and impossible to deal with, as he wrote to Holmboe (24 October) adding that “He is

²¹ The quotes from Crelle and Hansteen are given in [Bjerknes \(1885, 92\)](#).

extremely Catholic and bigoted, which is strange for a mathematician". On the other hand he was the only person actively engaged in pure mathematics, everyone else being engaged in magnetism and other kinds of physical theories, while Legendre, then 74, was "as old as stone".²²

In December Abel took up the lemniscate, whose arc length is given by the simplest of elliptic integrals: $s = \int_0^x \frac{dt}{\sqrt{1-t^4}}$. He wrote to Crelle that he had found that the lemniscatic arc can be divided by ruler and compass into n equal arcs for exactly the same values of n as the circular arc, as Gauss had hinted in the *Disquisitiones arithmeticae*. To Holmboe he said that he now saw as clear as day how Gauss had come to discover his results.²³

In January 1827 he was back in Berlin, which he preferred to Paris. It was in this year that his first major paper on elliptic integrals appeared in Crelle's *Journal*, and it will be worthwhile considering it in some detail, for it presents the first account of the theory of elliptic functions. The paper, his *Recherches sur les fonctions elliptiques* (Abel, 1828a), amply displays Abel's lucid style of exposition, fostered under the firm guidance of Crelle. It also indicates the benefits that flowed from taking the second step and allowing the variables to have complex values. In it Abel developed the theory of elliptic functions in close analogy with the theory of trigonometric functions. He introduced three functions (replacing, as it were, sine and cosine) and derived addition laws for them and investigated their division formulae. There he presented a thorough analysis of when the corresponding equations are solvable by radicals and proved Gauss's claims about the lemniscate. He concluded with finding expressions for the new functions as infinite series and infinite products.

Abel began his paper by summarising what Legendre had shown in his *Exercices*. Abel took the general elliptic integral of the first kind in a slightly different form:

$$\alpha = \int_0^x \frac{dt}{\sqrt{(1-c^2t^2)(1+e^2t^2)}}. \quad (1.21)$$

The inverse function $x = \phi(\alpha)$ therefore satisfies

$$\frac{d\phi}{d\alpha} = \sqrt{(1-c^2x^2)(1+e^2x^2)}.$$

Abel introduced the useful abbreviations

$$f(\alpha) = \sqrt{1-c^2\phi^2(\alpha)}, \quad F(\alpha) = \sqrt{1+e^2\phi^2(\alpha)}. \quad (1.22)$$

²²All quotations from Abel (1902, 45–46); see also Ore (1957, 147) and Stubhaug (2000, 405).

²³See Abel (1881, 2, 261), and Ore (1957, 154).

He then remarked that these functions have some interesting properties, for example, each of the equations $\phi(\alpha) = 0$, $f(\alpha) = 0$, $F(\alpha) = 0$ has infinitely many roots which can be determined, and for any integer m , $\phi(m\alpha)$ can easily be found in terms of $\phi(\alpha)$, etc. But the converse problem is harder: to express $\phi(\alpha)$, $f(\alpha)$, or $F(\alpha)$ in terms of $\phi(m\alpha)$ requires solving an equation of degree m^2 , the study of which is the principal point of the memoir. The analogy with the trigonometric functions is evident.

He defined

$$\frac{\omega}{2} = \int_0^{1/c} \frac{dt}{\sqrt{(1-c^2t^2)(1+e^2t^2)}} \quad (1.23)$$

and observed that ϕ is positive in the range $0 < \alpha < \frac{\omega}{2}$, and that $\phi(0) = 0$ and $\phi(\frac{\omega}{2}) = \frac{1}{c}$. Moreover, ϕ is an odd function, $\phi(-\alpha) = -\phi(\alpha)$. If α is replaced by $i\beta$, so $ix = \phi(i\beta)$ then $\beta = \int_0^x \frac{dt}{\sqrt{(1+c^2t^2)(1-e^2t^2)}}$ is real and positive between $x = 0$ and $x = 1/e$.²⁴ So inversely, defining

$$\frac{\tilde{\omega}}{2} = \int_0^{1/e} \frac{dt}{\sqrt{(1+c^2t^2)(1-e^2t^2)}}, \quad (1.24)$$

x is positive in the range $0 < \beta < \frac{\tilde{\omega}}{2}$. It is clear that $\phi(i\alpha) = i\phi(\alpha)$, and $F(i\alpha) = f(\alpha)$, and also that $\phi(\frac{i\tilde{\omega}}{2}) = \frac{i}{e}$. Now that many values of the functions ϕ, f, F are known along the real and imaginary axes, it remained to find their general values. Abel showed that

$$\begin{aligned} \phi(\alpha + \beta) &= \frac{\phi(\alpha)f(\beta)F(\beta) + \phi(\beta)f(\alpha)F(\alpha)}{R}, \\ f(\alpha + \beta) &= \frac{f(\alpha)f(\beta) - c^2\phi(\alpha)\phi(\beta)F(\alpha)F(\beta)}{R}, \\ F(\alpha + \beta) &= \frac{F(\alpha)F(\beta) + e^2\phi(\alpha)\phi(\beta)f(\alpha)f(\beta)}{R}, \end{aligned}$$

²⁴For the sake of convenience here, and everywhere in this book, we write i instead of $\sqrt{-1}$ even though $\sqrt{-1}$ was commonly used by mathematicians until the late 1840s. Abel himself wrote “where i to abbreviate, represents the imaginary quantity $\sqrt{-1}$ ”. In this he followed Gauss’s usage in the *Disquisitiones arithmeticae* of 1801. The first to use i to denote the imaginary unit was Euler in his (1777d), presented to the St. Petersburg Academy on May 5, 1777 but published only in 1794 as Supplement IV to Vol. 1, Chap. V of his *Institutiones calculi integralis*. There (p. 184) he stated: “In the following I will designate the formula $\sqrt{-1}$ by the letter i such that it will be $ii = -1$ and $-1/i = i$ as well.” It is likely that Gauss was prompted by this to make a systematic use of i .

where $R = 1 + c^2 e^2 \phi^2(\alpha) \phi^2(\beta)$, observing that these results followed from those in Legendre's *Exercices*, but also that they could be proved directly, as follows. Letting r denote the right-hand side of the first equation, he calculated $\frac{dr}{d\alpha}$ and observed that this expression was symmetric in α and β so $\frac{dr}{d\alpha} = \frac{dr}{d\beta}$ whence r is a function of $\alpha + \beta$ which can be found by setting $\beta = 0$, when $\phi(\alpha)$ is obtained, so $r(\alpha + \beta) = \phi(\alpha + \beta)$.

As Abel then said, a slew of other formulae then follow from these addition formulae, formulae for $\phi(\alpha + \beta) + \phi(\alpha - \beta)$, for $f(\alpha + \beta) + f(\alpha - \beta)$, for $\phi(\alpha \pm \frac{\omega}{2})$, and so forth. From

$$\phi(\alpha + \frac{\omega}{2}) = \phi(-\alpha + \frac{-\omega}{2}) \text{ and } \phi(\alpha + \frac{i\tilde{\omega}}{2}) = \phi(-\alpha + \frac{i\tilde{\omega}}{2}),$$

Abel deduced (in Sect. 4) that, as he put it,

$$\phi\left(\frac{\omega}{2} + \frac{i\tilde{\omega}}{2}\right) = f\left(\frac{\omega}{2} + \frac{i\tilde{\omega}}{2}\right) = F\left(\frac{\omega}{2} + \frac{i\tilde{\omega}}{2}\right) = \frac{1}{0}. \quad (1.25)$$

In due course this became the recognition that these functions have poles at $\frac{\omega}{2} + \frac{i\tilde{\omega}}{2}$. The formulae at once imply the functions are periodic:

$$\phi(\alpha + \omega) = -\phi(\alpha), \quad \phi(\alpha + 2\omega) = \phi(\alpha), \quad (1.26)$$

$$\phi(\alpha + i\tilde{\omega}) = -\phi(\alpha), \quad \phi(\alpha + 2i\tilde{\omega}) = \phi(\alpha), \quad (1.27)$$

with similar formulae for f and F , and Abel observed (Sect. 5)

The formulae that have been established make clear that one will have all the values of the functions $\phi(\alpha)$, $f(\alpha)$, $F(\alpha)$ for all real or imaginary values of the variable if one knows them for the real values of this quantity lying between $\frac{\omega}{2}$ and $\frac{-\omega}{2}$ and for the imaginary values of the form $i\beta$, where β lies between $\frac{-\tilde{\omega}}{2}$ and $\frac{\tilde{\omega}}{2}$.

One says that the functions are doubly periodic, because they have two distinct periods, 2ω and $2i\tilde{\omega}$. To find the zeros of $\phi(\alpha + i\beta)$, therefore, it is enough to look in the restricted range $\frac{-\omega}{2} \leq \alpha < \frac{\omega}{2}$, $\frac{-\tilde{\omega}}{2} \leq \beta < \frac{\tilde{\omega}}{2}$. By the addition formula, the equation $\phi(\alpha + i\beta) = 0$ reduces to two equations on separating out real and imaginary parts:

$$\phi(\alpha)f(i\beta)F(i\beta) = 0, \quad \phi(i\beta)f(\alpha)F(\alpha) = 0.$$

These equations in turn yield $\alpha = m\omega$, $\beta = n\tilde{\omega}$ (or, if you pursue $f(i\beta)F(i\beta) = 0$, $\alpha = (m + \frac{1}{2})\omega$, $\beta = (n + \frac{1}{2})\tilde{\omega}$, values for which ϕ is infinite). So the zeros of $\phi(x)$ are $x = m\omega + ni\tilde{\omega}$. A similar calculation enabled Abel to solve the equation $\phi(x) = \frac{1}{0}$ and find $x = (m + \frac{1}{2})\omega + (n + \frac{1}{2})i\tilde{\omega}$, and to solve the equation $\phi(x) = \phi(a)$ in terms of a and find $x = (-1)^{m+n}a + m\omega + ni\tilde{\omega}$. Although Abel did not use the phrase, this amounts to saying that the values of an elliptic function are known everywhere once they are known on a period parallelogram.

Nothing so far can be called difficult, indeed it was all made to look rather easy. Nor was it much more work for Abel to express $\phi(m\alpha)$ as a rational function of

$\phi(\alpha)$, $f(\alpha)$, and $F(\alpha)$, the analogy with the trigonometric functions was still a good guide. Abel set $\phi(n\alpha) = \frac{P_n}{Q_n}$ and found recurrence relations for P_n and Q_n , and dealt similarly with $f(n\alpha)$, and $F(n\alpha)$. It was with the converse question—express, say, $\phi(\alpha/m)$ as a function of $\phi(\alpha)$, $f(\alpha)$, and $F(\alpha)$ —that the research became hard, and it is in this connection that Abel did some of his finest work on the theory of equations, which was to have considerable implications for the future development of Galois theory.

To see why the solutions are so numerous leads us directly to the importance of introducing complex variables. Recall that to find $\sin(u/3)$ given $\sin u$ requires solving a cubic equation, for, from the equation $\sin u = 3 \sin(u/3) - 4 \sin^3(u/3)$ we deduce that $\sin(u/3)$ satisfies the cubic equation $4x^3 - 3x + \sin u = 0$. We also know that the angles $(u/3)$, $(u+2\pi)/3$ and $(u+4\pi)/3$ are such that their sines satisfy this equation. However, in the elliptic case, the division equation that gives $\phi(u/3)$ in terms of $\phi(u)$ is of degree 9. The key realisation is that in the elliptic case some of the roots are complex, whereas as in the trigonometric case they are all real. Once the elliptic function is treated as a function of a complex variable, the values of u that satisfy the equation for $\phi(u/3)$ will all clearly be of the form $u_0 + (m\omega + \bar{m}i\bar{\omega})/3$, and so there are 9 of them. This makes it clear why the number of solutions is unexpectedly large.²⁵

Abel next picked up the hint dropped by Gauss about the lemniscatic functions and investigated the equations that arise to see when, for example, they are solvable by radicals. When $m = 2$ it was easy for Abel to show (Sect. 13) that

$$\phi\left(\frac{\alpha}{2}\right) = \frac{1}{c} \sqrt{\frac{1-f(\alpha)}{1+F(\alpha)}}$$

with similar formulae for $f\left(\frac{\alpha}{2}\right)$ and $F\left(\frac{\alpha}{2}\right)$. It follows that if the values $\phi(\alpha)$, $f(\alpha)$, and $F(\alpha)$ are constructible, so are the values for $\phi\left(\frac{\alpha}{2}\right)$ and $\phi\left(\frac{\alpha}{2^n}\right)$, and similarly the f 's and F 's. Repeated bisection followed by iteration.

Abel then turned to the study of the equations

$$\phi(\alpha) = \frac{P_p}{Q_p},$$

and the corresponding equations for the functions f and F —the determination of the functions $\phi\left(\frac{\alpha}{p}\right)$, $f\left(\frac{\alpha}{p}\right)$, and $F\left(\frac{\alpha}{p}\right)$ as a functions of $\phi(\alpha)$, $f(\alpha)$, and $F(\alpha)$ when $p = 2n + 1$ is an odd prime, as he put it in Sect. 12. These equations are all of degree p^2 , and Abel showed that they are all solvable algebraically (that is, by radicals). The structure of his argument reveals how deeply he had penetrated the theory of

²⁵The actual number is n^2 if n is odd and $2n^2$ if n is even. The solutions are $\phi\left(\frac{u}{n} + 2\frac{m\omega + \mu\bar{\omega}i}{n}\right)$ when n is odd and $\phi\left((-1)^{m+\mu}\frac{u}{n} + 2\frac{m\omega + \mu\bar{\omega}i}{n}\right)$ when n is even, $0 \leq m, \mu < n$. See [Houzel \(1978, 24\)](#).

the solution of equations; only Gauss had gone as far, and he had for once published his ideas, in the *Disquisitiones arithmeticae*. There Gauss had made a deep analysis of the cyclotomic equation, which has the property that all of its complex roots are known once a primitive root is found. In the present setting also, once one root is known so are all the others—if $\phi\left(\frac{\alpha}{p}\right)$ is a root of the equation $\phi(\alpha) = c$, then so are the $p^2 - 1$ quantities $\phi\left(\alpha + \frac{m\omega + \tilde{m}\bar{\omega}}{p}\right)$. So in principle a great deal is known about the polynomial equation $P_p = \phi(\alpha)Q_p$.

After a lengthy argument, which we omit (Sects. 14–16), Abel concluded that $\phi(\alpha/p)$ was an algebraic function of $\phi(\alpha)$ and added that the same was true for $f\left(\frac{\alpha}{p}\right)$ as a function of $f(\alpha)$ and for $F\left(\frac{\alpha}{p}\right)$ as a function of $F(\alpha)$. Moreover the values $\phi(\alpha/p)$ could be written in terms of radicals involving the values of $\phi(\alpha)$ and the values of φ at the points $\frac{2\omega}{p}$ and $\frac{2\bar{\omega}}{p}$, so the equation was even solvable by radicals once those numbers have been adjoined. They are found by solving the equation $P_p(\alpha) = 0$, whose roots are $x = \phi\left(\frac{m\omega + \tilde{m}\bar{\omega}}{p}\right)$, $-n \leq m, \tilde{m} \leq n$. But $m = \tilde{m} = 0$ gives the uninteresting solution $x = 0$, so one has $P_{2n+1} = xR$, and $R = 0$ is an equation of degree $p^2 - 1 = (p+1)(p-1)$. Abel found it helpful to study this equation after making the substitution $x^2 = r$, when it is of degree $(2n+2)n$ in r , and he showed that its solution reduced to two equations, one of degree $2n+2$ and the other of degree n . Indeed by considering their coefficients, he showed that the roots of $R = 0$ could be found by solving one equation of degree $2n+2$ and then $2n+2$ equations of degree n . Moreover, these equations of degree n were solvable algebraically, while the equation of degree $2n+2$ was not generally so, although it could be in special cases. In this context he was happy to state as a particular case of his findings (Sect. 22):

One can divide the entire circumference of the lemniscate into m equal parts *by ruler and compass alone* if m is of the form 2^n or a prime of the form $2^n + 1$, or if m is a product of numbers of these two kinds. This theorem, as one sees, is exactly the same as the theorem of Gauss for the circle. (Italics Abel's.)

Abel concluded the first part of his memoir with expressions for $\phi(\alpha)$, $f(\alpha)$ and $F(\alpha)$ as infinite series and infinite products. Heuristically he derived them from expressions for $\phi\left(\frac{\alpha}{2n+1}\right)$ by letting n become infinite, when, as he put it, $\phi\left(\frac{\alpha}{2n+1}\right)$ disappears and one obtains for $\phi(\alpha)$ an algebraic expression with infinitely many terms. This kind of argument vividly recalls Euler's treatment of the trigonometric functions in his *Introductio in analysin infinitorum*, I, (1748a). However, Abel attempted a more rigorous limiting argument, but in the absence of any concept of uniform convergence it could not be said to be successful. Houzel (1978, 38) remarks at this point that Abel's treatment of the convergence questions is delicate. It is certainly very thorough, but we do not think it is convincing. The upshot is a rather intimidating expression for $\phi(\alpha)$, $f(\alpha)$, and $F(\alpha)$ that leave no doubt of Abel's immense technical facility with the traditional style of analysing functions.

Abel's ideas about complex variables were entirely formal, but that said he presented a fully developed theory of elliptic functions as complex functions, analogous to the trigonometric functions. We shall now see that Jacobi's way in to the subject was significantly different, and closer to Legendre's.

1.3 Jacobi

Jacobi was born into a banker's family in Potsdam near Berlin on 10 December 1804 and displayed his prodigious gifts for mathematics and languages from an early age (Fig 1.3). He entered the local gymnasium before he was 12 and within 6 months rose to the top class, where he had to remain for 4 years because he was not allowed to enter the university until he was 16. It was at school that he first read Euler. When he went to the University of Berlin he had to choose between mathematics and philology, then enjoying its golden age in Germany. He chose mathematics, but not the mostly elementary lectures then on offer, rather the heady diet of Euler, Lagrange and Laplace. By 1825 he had submitted his doctoral thesis and was given permission to begin his Habilitation, the qualification that gave anyone who possessed it the right to teach in any German university (although not necessarily to be paid). He also became a Christian at this time, a high point of the assimilationist trend among Germans of Jewish descent, which enabled him to become a Privatdozent or instructor at Berlin University. He was not yet 21. It is worth pointing out that in 1801, before the University of Berlin was founded, the city was little more than a garrison town, its middle class largely Jewish but subject to many legal restrictions and extra taxes. Foreign Jews had to enter the city by the Rosenthaler gate, which was otherwise used for cattle.²⁶

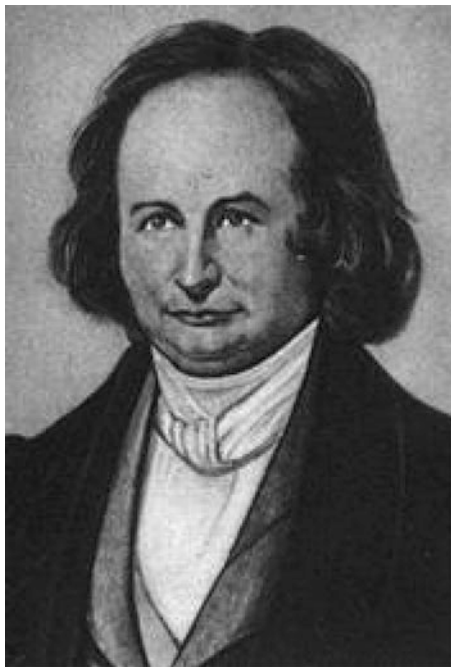
In May 1826 Jacobi transferred to the University at Königsberg, where he joined a small but strong science faculty consisting of Franz Neumann and Heinrich Dove in physics and the astronomer Friedrich Bessel. He became an associate professor there at the end of 1827. Inspired by his colleagues, his first publications are in applied mathematics, and he remained interested in the subject all his life. Ironically, success came easily to him with almost every thing he touched except the topic that made his name internationally and which interests us most: elliptic functions. His friend in later life, Dirichlet, tells this story in his memorial address of Jacobi²⁷:

One of his friends who noticed him in a bad mood one day, received this answer when he asked why he was out of sorts: You see me on the point of returning this book (Legendre's *Exercises*) to the library, with which I've been exceedingly unlucky. On other occasions when I have studied an important work this has stimulated me to some thoughts of my own and there has always been something in it for me. This time I have come away quite empty handed and have not been inspired in the least.

²⁶Information taken from [Elon \(1999, 20\)](#). See also [Elon \(2002\)](#).

²⁷[Dirichlet \(1852, 7\)](#).

Fig. 1.3 Carl Gustav Jacob Jacobi (From his *Gesammelte Werke*)



Later, but richer, was Dirichlet's comment,

By June 1827 Jacobi had indeed come to some new ideas of his own. Specifically, he had found new ways to transform one elliptic integral into another by rational changes of the variable. He was the first to discover the existence of transformations of every degree. Not having seen Legendre's *Traité* he did not know that Legendre had found a transformation of order 3. He sent a report of his discoveries to Schumacher for publication in his *Astronomische Nachrichten*, where they appeared in the September issue.

In his two notes Jacobi considered the transformation of elliptic functions, which he approached via the equation

$$\int_0^{\phi} \frac{dt}{\sqrt{1-c^2 \sin^2 t}} = m \int_0^{\psi} \frac{dt}{\sqrt{1-k^2 \sin^2 t}}. \quad (1.28)$$

Given an elliptic integral in terms of ϕ , it is required to transform it into another involving ψ . Each such transformation determines the values of the new modulus, k , and the number m . Jacobi observed that one can find such transformations by writing $\sin \phi$ as a rational function of $\sin \psi$ thus: $\sin \phi = \frac{U}{V}$, where U is a polynomial in odd powers of $\sin \psi$ up to the m th, and V is a polynomial in even powers of $\sin \psi$ up to the $(m-1)$ th. Such a transformation is said to be of order m . However, at

this stage Jacobi only knew how to find the polynomials U and V when $n = 3$ or 5 ; the rest of his claim was strictly speaking only a conjecture. He also pointed out that if one takes two or more transformations one can obtain solutions to the multiplication problem of order a composite number n relating two or more elliptic integrals:

$$\int_0^\phi \frac{dt}{\sqrt{1-c^2 \sin^2 t}} = n \int_0^\psi \frac{dt}{\sqrt{1-c^2 \sin^2 t}}.$$

In his second paper he showed how his new transformations, like Legendre's, could assist in calculating tables of values for the integrals.

These papers are rather small by comparison with Abel's and very much in Legendre's spirit. However, in the December edition of Schumacher's journal he broke new ground by proving his general transformation formula and inverting the elliptic integral. Here he argued that the substitution $y = \frac{U}{V}$ will transform

$$\frac{dy}{\sqrt{(1-a_0y)(1-a_1y)(1-a_2y)(1-a_3y)}} = \frac{dy}{\sqrt{f_4(y)}}$$

into

$$\frac{dy}{M\sqrt{(1-b_0y)(1-b_1y)(1-b_2y)(1-b_3y)}} = \frac{dy}{\sqrt{g_4(y)}},$$

where, if

$$T^2 = V^4 \frac{f_4(\frac{U}{V})}{g_4(x)}, \text{ then } \frac{T}{M} = V \frac{dU}{dx} - U \frac{dV}{dx}$$

and M is to be a constant.

So to transform $\frac{dy}{\sqrt{(1-y^2)(1-\lambda^2 y^2)}}$ into $\frac{dx}{M\sqrt{(1-x^2)(1-k^2 x^2)}}$ he denoted the integral

$\int_0^\phi \frac{dt}{\sqrt{1-k^2 \sin^2 t}}$ by $F(\phi)$ and defined

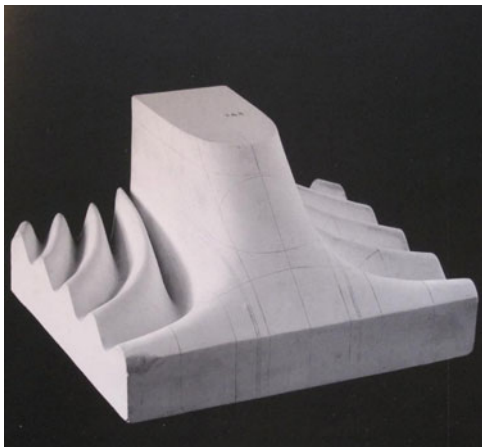
$$K = \int_0^{\pi/2} \frac{dt}{\sqrt{(1-k^2 \sin^2 t)}}.$$

Observing that if $F(\phi) + F(\psi) = F(\sigma)$, which is the expression given by Legendre (see (1.12) above) and writing $F(\phi) = \xi$, Jacobi said

$$F(\phi) = \int_0^x \frac{dt}{\sqrt{(1-t^2)(1-k^2 t^2)}}$$

and $x = \sin \alpha \xi$ (Fig. 1.4).

Fig. 1.4 The real part of Jacobi's amplitude function (Fischer, *Mathematische Modelle – Mathematical Models*, Vieweg ©1986, number 132)



This is, of course, the crucial step of inverting the elliptic integral.²⁸ Formulae like the one for σ in terms of ϕ and ψ can be reinterpreted so as to apply to $x = \sin \alpha \xi$, and pursuing this insight led Jacobi to his formulae first for V in terms of x then U in terms of x and finally for M . The coefficients of U and V and the expression for M are full of terms like $\sin \alpha \frac{2K}{2n+1}$ and $\sin \alpha \frac{2nK}{2n+1}$, so it is quite evident that without inversion Jacobi could never have proved his general claim. When, then, did he discover it?

In this connection the famous correspondence between Legendre and Jacobi is most informative. It was begun when Jacobi wrote to Legendre on 5th August 1827 to inform him of the discoveries which Schumacher was shortly to publish. The letter is rather odd in tone: Jacobi was trying to be modest—which never came easily to him, he was a notoriously irascible person—yet he knew he had something to say. He described his work on elliptic integrals, mentioned in passing that Gauss had discovered a transformation of order 7, and ended with some remarks about cubic and biquadratic reciprocity in number theory (see Sect. 4.7.2).

Legendre replied on 30 November 1827, claiming that Jacobi's letter had been delayed and only just reached him. He welcomed the transformation of order 5 although he recognised the one of order 3 as his own. However, he could not find polynomials U and V for the 7th order transformation, and rather doubted that they could exist. Jacobi had not published a proof, and he urged him to do so. But the remarks about Gauss could not pass unnoticed: without proof, Gauss had claimed the law of quadratic reciprocity as his when he, Legendre, had published it in 1785, and Gauss had then gone on to try and poach the method of least squares. So Jacobi should not worry that Gauss claimed to have discovered Jacobi's ideas in 1808, such excessive impudence was unbelievable in a man of Gauss's abilities.

²⁸Jacobi wrote the new function as a composite: the sine of the amplitude of ξ . As the function became more widely used its name contracted to *sinam* and eventually to *sn*.

Historians observe that Gauss only claimed, correctly, to be the first to prove the law of quadratic reciprocity, attributing its discovery to Legendre, and priority of discovery for the method of least squares is indeed due to Gauss, although Legendre published first. It must have been difficult to endure the lofty utterances coming from Göttingen, and Legendre had had more than his share. Legendre did more than write privately to Jacobi, he communicated his results to the Académie in the warmest terms on 5 November 1827, and his report was published in the *Globe* on the 29th.

Jacobi was delighted with the much older man's response and took the opportunity of his reply on 12 January 1828 to acquaint Legendre with the content of Abel's *Recherches*, the first part of which was now in print. He put Abel's work into his own notation, but otherwise summarised it much as we have above, dwelling on the striking division of the lemniscate. He then pointed out that the new edition of Schumacher's journal carried a proof of the existence of the general transformation and started to discuss modular equations (which we will define below, see p. 47). In Legendre's reply, 9 February 1828, Legendre explained that he had already seen Abel's paper, although he was happy to have had it analysed in a language closer to his own. In what has become a famous passage he went on²⁹:

It is a great satisfaction to me to see two young mathematicians, like you and him, cultivate successfully a branch of analysis which has for so long been my favourite object of study and which has not attracted the attention in my country that it deserves.

In his reply to this letter (12 April 1828) Jacobi retraced his route to his original discovery. This makes it clear that he had had the idea of inverting the integrals before he wrote his second letter to Schumacher, in August 1827. This seemingly establishes his independence from Abel, by just over a month. But the case is not convincing, although Krazer accepted it (see [Krazer 1909](#), 59). Jacobi first admitted that his published proofs were different from his discovery method and then made his first study of the transformations hinge on the substitution $\sin \alpha \frac{2K}{3}$. Other views are possible. Bjerknes in his biography of Abel ([Bjerknes 1885](#), 83–85), was the first to raise the idea that Abel's work might have provided the necessary inspiration for Jacobi, which he later covered up. This was not accepted by Sophus Lie, who was working on the second edition of Abel's collected works at the time, and was the cause of some friction between the two (see [Stubhaug 2002](#), 284). But Ore argues convincingly (1957, 180–190) that Jacobi could well have seen Abel's *Recherches* at a time when he was stuck for a general proof of his first important insight. His application was in for a professorship at Königsberg but not yet decided, Legendre was putting him under pressure to provide a proof, and suddenly another man opens up the whole subject. How natural to use his ideas, and how smoothly, we add, does Jacobi flow from describing Abel's work to solving his own problem. Jacobi is usually generous to Abel in letters but avoids mentioning his name in print. Ore is properly cautious and we incline to agree with him that the idea of inversion “in

²⁹See [Legendre and Jacobi \(1875, 224\)](#).

all probability was revealed to him through reading Abel's *Recherches*" (Ore, 1957, 184). What Jacobi did with the idea in any case exonerates him from what small weakness not acknowledging published work may comprise.

Now that both men were in print, it soon became evident that an exciting development in mathematics was taking place. Abel, who must have thought he had this subject to himself, was at first quite alarmed. We have this account of his reaction in a letter from Hansteen to Schumacher that Schumacher quoted to Gauss³⁰:

Abel sends herewith an article about elliptic transcendents, which he asked to have printed as soon as possible, since Jacobi is on his heels. The other day, when I handed him the last number of the *Astronomische Nachrichten*, he became quite pale and was compelled to run to the confectioner's shop and take a schnapps of bitter to counteract his alteration. For several years he has been in possession of a general method which he communicates in this paper, and which includes more than Jacobi's theorems.

As Ore pointed out, Abel dealt with the sudden appearance of Jacobi by switching his attention to Schumacher's *Astronomische Nachrichten*, and contributing to it an article on the transformation of elliptic functions but one written from a vastly more novel standpoint. "My knockout of Jacobi" he called it in a letter to Holmboe on 29 July, 1828,³¹ also calling his (1828b) the "Death-ification of Jacobi" (Stubhaug 2000, 454). Only one participant was not impressed: Gauss replied to Schumacher (23 May 1828) who told Crelle who in turn told Abel that "he (Abel) has come about one third of the way that I have gone in my researches, with the same aim and even with the same choice of notation."³² We shall consider in Sect. 1.5 the extent to which this unsympathetic verdict is correct, but both men were soon to surpass him and Gauss still did nothing to encourage them.

Evidently, 1828 and 1829 were to be hectic years in the development of elliptic functions. The contemporary response was affected by the context in which all the new work appeared, which had been set by Legendre's theory of elliptic integrals as real functions of their upper end point, x , together with a study of the complete integral K as a function of the modulus, k . The use of transformations which relate elliptic integrals to different moduli appeared in this theory as a means to computing tables of values of the integrals. Questions of multiplication or division of elliptic integrals (given x_0 find x such that $\int_0^x = m \int_0^{x_0}$ for specific rational m) had also been studied by various writers, chiefly because of their geometric interpretation and the curious fact that their solutions often displayed a surprising algebraic, rather than transcendental, dependence of x on x_0 .³³ Finally, elliptic integrals arose naturally in astronomy because planets and asteroids traverse ellipses, so there was a working

³⁰Quoted in Ore (1957, 189).

³¹See Abel (1902, 68). Engl. trl. in Ore (1957, 190).

³²Gauss to Bessel, 30 March 1828, in Gauss (1880, nr. 63).

³³The work of Landen and Lagrange is prominent in this connection. There was also a long tradition of work on the rectification of ellipse. See Enneper (1876, Sect. 44) and Houzel (1978).

theory of approximations to elliptic integrals there, which indeed was the occasion of Gauss's only publication on the subject.³⁴

Abel's *Recherches* presented a reformulation of much of this material in terms of the complex valued inverse function $x = x(y)$ of the complex variable y . The virtue of this theory was that this inverse function is easier to understand. In particular, the multiplication and division theory of elliptic functions, illuminated by the observation of double periodicity, led to a rich family of polynomial equations many of which were unexpectedly solvable by radicals. Even the more complicated and more general theory of transformations of elliptic functions was illuminated in this way, not only by the discovery of infinitely many unexpected transformations, but by the hint of conceptual order that came with their discovery. The implications of this for applied mathematics were less surely evident. Abel had not discussed them, and Legendre's examples often required the integral, not its inverse function.

In all of this the question of transformations is central. We have seen examples in Jacobi's work. Abel, in the later parts of his *Recherches* took up the theme, in a way that provoked Enneper (1876, 292) to the comment that Abel here showed "a depth of insight, and a richness of surprising ideas which alone would suffice to keep the memory of his wonderful genius alive in the history of mathematics". Independently of Jacobi, Abel introduced (Sect. 41) an infinite number of transformations:

$$\int_0^y \frac{dt}{\sqrt{(1-c_1^2 t^2)(1+e_1^2 t^2)}} = \pm a \int_0^x \frac{dt}{\sqrt{(1-c^2 t^2)(1+e^2 t^2)}}, \quad (1.29)$$

where the new moduli, c_1 and e_1 , and the number a depended in an explicit way on x and the quantity

$$\alpha = \frac{(m + \tilde{m})\omega + (m - \tilde{m})i\tilde{\omega}}{2n + 1}$$

at least one of m or $\tilde{m}$ being prime to $2n + 1$. He also gave the formulae for c_1 , e_1 , and x ; they involve his elliptic function ϕ . So Abel used the double periodicity of the elliptic functions to obtain his transformations, and found, as he put it (Sect. 45) "a certain number of transformations corresponding to each value of $2n + 1$ ". Indeed he claimed (Sect. 49) that his transformations combined with Legendre's led to all possible transformations.

A special case of great interest observed by Abel concerned transformations leaving the modulus unaltered and the integral multiplied by a complex number, a :

$$\int_0^y \frac{dt}{\sqrt{(1-t^2)(1+\mu^2 t^2)}} = a \int_0^x \frac{dt}{\sqrt{(1-t^2)(1+\mu^2 t^2)}}. \quad (1.30)$$

³⁴See Gauss (1818), discussed in Geppert (1927).

On such occasions the transformation is called complex multiplication by a .³⁵ Abel claimed (Sect. 50) that when a was rational or of the form $m \pm i\sqrt{n}$, m and n rational, then the equation above had algebraic solutions, but in the second case the modulus μ could only have certain special values that satisfied an equation with infinitely many real or imaginary roots. These became known as the singular moduli and Abel conjectured that they were the roots of polynomials which were solvable by radicals, a conjecture first proved in Kronecker (1857, see Houzel 2007). Since it will appear (see the Appendix below) that for the elliptic functions x and y that occur in (1.30) to be algebraically related it is necessary and sufficient that the lattice of periods be mapped onto itself, Abel's problem is precisely captured by the theory of complex multiplication, and the condition on a has been explained. The fact that the lattice is special underlies Abel's claim about the corresponding modulus.³⁶

Abel's presentation was a significant step in the direction mapped out by Euler and Lagrange; algebraic solutions to differential equations (1.30) were now shown to exist for some irrational a 's but only for a precisely delineated class. Abel gave explicit solutions in simple cases: $a = \sqrt{-3}$, when $e = \sqrt{3} + 2$, and $a = \sqrt{-5}$ and e satisfies a cubic equation; in each case y is a rational function of x . Finally, when $a = i\sqrt{2n+1}$, he found explicitly that e was given by an infinite series (Sect. 52):

$$e = \frac{4\pi}{\omega} \left(\frac{h}{h^2+1} + \frac{h^3}{h^6+1} + \cdots \right), \quad (1.31)$$

where $h = \exp\left(\frac{\pi}{2\sqrt{2n+1}}\right)$. The proofs of his claims about these differential equations were given in his papers (1828b and Addition) in Schumacher's journal, which is the "knockout of Jacobi" we referred to earlier. Abel ended his *Recherches* with a note comparing his results with Jacobi's, noting that their results were in agreement but that his came with proofs on occasions when Jacobi's did not, and that there were times when his proofs were simpler than those Jacobi had provided.

The Jacobi–Legendre correspondence shows that Jacobi at once began to use Abel's ideas in his own study of transformations, admitting to Legendre (12 April 1828) that he could not do without Abel's analysis. Here appear for the first time several of the striking infinite series that Jacobi was so adept at handling, of which the most remarkable in his opinion was

$$\sqrt{\frac{2K}{\pi}} = 1 + 2q + 2q^4 + 2q^9 + 2q^{16} + \cdots, \quad (1.32)$$

where K is a complete integral and $q = e^{-\pi K'/K}$. Notice that the coefficients give the number of ways the exponents n^2 can be written as a square: as n^2 and as $(-n)^2$ —this is trivial, but Jacobi was to draw profound consequences. Jacobi also found this

³⁵For a short modern explanation, see the Appendix at the end of this chapter.

³⁶We shall not pursue the ways the theory of complex multiplication was to enrich number theory. See the references in Goldstein et al. (2007) or, for a modern mathematical account (Silverman, 1986).

striking identity concerning eighth powers:

$$\begin{aligned} \left((1-q)(1-q^3)(1-q^5)\dots \right)^8 + 16q \left((1+q^2)(1+q^4)(1+q^6)\dots \right)^8 \\ = \left((1+q)(1+q^3)(1+q^5)\dots \right)^8. \end{aligned} \quad (1.33)$$

We mention these identities here because they will entertain us below, when we shall explain how Jacobi derived them (see p. 51). They are deduced ultimately from expressions for $\sin \alpha u$ as a quotient of infinite products, which at that time had only been proved by Abel, and Jacobi's admission that he relied on Abel's work at this point provoked Legendre to reply (see [Legendre and Jacobi 1875](#), 415) that it

testifies to your candour, a quality that accords so well with real talent [...] but having done justice to the beautiful work of M. Abel I place it far below your own discoveries and would like the glory of them, that is to say of their proofs, to belong entirely to you.

Jacobi indeed found independent proofs of the crucial theorems soon afterwards and wrote to Legendre to tell him so on 16 June 1828.

When Jacobi wrote to Legendre on 9 September 1828 he noted that Legendre had not commented on the remarkable series Jacobi had found and noted that the series for $\sqrt{\frac{2K}{\pi}}$ was the key to results about the number of ways a number can be written as a sum of squares. He pointed out that

$$\begin{aligned} \left(\frac{2K}{\pi} \right)^2 &= 1 + \frac{8q}{1-q} + \frac{16q^2}{1+q^2} + \frac{24q^3}{1-q^3} + \dots \\ &= 1 + \frac{8q}{(1-q)^2} + \frac{8q^2}{(1+q^2)^2} + \frac{8q^3}{(1-q^3)^2} + \dots \\ &= 1 + 8 \sum \phi(p) (q^p + 3q^{2p} + 3q^{4p} + 3q^{8p} + \dots), \end{aligned}$$

where the sum is taken over all odd numbers p and $\phi(p)$ is the sum of the factors of p . Since this last series contains every power of q , by comparison with fourth power of the series for $\sqrt{\frac{2K}{\pi}}$, one finds that every number is the sum of four squares. A little more work was to allow Jacobi in the *Fundamenta nova* to deduce that the number of ways a number is the sum of four squares is 8 times the sum of its divisors that do not divide 4.

Jacobi's response to the “knockout” is also to be found in his letter to Legendre of 9 September 1828. “It is” he said “as far above my praise as it is above my own work” valuing it particularly for supplying the rigorous proofs his own work lacked. But he pointed out in a brief note in Crelle's *Journal* ([Jacobi, 1828b](#)) that he knew there were $p+1$ transformations of order a prime p and gave them explicitly as the result of substituting q^p or any of the p distinct values of $q^{1/p}$ into

$$\sqrt{k} = \frac{2(q^{1/4} + 2q^{9/4} + 2q^{25/4} + \dots)}{1 + 2q + 2q^4 + 2q^9 + \dots}. \quad (1.34)$$

Jacobi was also generous in his praise of Abel to Crelle, who was trying desperately to get a job anywhere in Europe for the young Norwegian, whose health was now being weakened by tuberculosis. Also in September 1828 Legendre, Poisson, Lacroix and Maurice wrote from the Institut de France to the Swedish King Karl Johan urging him to find a place for Abel in Stockholm. On October 25th Legendre finally wrote directly to Abel,³⁷ and Abel in his reply (25 November 1828) said that “Jacobi will certainly perfect to an undreamed-of degree the theory of elliptic functions but even mathematics in general. No-one can esteem him more highly than I do”.³⁸ This letter is interesting for other reasons, because in it Abel alludes to a general theory whereby it can be determined whether an arbitrary polynomial equation is solvable by radicals or not. We have already seen how closely linked in Abel’s mind were the theories of elliptic functions and solvability by radicals, a fact which was to be decisive for the future of both subjects.

The events of 1829 can be briefly described. Abel was by then back in Norway, where a temporary improvement in his health was followed by its collapse towards the end of February. While measures proceeded to get him a suitable academic position, his tuberculosis deepened its hold on him, and on the 6th of April, 1829, Abel died.³⁹ He was only 26.

The manuscript Abel had been working on for the last few months, his *Précis*, was published posthumously in the fourth volume of Crelle’s *Journal* later that year. In it, Abel defined the following functions:

$$\Delta(x, c) = \pm \sqrt{(1 - x^2)(1 - c^2 x^2)} \quad (1.35)$$

$$\tilde{\omega}(x, c) = \int \frac{dx}{\Delta(x, c)} \quad (1.36)$$

$$\tilde{\omega}_0(x, c) = \int \frac{x^2 dx}{\Delta(x, c)} \quad (1.37)$$

$$\Pi(x, c, a) = \int \frac{dx}{\left(1 - \frac{x^2}{a^2}\right) \Delta(x, c)}. \quad (1.38)$$

The last three are, respectively, elliptic integrals of the first, second and third kinds. The fundamental problem he tackled in the first part of the paper is to find when a sum of such functions (depending on different parameters and evaluated for different values of the variable) reduces to the sum of an algebraic function and the logarithms

³⁷See Abel (1902, 77–79).

³⁸Abel (1902, 90). Engl. trl. in Ore (1957, 213).

³⁹Gauss wrote to Schumacher on 19 May to say that “Abel’s death, ... is a very great loss to science”. Quoted in Dunnington (2004, 255).

of other algebraic functions of the variables. This is a significant generalisation of the addition formulae of Fagnano and Euler and foreshadows his later paper with arbitrary algebraic integrands (discussed below, see Sect. 4.4). His solution led on to a complete solution of the transformation problem for elliptic functions, which depends on the equation:

$$\frac{dy}{\Delta(y, c')} = \varepsilon \frac{dx}{\Delta(x, c)}, \quad (1.39)$$

where ε is a constant. From that Abel deduced the general transformation of an elliptic function of the first kind and showed that the corresponding moduli were related by an algebraic equation.

In order to be a little more precise, we repeat some of Abel's definitions. Abel introduced the complete elliptic integrals

$$\frac{\tilde{\omega}}{2} = \int_0^1 \frac{dx}{\Delta(x, c)}, \quad \frac{\omega}{2} = \int_0^1 \frac{dx}{\Delta(x, b)}, \quad (1.40)$$

where $b = \sqrt{1 - c^2}$ and noted that if $\lambda(\theta)$ is defined by the equation $\theta = \int_0^{\lambda(\theta)} \frac{dx}{\Delta(x, c)}$, then $\lambda(\theta + 2\tilde{\omega}) = \lambda(\theta)$, and $\lambda(\theta + i\omega) = \lambda(\theta)$.

He defined $q = \exp(-\frac{\omega}{\tilde{\omega}}\pi)$ and set $q_1 = q^\mu = \exp(-\mu \frac{\omega}{\tilde{\omega}} + \mu' i)\pi$, where μ and μ' are rational numbers. Abel then showed that when c is real and less than 1 (but c' may be real or imaginary) and y is an arbitrary function of x , (1.39) can be satisfied if and only if either ε is real and an integer multiple of $\tilde{\omega}'/\tilde{\omega}$, or ε is purely imaginary and a half-integer multiple of $i\omega'/\tilde{\omega}$. In these cases, the modulus c is given by quotient of two infinite products involving q_1 , which Abel gave explicitly. Abel also noted that the modular equation connecting c and c' , while not generally solvable by radicals, has the property that all roots are known rationally once any two are.

In the second part of the paper, Abel replaced the above functions with their inverses expressed in terms of the function $\lambda(\theta)$, so

$$\tilde{\omega}_0(x, c) = \int \lambda^2(\theta) d\theta, \quad \Pi(x, c, a) = \int \frac{d\theta}{\left(1 - \frac{\lambda^2(\theta)}{a^2}\right)}. \quad (1.41)$$

The moduli are now required to be real and less than 1. "In this form", Abel remarked⁴⁰, "the elliptic functions have many interesting properties and are much easier to study. Above all, it is the function $\lambda(\theta)$ that merits particular attention". He showed that it can be represented as the quotient of two everywhere convergent power series,

⁴⁰See [Abel \(1829b, 521\)](#) where he referred to his (1826a).

$$\phi(\theta) = \theta + a\theta^3 + a'\theta^5 + \cdots \quad (1.42)$$

$$f(\theta) = 1 + b'\theta^4 + b''\theta^6 + \cdots, \quad (1.43)$$

and that the functions ϕ and f have similar properties to λ .

Abel also broached the subject of arbitrary integrands. In a paper submitted to the French Académie in 1826 he dealt magnificently with the general case, but at the time of his death the paper was sitting as good as forgotten in a pile of papers in the care of Cauchy. He had, however, also published a short note (Abel, 1828c) on a special but significant case in Crelle's *Journal* in 1828, in which he discussed integrals of the form $\int \frac{rdx}{\sqrt{R}}$, where $r(x)$ is an arbitrary rational function and $R(x)$ is an arbitrary polynomial. The theme of that paper was a generalisation of the addition theorem of the *Précis*, and we shall look at it in Sect. 4.4, where we shall see that this paper innocently brought about the rescue of Abel's declining reputation.

Also in 1829 Jacobi published his first definitive account of the theory of elliptic functions, his book *Fundamenta nova theoriae functionum ellipticarum* (New foundations of a theory of elliptic functions). Writing in Latin was a dying custom, kept alive largely in Germany perhaps out of a desire to reach an international audience, and perhaps because a mastery of Latin was a requirement of all German gymnasias. One recalls that Jacobi was particularly good at languages; in 1832 his appointment as a full professor followed a four-hour disputation in Latin.

Jacobi began his treatise with an account of the first problem that had drawn his attention to the subject, the question of the transformation of elliptic integrals. He wrote:

The problem we shall propose is generally this: to find a rational function y of x such that

$$\frac{dy}{\sqrt{A' + B'y + C'y^2 + D'y^3 + E'y^4}} = \frac{dx}{\sqrt{A + Bx + Cx^2 + Dx^3 + Ex^4}}.$$

He also raised the multiplication problem, which is to find solutions to

$$\frac{dy}{\sqrt{A' + B'y + C'y^2 + D'y^3 + E'y^4}} = \frac{Mdx}{\sqrt{A + Bx + Cx^2 + Dx^3 + Ex^4}}, \quad (1.44)$$

where the quantity M , which may depend on the parameters $A, B, \dots, E$, does not depend on x .

Jacobi's idea was to write $y = \frac{U}{V}$, where U and V are polynomials in x of degrees p and m , respectively, where $p \geq m$. This reduces the integrand in y to another of the same form, possibly multiplied by a constant, provided the constants can be suitably chosen. When this is the case, the transformation is said to be of order p .

Jacobi showed by counting constants that this could be done for any value of p , in other words that there are more coefficients in the polynomials U and V than there are equations for them to satisfy—a typically nineteenth-century approach. Indeed, there will always be three arbitrary constants in any such transformation.

So, as he showed, one can transform $\frac{dy}{\sqrt{(1-y^2)(1-\lambda^2x^2)}}$ into $\frac{Mdx}{\sqrt{(1-x^2)(1-k^2x^2)}}$ with this transformation of order 3:

$$U = x(a + a'x^2) \quad , \quad V = 1 + b'x^2 \quad . \quad (1.45)$$

The moduli λ and k are then related by this equation, where $u^4 = k$ and $v^4 = \lambda$:

$$u^4 - v^4 + 2uv(1 - u^2v^2) = 0 \quad (1.46)$$

and the multiplier is $M = \frac{v}{v+2u^3}$. The transformation of order 5 produces this relation between u and v :

$$u^6 - v^6 + 5u^2v^2(u^2 - v^2) + 4uv(1 - u^4v^4) = 0 \quad , \quad (1.47)$$

where $M = \frac{v(1-uv^3)}{(v-u^5)}$. Equations like these between two moduli he called modular equations (Sect. 24); they later caught the attention of Galois, because the cases of low degree can be remarkably simplified.⁴¹

As we mentioned earlier, Jacobi's solution to these problems proceed by inverting the elliptic integral,

$$u = \int_0^\phi \frac{dt}{\sqrt{1-k^2\sin^2 t}} = \int_0^x \frac{dt}{\sqrt{(1-t^2)(1-k^2t^2)}}$$

to obtain elliptic functions. Following Legendre he called ϕ an angle, the amplitude of the function u , and wrote $\phi = \text{am } u$, $x = \text{sinam } u$. He called x an elliptic function. He defined the complete elliptic integrals

$$K = \int_0^{\pi/2} \frac{dt}{\sqrt{1-k^2\sin^2(t)}} \quad , \quad K' = \int_0^{\pi/2} \frac{dt}{\sqrt{1-k'^2\sin^2(t)}} \quad , \quad (1.48)$$

where k and k' are complementary moduli ($k^2 + k'^2 = 1$). To obtain values for an elliptic function with a purely imaginary argument he changed to a new variable defined by the transformation $\sin \phi = i \tan \psi$, $\cos \phi = \sec \psi$. He then used an addition theorem to define elliptic functions for arbitrary complex variables and finally deduced that the new functions were doubly periodic:

Elliptic functions enjoy two periods, one real and the other imaginary, at least when the modulus k is real. Both are imaginary when the modulus itself is imaginary (we could call this the principle of the two periods).

The first half of the *Fundamenta nova* dealt at length with the theory of transformations of elliptic functions. Jacobi showed that transformations of every order do exist and investigated the number of each order. He studied the effect of a

⁴¹See Gray (2000a, 116–118).

transformation on the moduli and how the composition of two transformations can be made to yield a solution to the multiplication problem. He showed, as Legendre had earlier, that the complete elliptic integrals satisfy the differential equation

$$k(1-k^2)\frac{d^2Q}{dk^2} + (1-3k^2)\frac{dQ}{dk} - kQ = 0 \quad (1.49)$$

and applied this differential equation to the study of the multiplier M .

In the second half of the book he showed how to write the elliptic functions as quotients of one infinite product by another, and how the complete elliptic integrals depended on the modulus. Building on Euler's (1768b) he found such striking formulae as (Sect. 36, nr 7):

$$k = 4\sqrt{q} \left(\frac{(1+q^2)(1+q^4)(1+q^6)\dots}{(1+q)(1+q^3)(1+q^5)\dots} \right)^4, \quad (1.50)$$

where $q = e^{-\pi K'/K}$. The same expression appears in Abel's posthumous *Précis* (Sect. 7).

In Sect. 52 there appeared for the first time in Jacobi's work a quantity that was to play a more important, foundational role in later versions of the theory: the theta function, defined on p. 198 by the formula

$$\theta(u) = \theta(0) \exp \int_0^u Z(t) dt, \quad (1.51)$$

where Z was defined in terms of the complete and incomplete elliptic integrals of the first and second kinds by the formula $Z(u) = \frac{F^1 E(\phi) - E^1 F(\phi)}{F^1 \Delta(\phi)}$.

Jacobi derived a great many equations involving the theta function and the elliptic functions. He investigated it as a function of a complex variable, established addition formulae for it and showed that it was periodic:

$$\theta(u + 2K) = \theta(u) \quad (1.52)$$

but (see Sect. 57) not doubly periodic, and instead that

$$\theta(u + 2iK') = -e^{\frac{\pi(K' - iu)}{K}} \theta(u). \quad (1.53)$$

Many of the power series he introduced, and the book is full of them, were in terms of the variable q . He found series for $\sinam(2Kx/\pi)$ and $\theta(2Kx/\pi) \cdot \theta(0)$, and came close to writing the function $\sinam$ as a quotient of two theta functions. Although the book may have shown to him, as he wrote to Dirichlet, that introducing complex quantities "alone solved all the riddles", his formal approach was to raise as many problems as it solved.⁴² It was enormously rich in power series and identities

⁴²See Dirichlet (1852, 10).

between new functions. What it is not, and could not have been, is anything like a study of functions in the sense of a theory of functions of a complex variable. It is clear in historical grounds why that is. How it prompted such a theory, and was itself changed by it, forms an important part of this book.

1.4 Elliptic Integrals and Elliptic Functions

To what extent are the first theories of elliptic functions theories of functions of a complex variable? The answer is made harder to obtain by the confused state of the early developments of complex function theory, then almost exclusively in the hands of Cauchy. As Chap. 3 describes, the situation was doubly confused: mathematically confused, in that Cauchy's own ideas were in a turbulent state of development, and confused as regards their dissemination.

There is a straightforward sense in which Abel's and Jacobi's ideas do belong in any history of complex function theory. The objects they discussed were genuinely functions and indeed functions of a complex variable. But if one goes beyond this superficial observation and seeks to insist on points of detail, the ground rapidly becomes slippery. What, for example, was a complex variable to either Abel or Jacobi? What, indeed, was a complex number? Neither ever addressed the point head on. Jacobi's answer would surely have been formal: a complex variable is an expression of the form $x + iy$, to which the usual rules of arithmetic apply and $i^2 = -1$. Such formal mathematics was his stock in trade. Abel's answer would have been along the same lines, for if he did not quite match Jacobi's skill with formulae, his interest in mathematics lay heavily on the side of algebra; Sylow, who edited Abel's works, described him as primarily an algebraist. One does not need a geometrical interpretation of complex numbers to pursue the theory of equations.

But how can one justify in a formal way all the manipulations with integrals that they carried out? What does it mean to deduce from $\int_0^u \frac{dt}{\sqrt{1-t^4}} = v$ by means of

the substitution $s = it$ that $\int_0^{iu} \frac{-ids}{\sqrt{1-s^4}} = v$, and so $u(v) = iu(iv)$? What does it mean to extend the addition theorem defined for real variables, and for purely imaginary ones if one accepts the validity of the substitution just described, to functions of a complex variable? The answer we propose is that for Abel and Jacobi the validity of the formal operations conferred a common-sense meaning on the formulae that resulted. So the fact that the integral is not well defined once the variables have become complex but is an infinitely many-valued expression is somewhat beside the point. It is the same with $\log x$; it does not have a unique meaning once x can be complex, but $\log x$ does not arise outside of any context. And given a context it will be clear what it means. Common-sense and a little mathematical sophistication enables one to interpret $\int_1^x \frac{dt}{t}$, and to know that $\frac{d \log x}{dx} = \frac{1}{x}$. In the

same way, simple formal algebra enables you to work with (many-valued) elliptic integrals and complex valued elliptic functions.

It is interesting that in the first flood of invention, the question did not present itself. At no stage in this work did Abel or Jacobi feel compelled to raise it, much less address it. Cauchy's theory of complex integrals is never mentioned. Nor in their entire correspondence did Legendre see fit to hint that, even at some later stage, they might like to benefit from consulting Cauchy's work. Such a robust, common-sense view has a lot to commend it, but there is a price to pay: the nature of a complex integral and its dependence on the path of integration cannot be raised. Perhaps surprisingly therefore we conclude that in the first phase of the creation of a theory of elliptic functions there was no theory of complex integrals, contrary to the claim in [Dieudonné \(1974, 1, 44\)](#). We shall return to this point in Chap. 4, when we shall argue that the absence even in Cauchy's theory of a good grasp of many-valued integrands drove people to seek other foundations altogether for elliptic functions, and to abandon, at least for a while, the starting point of elliptic integrals.

In the same way, there is no hint in this theory of a classification of infinite points into poles of various orders, let alone a distinction between a pole of finite order and an essential singularity. The fact that these functions have points where they become infinite is described, but the recognition that poles are part-and-parcel of a complex function still lay in the future.⁴³ As we shall see, this recognition, nowadays encapsulated in Liouville's theorem, was first obtained when mathematicians sought to define a theory of doubly periodic functions without recourse to elliptic integrals.

The division of elliptic integrals into three kinds, and Abel's later extension of Euler's theorem to any algebraic integral, does not of itself imply a recognition that integrands either have no poles, or simple poles, or poles of higher orders. We shall argue in Chap. 4, after this part of Abel's work has been described in detail, that this view is false if it implies that Abel had such an insight, but true if taken to mean that this way of classifying integrands can be defended in such terms. Accordingly, we see the threefold classification of elliptic integrals as another challenge the theory of elliptic functions presented to any one seeking to base it on a theory of complex functions.

In short, Abel's and Jacobi's theories of elliptic functions were purely formal in so far as they were a theory of complex integrals and complex functions of a complex variable, and so presented an important stimulus to anyone seeking to develop it on the basis of an autonomous theory of functions of a complex variable.

On the other hand, the novelty of the theory cannot be denied. Other than polynomials and the trigonometric and exponential functions, there were very few other functions of a complex variable known at the time. The new theory commended itself to its readers by presenting a natural generalisation of the trigonometric functions in a complex setting. Handling these functions, deducing consequences of their addition laws, and so forth, is intrinsically attractive to a

⁴³The term "pole" was first introduced in [Neumann \(1865a, 38\)](#).

mathematician. For example, the fact that the division of elliptic functions leads to polynomial equations with the striking property of being solvable by radicals, known to Gauss, Abel and Jacobi, was a profound stimulus to Galois.

Another early application was to number theory. An old assertion of Fermat's is that every positive integer is the sum of at most four squares, but it was not proved until Lagrange in 1770 consummated an argument begun by Euler in 1749 (see Weil 1984, 178–179). Jacobi found that the coefficients r_n in one of his power series were essentially the number of ways n could be expressed as a sum of four squares. Because he also knew about the behaviour of this function, he could obtain a lovely identity for r_n :

$$r_n = 8 \sum_{d|n, 4 \nmid d} d.$$

In words, r_n is eight times the sum of the divisors of n that are not multiples of 4. He announced this result in his (1828a) and showed how to prove on the last page of his *Fundamenta nova*. Weil (1984, 186) made the interesting observation that Euler had pointed out in a letter to Goldbach in 1750 that a proof using power series would be the most natural way of proving Fermat's theorem; Jacobi certainly did not know of that letter. Because knowing in how many ways a positive integer is a sum of four squares is deeper than simply knowing that it can be done, number theorists were quick to follow Jacobi's lead (we shall describe some of those achievements in more detail in Sect. 4.7).

Jacobi obtained his results⁴⁴ by working with the function later labelled

$$\theta_3(0, q) = 1 + \sum_{n=1}^{\infty} 2q^{n^2}.$$

The coefficient of q^j is the number of ways the integer $j = n^2$ can be expressed as a sum of one square (i.e., a single square), where we count n^2 and $(-n)^2$ as distinct. It follows that the coefficient of j in the power series expansion of

$$\theta_3(0, q)^m = 1 + \sum_j a_j q^j$$

represents the number of ways j can be written as a sum of m squares, where we count as distinct ways that differ in the order or sign of the summands. For example, $5 = 1^2 + 2^2$, from which we get a total of 8 representations by replacing 1 with -1 , or 2 with -2 or switching the order of 1 and 2 and so on.

This series did not enable Jacobi to write down the number of ways any given number j can be expressed as a sum of squares: a formula for the coefficient of q^j is needed for that. But Jacobi had come at the power series via his theory of elliptic functions, and he knew that they were periodic. He therefore followed the

⁴⁴See the very helpful exposition by Eric Conrad: Jacobi's Four Square theorem, at <http://www.math.ohio-state.edu/econrad/Jacobi/sumofsq/sumofsq.html>.

clear example set by Fourier in 1822 and expressed his periodic functions as Fourier series. He obtained these Fourier series:

$$\frac{2K}{\pi} \sin \operatorname{am} \left(\frac{2Kx}{\pi}, k \right) = \frac{4}{k} \sum_{j=0}^{\infty} \frac{q^{(2j+1)/2}}{(1 - q^{2j+1})} \sin(2j+1)x .$$

$$\frac{2K}{\pi} \cos \operatorname{am} \left(\frac{2Kx}{\pi}, k \right) = \frac{4}{k} \sum_{j=0}^{\infty} \frac{q^{(2j+1)/2}}{(1 + q^{2j+1})} \cos(2j+1)x .$$

$$\frac{2K}{\pi} \Delta \operatorname{am} \left(\frac{2Kx}{\pi}, k \right) = 1 + 4 \sum_{j=0}^{\infty} \frac{q^j}{(1 + q^{2j})} \cos 2jx .$$

Knowledge about elliptic functions therefore turns into knowledge about these power series. More precisely, the series for $\Delta \operatorname{am}$, with $z = 0$ is

$$\frac{2K}{\pi} = 1 + 4 \sum \frac{q^j}{1 + q^{2j}};$$

and the series for its square is

$$\left(\frac{2K}{\pi} \right)^2 = 1 + 8 \sum \frac{q^j}{(1 + (-q)^j)^2};$$

and, of course,

$$\theta_3(0, q) = \sqrt{\left(\frac{2K}{\pi} \right)}.$$

Jacobi expanded the series for $\frac{2K}{\pi}$ as a power series and obtained

$$1 + 4 \sum_{j=1}^{\infty} (d_1(j) - d_3(j)) q^j,$$

where $d_1(j)$ is the number of divisors of j that are congruent to 1 mod 4 and $d_3(j)$ is the number of divisors of j that are congruent to 3 mod 4. This gave him this theorem: The number of ways a positive integer can be a sum of two squares is equal to four times the difference of the numbers of divisors congruent to 1 and 3 modulo 4.

When Jacobi expanded the series for $\left(\frac{2K}{\pi} \right)^2$ he obtained

$$1 + 8 \sum_{k|j, 4 \nmid k} (\sum k) q^j ,$$

where the sum is taken over all divisors of j that are not divisible by 4. This gave him his four squares theorem: The number of ways an integer can be written as the

sum of four squares is equal to eight times the sum of all its divisors which are not divisible by 4.

One other application of the new functions is worth mentioning. It rapidly became important, and remained so, and it has recently been studied again by mathematicians (see [Griffiths and Harris 1978](#)). This is the interpretation Jacobi gave to a striking theorem of Poncelet about polygons, called the Poncelet closure theorem. The theorem has a complicated history but the crucial step had been taken by Poncelet in his *Traité des propriétés projectives des figures* (1822).⁴⁵ Take any two conics C and D with C inside D , say, and suppose for simplicity that both of them are circles, centres C_0 and D_0 , respectively. Pick a point P_0 on D and draw a tangent from it to C , which meets D again at P_1 , say. Repeat this construction starting with P_1 to obtain P_2 , P_2 to obtain P_3 , and so on.

In this way a sequence of points $\{P_n\}$ is obtained on D , and Poncelet's theorem asserts that if, for some n , $P = P_0$, the sequence closes and therefore there is an n -gon inscribed in D and circumscribing C , then there is always such an interscribed n -gon whatever position is taken for P_0 . Poncelet's proof is a tour de force of geometry. Jacobi, basing his approach on Steiner's investigation of the special cases $n = 5$ and $n = 6$, investigated the relationship between ϕ_n and ϕ_{n-1} where ϕ_n is the angle $P_n C_0$ makes with $C_0 D_0$. It is easy enough to find this relationship:

$$\tan\left(\frac{\phi_{n+2} + \phi_n}{2}\right) = \frac{R-a}{R+a} \tan \phi_{n+1},$$

where R is the radius of D and $a = |C_0 D_0|$. "In this form of the equations" said [Jacobi \(1828c, 285\)](#) "it springs to the eyes at once that they coincide with those for the multiplication of elliptic functions" and he showed that

$$\phi_n = \text{am}(u + nt),$$

where in his notation $\int_0^\phi \frac{dt}{\sqrt{1-k^2 \sin^2 t}} = u$, so $\phi = \text{am} u$, and k is an explicit function of R and a . So the n -gon closes when $\text{am}(u + nt) = \text{am} u$, which it does for suitable t and n independently of u (nt has merely to be a period). So unexpected and simple was Jacobi's proof that many later writers referred to it as Jacobi's geometric proof of the multiplication formula for elliptic functions, thus completely reversing the intention of his original argument.

1.4.1 Immediate Responses

The dramatic originality of the work of Abel and Jacobi, its richness, subtlety, and depth, did not make it easy to contribute to their work, and indeed the first responses

⁴⁵Given a fascinating discussion in [Bos et al. \(1987\)](#).

are little more than reworkings of aspects of the original papers. But they do show how widely the news spread.

The Italian geometer and astronomer Giovanni Plana expressed his view in his (1829), which was devoted entirely to giving a quicker derivation of some of the formulae in Jacobi's work and made no mention of Abel's. His fundamental criticism still strikes one as fair: "one does not easily understand by what chain of ideas he could have been led naturally to the singular form that he gives a certain rational function of a single variable that serves as his point of departure for his demonstration But once the discovery is made, one can be curious to know if there is effectively another, elementary and direct, way". (Plana, 1829, 333).

The same opinion was held by James Ivory, in a memoir to the Royal Society of London (Ivory, 1829) although he was much more laudatory in his comments on the work of Abel and Jacobi as a whole. Even so (Ivory, 1829, 351), "...it seldom happens that an inventor arrives by the shortest road at the results which he has created, or explains them in the simplest manner. The demonstrations of M. Jacobi require long and complicated calculations; and it can hardly be said that the train of deductions leads naturally to the truths which are proved". His own contributions was to simplify and unify the Jacobian theory of transformations by pursuing the analogy with the trigonometric functions more closely.

The first book on the subject came out in 1841: Pieter Frans Verhulst's *Traité élémentaire des fonctions elliptiques, [etc.]*, published in Brussels in 1841. This was entirely devoted to the requisite calculations but is further evidenced that the topic of elliptic functions was widely considered to be important.

1.5 Gauss

Gauss's ideas about complex functions emerged in the course of his work on a variety of topics, many of them to do with elliptic integrals and elliptic functions and then with the theory of numbers, so it seems best to follow him in that respect, and postpone a discussion of his ideas about complex numbers and functions until the end of this section.

Carl Friedrich Gauss has, rightly, been the subject of several biographies of which Dunnington (2004) and Wussing (2011) are the most complete and Bühler (1981) the best introduction to Gauss as a mathematician (Fig 1.5). Gauss was born in 1777 in Brunswick to a working class family. His intellectual precocity brought him to the attention of the Duke of Brunswick at the age of 14, who paid him a stipend to attend the Collegium Carolinum in Brunswick. From there he went to the University at Göttingen, still on the Duke's stipend, and by now he was making the first of his many original discoveries. The first to be published was his proof that the regular 17-sided polygon can be constructed by ruler and compass alone, which concealed a profound insight into the structure of polynomial equations from the mathematician he showed it to, the by-then elderly Georg Kaestner. In 1801 Gauss

Fig. 1.5 Carl Friedrich Gauss (Portrait by Schwartz, in Bühler, *Gauss: a biographical study*, Springer ©1981, frontispiece)



published his major book on number theory, his *Disquisitiones arithmeticae*,⁴⁶ some of whose implications we shall trace below, but he became famous in the astronomical community that year too.

The asteroid Ceres had been found by the Italian astronomer Piazzi on 1 January, 1801 but it had disappeared behind the sun after 41 nights of observations. The news of a possible new planet generated much excitement, and by September 1801 Piazzi had published all his data. Professional astronomers now raced to find it, but in 1 January, 1802 two German astronomers, Olbers and von Zach, independently confirmed that Gauss's predictions had proved sufficiently accurate to locate Ceres once again (Dunnington 2004, 49–55). It has since become clear that Gauss had invented the least square law in statistics in order to make the best sense of Piazzi's data.⁴⁷

It was astronomy that was to provide Gauss with a career. German telescopes were the state of the art, the heavy computational load was congenial to Gauss because he was also one of the few major mathematicians to have extra-ordinary powers of mental arithmetic, and the value the German intellectual community placed on astronomy helped Gauss to feel he was repaying his debt to the Duke of Brunswick. He was active for many years in the discovery of more and more asteroids and enjoyed his contacts with the astronomical community; men like Bessel and Olbers counted among his friends. It led in turn to the arduous work

⁴⁶See Goldstein et al. (2007).

⁴⁷See Dunnington (2004, 498) and for a vigorous defence of Legendre's rights in the matter Stigler (1986).

Gauss spent on the survey of estates of Hannover in the 1820s, from which he distilled his reformulation of differential geometry and the crucial discovery of the intrinsic nature of (Gaussian) curvature. In the 1830s and 1840s his interest in physics led him to work on geomagnetism, where he organised the first international survey of the Earth's magnetic field and its variation and confirmed that the Earth has only two magnetic poles (some people had suggested four). He and his assistant Wilhelm Weber also wired up Göttingen to equip it with a functioning telegraph, but it worked without amplifiers and so was confined to un-economic distances. Out of this work came Gauss's influential treatment of potential theory.

And all this time he worked on mathematics: four proofs of the fundamental theorem of algebra; six proofs of quadratic reciprocity; he was the first to be convinced that the geometry of space might be non-Euclidean (ahead of Bolyai and Lobachevskii). It is to his achievements in the fields of complex function theory and elliptic functions that we now turn.

1.5.1 *Gauss on Elliptic Integrals and Elliptic Functions*

We have already quoted Gauss's remark that Abel had come about a third of the way in his theory of elliptic functions. We shall now consider in detail what Gauss had accomplished in 35 years of intermittent work on that theme. The richness of material forces us to be selective; we shall concentrate on how he organised and re-organised his way through the subject, looking particularly at the implications for a theory of functions of a complex variable. Paradoxically, it was Gauss's marked reluctance to publish that has left the historian with such a wealth of material. With no polished version of his ideas in print, Gauss had no incentive to destroy his working notes. Instead of a book that surely would have hidden the routes to his discoveries we have a profusion of drafts and formulae. All these are collected in the Gauss *Werke*, where, moreover, they have been exceptionally well analysed by Schlesinger and others. Indeed the literature on this topic, from Klein and Schlesinger through Geppert and down to Cox in our own day forms one of the high points in the history of modern mathematics.⁴⁸

Gauss first considered the lemniscatic integral

⁴⁸This material includes Gauss's diary (for an English translation, see [Gray 1984b](#), reprinted with corrections in Dunnington 2004) which enables some discoveries to be dated precisely, some of Gauss's pocket notebooks, often dated on the first page, and other jottings including marginalia. From this it is possible to build up a reasonably detailed chronology of events, as was done by Schlesinger (Gauss, *Werke*, 10.2), and we shall follow his chronology except at one point, to be mentioned below. Accordingly, we have suppressed all the analysis of how the discoveries are dated. The interested reader should consult [Cox \(1984\)](#) and the essay by Schlesinger.

$$z = \int_0^x \frac{dx}{\sqrt{(1-x^4)}} \quad (1.54)$$

in September 1796, when he expanded the integrand as a power series and integrated it term by term, thus obtaining z as a power series in x . This he then inverted by the formal methods of reversion of series, obtaining x as a power series in z . In January 1797 he read Euler's posthumously published paper (1775d) on elliptic integrals, and Stirling's book *Methodus differentialis* of 1730. From Euler's paper he learned this remarkable result: if

$$A = \int_0^1 \frac{dx}{\sqrt{(1-x^4)}} \quad \text{and} \quad B = \int_0^1 \frac{x^2 dx}{\sqrt{1-x^4}}$$

then $AB = \pi/4$. Gauss now began "to examine thoroughly the lemniscate" (Diary entry nr. 51). He called the function z of x defined above by several names; settling on sl for *sinus lemniscaticus* and cl for the cosine, so $cl(x) = sl(\frac{\varpi}{2} - x)$. We may therefore write one of his findings this way⁴⁹:

$$sl^2(x) + cl^2(x) + cl^2(x)sl^2(x) = 1.$$

This is Euler's addition formula for elliptic integrals in the new setting. Other formulae followed, for such things as $sl(2x)$ and $sl(x_1 + x_2)$. The one for $sl(3x)$ is interesting and caught Gauss's attention. Writing s for $sl(x)$, it is:

$$sl(3x) = \frac{s(3 - 6s^4 - s^8)}{1 + 6s^4 - 3s^8}. \quad (1.55)$$

So division of the lemniscatic sine by 3 leads to an equation of degree 9, whereas division of the ordinary sine leads only to a cubic equation. On March 19 he noted in his diary (entry nr. 60): "Why dividing the lemniscate into n parts leads to an equation of degree n^2 ". The reason is that all but n of the roots are complex.⁵⁰ He thereupon regarded sl and cl as functions of a complex variable. The change of variable from x to ix in the lemniscatic integral establishes that $sl(ix) = isl(x)$, from which formulae for $sl(x_1 + ix_2)$ were written down. Similarly, $cl(ix) = 1/cl(x)$.

Gauss had already seen that the addition formula implies that, as a real function, sl was a periodic function with period 2ϖ , where $\int_0^1 \frac{dx}{\sqrt{1-x^4}} = \frac{\varpi}{2}$. It followed that the complex function sl had two distinct periods, 2ϖ and $2i\varpi$, and so, when m and n are integers,

⁴⁹Here we follow the draft *Elegantiores integralis* $\int \frac{dx}{\sqrt{1-x^4}}$ proprietatis, which dates from before 1801, see Gauss *Werke* 3, 404–412.

⁵⁰Two days later he showed that the lemniscate is divisible by ruler and compass into five parts, thus finding an unexpected parallel between the divisibility theories of the lemniscate and the circle.

$$sl(x + (m + in)2\varpi) = sl(x),$$

the first occurrence of the Gaussian integers, $m + in$ (see *Werke* 3, 411–412). It was therefore easy to write down all the points where sl and cl were zero or infinite, and so for Gauss to write sl and cl as quotients of two infinite series. Gauss wrote $sl(x) = M(x)/N(x)$ and looked at the functions M and N in their own right as functions of a complex variable. The formulae giving $sl(2x)$ in terms of $sl(x)$ yielded formulae for such things as $N(2x)$, and special values of $sl(x)$ could be found from the simplest ones by using the addition formulae. So Gauss did one of his provocative little sums. He calculated $N(\varpi)$ to five decimal places and its log to 4 and noted that this seemed to be $\pi/2$. He wrote “Log hyp this number = $1.5708 = \frac{1}{2}\pi$ of the circle?” (*Werke*, 10.1, 158). In his diary (29 March 1797, nr. 63) he checked this coincidence to six decimal places and commented that this “is most remarkable and a proof of this property promises the most serious increase in analysis”.

This observation excited Gauss because he connected it to Euler’s remarkable result $AB = \pi/4$. Here $A = \varpi/2 = \int_0^1 \frac{dx}{\sqrt{(1-x^4)}}$ is the value of the complete lemniscatic integral, and so B must be $\pi/2\varpi$. This yields another connection between ϖ and π .

1.5.1.1 The Arithmetic–Geometric Mean

In July 1798 an improved representation of the numerator and denominator of the lemniscatic functions led Gauss back to the calculation of ϖ , and he commented in his Diary (July, nr. 92): “We have found out the most elegant things exceeding all expectations and that by methods which open up to us a whole new field ahead”. His entry to this new field was, however, still a year away. He found it on returning to an old love of his, the arithmetic–geometric mean. This is defined for two real numbers a and b as follows: set $a_0 = a$ and $b_0 = b$, and recursively

$$a_{n+1} = \frac{1}{2}(a_n + b_n), \quad \text{and} \quad b_{n+1} = \sqrt{a_n b_n}. \quad (1.56)$$

Then it is easily seen that the two sequences (a_n) and (b_n) converge to the same value, called the arithmetic–geometric mean of a and b , abbreviated to the agm and written $M(a, b)$ here.⁵¹ Since $M(\lambda a, \lambda b) = \lambda M(a, b)$, $M(a, b) = aM(1, b/a)$, and it is enough to study $M(1, x)$. On 30 May 1799 Gauss wrote in his Diary (nr. 98): “We have found that the arithmetic–geometric mean between 1 and 2 is π/ϖ to 11 places, which thing being proved a new field in analysis will certainly be opened up”. A letter from Pfaff to Gauss (*Werke* 10.1, 232) makes it clear that even in November 1799 the proof was eluding Gauss.

⁵¹Convergence only fails if the initial values chosen include 0.

One of the Gauss's earliest results about the agm was a power series for $M(1, 1+x)$. It seems not to be known how Gauss first came to this series, but in 1800 he gave this method (*Werke* 3, 365). He found, on writing $x = 2t + t^2$, that

$$\begin{aligned} M(1, 1+x) &= M(1, 1+2t+t^2) \\ &= M(1+t, 1+t+t^2/2) = (1+t)M\left(1, 1+\frac{t^2}{2(1+t)}\right) \end{aligned}$$

so

$$M(1, 1+2t+t^2) = (1+t)M\left(1, \frac{t^2}{2(1+t)}\right). \quad (1.57)$$

So, on setting $M(1, 1+x) = 1 + ax + bx^2 + \dots$ and expanding both sides and collecting like terms, he obtained the coefficients of the power series expansion of $M(1, 1+x)$.⁵² The series begins

$$M(1, 1+x) = 1 + \frac{x}{2} - \frac{x^2}{16} + \frac{x^3}{32} - \frac{21x^4}{1024} + \dots$$

Gauss also considered two functions related to the agm and which also have attractive power series expansions. The first is $M(1+x, 1-x)$, and the second its reciprocal.⁵³ This second function turned out to be most important in all the future developments. It has this expansion:

$$K(x) = \frac{1}{M(1+x, 1-x)} = 1 + \frac{x^2}{4} + \frac{9x^4}{64} + \frac{25x^6}{256} + \dots \quad (1.58)$$

The coefficient of x^{2n} is $\left(\frac{1.3.5\dots(2n-1)}{2.4.6\dots 2n}\right)^2$. He showed formally that the function $K(x)$ satisfies the linear ordinary differential equation

$$(x^3 - x)K'' - K'(3x^2 - 1) + xK = 0 \quad (1.59)$$

and noted that another independent integral of this differential equation is $M^{-1}(1, x)$.

Gauss connected this result with an earlier calculation of his (*Werke* 10.1, 267). As soon as he had considered the functions sl and cl as functions of a complex variable, he had written down the formal Fourier series expansion of $\frac{1}{\sqrt{(1+\sin^2 V)}}$:

⁵²Gauss did not check the convergence of this series, but the agm converges very fast and its use is the key to the rapid evaluation of elliptic integrals.

⁵³Schlesinger (1912, 63) suggested that it was the appearance of the reciprocal of $M(1, \sqrt{2})$ that led Gauss to consider not the agm in general but its reciprocal.

$$\frac{1}{\sqrt{(1 + \sin^2 V)}} = a + b \cos 2V + c \cos 4V + d \cos 6V + \dots \quad (1.60)$$

and observed that

$$a = 1 - \frac{1^2}{2^2} + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2} - \dots = \frac{2}{\pi} \int_0^1 \frac{dx}{\sqrt{1-x^4}}, \quad (1.61)$$

which is, of course ϖ/π . Now he expanded $1/\sqrt{(1+y\sin^2 V)}$ similarly and wrote down that the reciprocal of $M(1, \sqrt{1+y})$ is the V -free part (as he called it, *Werke* 10.1, 183) of the expansion of $\frac{1}{\sqrt{1+y\sin^2 V}}$. This gave him that

$$M(1, \sqrt{1+y}) \cdot \int_0^1 \frac{dV}{\sqrt{1+y\sin^2 V}} = \pi/2.$$

He now set $y = -k^2$, and this became

$$M(1, \sqrt{1-k^2}) \cdot \int_0^1 \frac{dV}{\sqrt{1-k^2\sin^2 V}} = \pi/2. \quad (1.62)$$

So Gauss had shown that the complete elliptic integral, regarded as a function of the modulus k , was given by the reciprocal of the agm at the complementary modulus, $k' = \sqrt{1-k^2}$.

The significance of the clue given by the value of π/ϖ is now revealed: when $k = i$ the above expression for M reduces to

$$M(1, \sqrt{2}) \cdot \int_0^{\pi/2} \frac{dV}{\sqrt{1+\sin^2 V}} = \pi/2.$$

By means of the known value of $\varpi/2$ for the complete lemniscatic integral

$$\int_0^{\pi/2} \frac{dV}{\sqrt{1+\sin^2 V}} = \int_0^1 \frac{dx}{\sqrt{1-x^4}},$$

this reduces to $M(1, \sqrt{2}) = \pi/\varpi$.

Thus Gauss had the conceptual explanation he sought for the numerical coincidence that had intrigued him for so long, and it was obtained by generalising the original question about complete lemniscatic integrals to the setting of complete

elliptic integrals. The enigmatic value of $N(\varpi)$ was simultaneously explained from the power series expansion of N , for the value of $\int_0^\pi \frac{d\phi}{\sqrt{1-k^2 \cos^2 \phi}}$, a complete elliptic integral of the first kind, is $\pi N(k)$.

During the first few months of 1800 Gauss pursued this insight until he could confide in his Diary on May 6 (nr. 105) that he had led the theory of elliptic integrals “to the summit of universality”. Even that estimation had to be promptly set aside. On May 22 the theory was “greatly increased and unified”, becoming “most beautifully bound together and increased infinitely” (nr. 106). These entries, and two more written on or before June 3, make it clear that Gauss, who had already studied the lemniscatic functions and obtained a crucial insight into the value of the general complete elliptic integral, now proceeded to invert the general elliptic integral. In short, he began to study the general elliptic function with (real) modulus k . The formula connecting the complete integral and the agm immediately gave him an expression for a period in terms of the modulus.

Some aspects of the generalisation are straightforward, although never trivial.⁵⁴ A substantial result was obtained by Gauss in May 1800 (Diary entry nr. 108): the new elliptic functions can be expanded as quotients of power series. So Gauss now, as other authors were to later, pursued two approaches. One proceeded from the inversion of an elliptic integral depending on a parameter k . The other developed elliptic functions directly as quotients of entire functions, without reference to an integral. The natural question then is: do these two approaches describe the same objects?

On the first approach, an elliptic integral depends on its modulus, k . The corresponding inverse elliptic function has periods that are functions of this modulus, and these periods are multiples of the complete integrals. Gauss’s second approach proceeded in a way that was to be paradigmatic in the theory of complex functions: he sought to represent his new functions as quotients of (to use modern language) holomorphic⁵⁵ functions. That is, he represented a function f as a quotient $f(z) = \frac{g(z)}{h(z)}$ where g and h have no poles in the complex plane and the zeros of g correspond to the zeros of f while the zeros of h correspond to the poles of f . This approach would have been more influential if Gauss had published it in 1800 when he discovered it, but he left it to others to discover, as, happily, they did, and it was only published in 1868 in the third volume of his *Werke*.

In the lemniscatic case, Gauss had introduced functions P and Q as follows. He introduced a new variable, $s = \sin(\varphi\pi/\varpi)$. The function $sl(\varphi)$ has period 2ϖ , so it can be written as a function of s . Gauss found that $sl(\varphi) = P(\varphi)/Q(\varphi)$, where P and Q had zeros where the quotient function sl has, respectively, zeros and infinities, and the relationships between $P(\varphi)$ and $P(\varphi + \text{period})$ and $Q(\varphi)$ and $Q(\varphi + \text{period})$ ensure that the quotient is a doubly periodic function.

⁵⁴For example, the addition theorem, written down in November 1799, see (*Werke* 10.1, 196).

⁵⁵This term was introduced by Briot and Bouquet in (1875, 14).

More precisely, Gauss showed (*Werke* 3, 416) that P can be written as this infinite product, in which $s = \sin(\frac{\pi}{\varpi}\phi)$:

$$P(\phi) = (\varpi/\pi)s \prod_{k=1}^{\infty} \left(1 + \frac{s^2}{\sinh^2 k\pi}\right)$$

and Q as this one:

$$Q(\phi) = \prod_{k=0}^{\infty} \left(1 - \frac{s^2}{\cosh^2(2k+1)\pi/2}\right).$$

Gauss also expressed P and Q as Fourier series (*Werke* 3, 418), where

$$P(\phi\varpi) = 2^{3/4} \sqrt{\frac{\pi}{\varpi}} \left(e^{-\pi/4} \sin(\phi\pi) - e^{-9\pi/4} \sin(3\phi\pi) + e^{-25\pi/4} \sin(5\phi\pi) - \dots \right) \quad (1.63)$$

and

$$Q(\phi\varpi) = 2^{-1/4} \sqrt{\frac{\pi}{\varpi}} \left(1 + 2e^{-\pi} \cos(2\phi\pi) + 2e^{-4\pi} \cos(4\phi\pi) + \dots \right).$$

This makes it very clear that Gauss had already come to the theory of theta functions that was later to animate Jacobi (and indeed Gauss had four such functions—we have suppressed his R and S).

To obtain the functions that do for a general elliptic function what P and Q do for the lemniscatic function, Gauss merely had to observe how the quotient P/Q has the appropriate periods in the lemniscatic case and make appropriate modifications.⁵⁶ Since he knew how the periods depend on the modulus, his journey from integral to quotient was concluded.

The difficult way round is to see that every function defined as a quotient arises as the inverse of a suitable elliptic integral. Gauss had shown (*Werke* 10.1, 69) that the periods of a general elliptic function obtained from an elliptic integral with modulus k (i.e. of the form $\int \frac{dx}{\sqrt{(1-x^2)(1-k^2x^2)}}$) are

$$\varpi = \frac{\pi}{M\left(1, \sqrt{1-k^2}\right)}, \quad \text{and } \varpi' = \frac{\pi}{kM\left(1, \sqrt{1+\frac{1}{k^2}}\right)}. \quad (1.64)$$

⁵⁶The word “merely” here is ironic; the step is hardly an easy one.

If one follows Gauss and writes $k = \tan v$, these expressions for ϖ and ϖ' become⁵⁷ $\pi \cos v / M(1, \cos v)$ and $\pi \cos v / M(1, \sin v)$. So the problem is to show, given functions P and Q , say, whose quotient P/Q has periods $m\varpi + m'i\varpi'$, that ϖ and ϖ' can be expressed as these functions of k : $\varpi = \pi \cos v / M(1, \cos v)$ and $\varpi' = \pi \cos v / M(1, \sin v)$.

By a simple scaling argument, it is enough to find k such that the ratio ϖ/ϖ' is correct. This ratio is $M(1, \cos v) / M(1, \sin v)$, and so Gauss defined the function $z = \exp\left(-\frac{\pi}{2} \cdot \frac{M(1, \cos v)}{M(1, \sin v)}\right)$. The problem is now to show that this function takes every possible value between 0 and 1 as v goes between 0 and $\pi/2$. This is reasonably clear from the power series expansion, which shows that as t tends to 0, $M(1, t)$ tends to infinity.⁵⁸

There was a further generalisation to be made. All the elliptic functions that have arisen so far have had one real and one purely imaginary period. This came about because the corresponding elliptic integrals had a real modulus. This restriction arose historically because real quantities were regarded as completely general, and because elliptic integrals with real moduli make geometrical and physical sense. Nonetheless, once elliptic functions have been shown to be best considered as complex functions of a complex variable, it is possible to abandon this restriction on the periods. Once this is done, the ratio of the periods can no longer be purely imaginary, and so the agm must itself become a complex-valued function of a complex variable. This raises a very difficult problem indeed.

The nub of the difficulty is that the square root of a complex number is not uniquely defined, nor is there a workable convention for selecting a square root akin to the rule “choose the positive root of a positive number”. Instead, at each stage in the computation of an agm of two complex numbers, a choice must be made.⁵⁹ This freedom of choice affects the outcome, but it does not wreck the whole story. It turns out that at each stage there is always a correct choice for the square root which ensures that the sequences defining the agm converge to a common limit. Indeed, if the numbers at the n th stage are a_n and b_n (and therefore $a_{n+1} = (a_n + b_n)/2$), then b_{n+1} is the correct root of $a_n b_n$ if $|a_{n+1} - b_{n+1}| \leq |a_{n+1} + b_{n+1}|$ and, if equality holds, $\text{Im}(b_{n+1}/a_{n+1}) > 0$. Moreover, if a finite number of incorrect choices are made, the series still converge to a common limit, although generally one different from the limit obtained when all the choices were correct.

These convergence claims are not difficult to verify. Gauss seems to have yet again found the way forward by a computation, not of the most general case but of a case in which one of the initial values was negative. Much more striking is his observation, made in the diary on 3 June 1800 (nr. 109), that connection

⁵⁷See (*Werke* 10.1, 194 and 197), and (Cox, 1984, 325).

⁵⁸Schlesinger suggests that Gauss was aware of the much more precise limit $\lim_{x \rightarrow \infty} \frac{1}{x} M(1, x) \log 4x = \pi/2$, see (*Werke* 10.1, 268).

⁵⁹How this can be done is well described in Cox (1984).

between the infinitely many means has been completely cleared up. It is followed two days later by the remark (nr. 110): “We have now immediately applied our theory to elliptic transcendents”, confirming the interpretation that the way Gauss took to generalising his theory of elliptic functions was to make the agm into a complex function. Unfortunately, nothing survives from 1800 to indicate what Gauss’s complete solution was. The best indication we have is from a note from as late as 1825, which says (*Werke*, 10.1, 219) “The agm changes if one chooses a negative value (for the square root): yet all results take the following form: $\frac{1}{(\mu)} = \frac{1}{\mu} + \frac{4ik}{\lambda}$ ”. This asserts a fairly simple connection between any three values λ , μ and (μ) , of the agm of two numbers, which is indeed valid under certain assumptions one may suppose Gauss made tacitly; one requires that both $(\mu)/\lambda$ and μ/λ have positive real part. If this requirement is dropped, then one obtains instead:

$$\frac{1}{(\mu)} = \frac{\delta}{\mu} \frac{\gamma}{\lambda}, \quad (1.65)$$

where δ and γ are integers satisfying $\delta \equiv 1, \gamma \equiv 0 \pmod{4}$.

It would be interesting to know when Gauss came to this insight into the agm as a complex function. It is intimately tangled up with other questions, such as his developing knowledge of the study of certain related functions as complex functions. The mathematical situation is that there is no way known of proving the claim about the agm which does not lead through some of the deepest parts of the theory of these other functions as complex functions.

Schlesinger (1912, 95) argued that Gauss seems not to have calculated agms of complex numbers directly, but to have worked with functions p , q and r defined as follows (see Gauss *Werke* 10.1, 218)

$$p(t) = 1 + 2t + 2t^4 + 2t^9 + \dots, \quad (1.66)$$

$$q(t) = 1 - 2t + 2t^4 - 2t^9 + \dots, \quad (1.67)$$

$$r(t) = 2t^{1/4} + 2t^{9/4} + 2t^{25/4} + \dots \quad (1.68)$$

and connected to the agm of a and b this way: $p^2(t) = a/M(a, b)$, $q^2(t) = b/M(a, b)$, $r^2(t) = a^2 - b^2$. At one stage in 1810 Gauss (*Werke* 10.1, 224) studied how the functions p , q , and r transform under the transformation

$$t \mapsto \frac{\alpha t - \beta i}{i\gamma t + \delta} \quad (1.69)$$

for the six cases where the matrix is congruent to the identity matrix mod 2. These transformations show (following Gauss, *Werke* 3, 478, dated 1827) that the ratios $p^2 : q^2 : r^2$ are unaltered by transformations where γ and δ are as above. Schlesinger interpreted this as the mathematical meaning of the Diary entry nr. 109 of June 3.

Elsewhere (in 1805, see *Werke* 3, 477 and 10.2, 102–103), Gauss drew the diagram in Fig. 1.6, which was misunderstood by Schering when editing the third

Fig. 1.6 Gauss's diagram as reproduced in *Werke* 3, 477

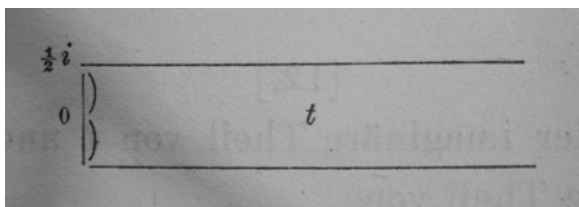
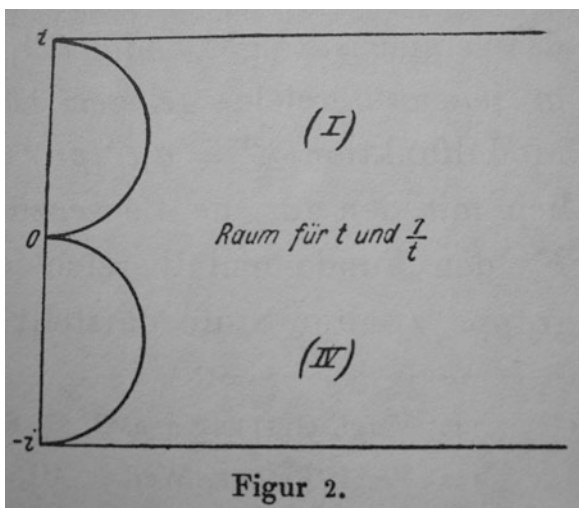


Fig. 1.7 Gauss's diagram as corrected in *Werke* 8, 105



volume of Gauss's *Werke*, p. 478, but corrected by the editors of Vol. 8 (see *Werke* 8, 105 and Fig. 1.7). Its significance is that any function of a complex variable t which is invariant under the substitutions $t \mapsto t + i$ and $t \mapsto 1/t$ takes all its values on precisely the domain indicated.

In another place (*Werke* 8, 101) Gauss observed that, if $q(t)/p(t) = A$ is regarded as an equation for t , then the solution is $t = \mu/\lambda$ where, if $A^2 = n/m$, $\mu = M(m, n)$ and $\lambda = M(m + n, m - n)$. To connect this with what has gone before, suppose without loss of generality that $m = 1$ and replace $n = A^2$ by $x = \cos v$ so $\mu/\lambda = M(1, x)/M(1, (1 - x^2)) = M(1, \cos v)/M(1, \sin v)$, which we shall temporarily write $g(x)$. Write the other quotient $q(t)/p(t)$ as $f(t)$. Then the claim is that $f^2(t) = x$ if $t = g(x)$, in other words, f^2 and g are inverse functions. Moreover, Gauss went on, "one obtains thus only one value t ; all others will be contained in the formula $\frac{\alpha t - 2\beta i}{\delta - 2\gamma i}$, where α, β, γ , and δ are all integers that satisfy the equation $\alpha\delta - \beta\gamma = 1$ ". Putting this together with the previous results, we see that Gauss had a complete solution to the inversion of elliptic integrals even when they depended on a complex parameter k .

The only question is when. Schlesinger and Cox argue that the Diary entry nr. 109 of June 3 1800 enables one to date the discovery to 1800. The absence of accompanying material, and the presence of appropriate material of a later date is,

on this view, just an accident of transmission. An alternative view would be that the surviving material indicates accurately when these difficult insights were made, and that the Diary entry is ambiguous. There are occasions when Gauss made claims in his Diary of results whose truth he had become certain but whose proof eluded him; the striking claims made in entry nr. 92 and their delayed resolution have already been discussed, and nr. 118, May 1801 is another case in point (see below Sect. 4.7.1). It is surely possible that the diary entries mean only that Gauss had begun to investigate the agm for complex values, and had been pleased to suspect a connection between them. The detailed verification had to wait for a much later date.

Since Gauss never published an account of his discoveries, dating them precisely is interesting but not vital, and we shall not insist on it further. In this book we have three questions to ask about Gauss's work:

- How does it compare with the later discoveries of Abel and Jacobi?
- To what extent was it an example of complex function theory?
- And to what extent did it influence his ideas about complex functions in general?

The theory Gauss developed of what might be called elliptic functions with a real modulus had, as we saw, two aspects: a direct approach starting with the inversion of the corresponding elliptic integral and an indirect approach starting from some suitable power series. The agreement between these two theories was assured by establishing certain properties of the agm, thought of as a function of the real modulus. The fruits of these theories were such things as the rich collection of identities between various functions, and results about the division of elliptic functions. When it came to the general theory for an arbitrary complex modulus, Gauss, however, abandoned the twofold structure of the real theory. There is no study of elliptic integrals with complex modulus starting from the Euler addition theorem and proceeding via the introduction of complex variables and the inversion of the integral to the double periodicity of the corresponding elliptic functions and their division equations. Nor, as we have seen, was there any extensive numerical investigation of the agm of pairs of complex numbers. Instead, the weight of the investigation was placed on generalising to the complex case the power series side of the real theory. So our first conclusion is that complex integrals were not considered an acceptable starting point for Gauss.

This raises the question of the extent to which he continued to regard elliptic integrals with a real modulus but complex end points and paths of integration as well understood. We shall discuss this point further below, when we look at his developing ideas about complex function theory.

The theory Gauss did develop was based on power series in complex variables. Gauss was certain that a viable theory of a complex variable could be based on the representation of complex numbers as points in the plane. In his *Disquisitiones arithmeticae* of 1801 he had illustrated his discoveries in number theory with that representation, for example in his theory of the ruler and compass construction constructibility of the regular 17-gon. So it is entirely natural that some of his deepest results about the transformation of the complex functions he was studying should be couched in three overlapping modes. He stressed the number-theoretic

side of his transformations, he presented them explicitly in terms of their effect on the power series, and he accompanied them with the geometrical diagram for the function q/p discussed above. Our second conclusion is therefore that Gauss went beyond a formal or purely algebraic theory of a complex variable and embraced its geometric representation. The implications this was to have for his general study of complex functions will be further discussed below.

How far then did his theory surpass that of Abel's in 1828? Gauss himself wrote to Bessel that Abel "has followed the same path that I embarked upon in 1798, so that the great coincidence of the results is not to be wondered at ..." (Gauss, 1880, 477). Indeed, in some cases even the notation was the same; for example, both denoted the value of the complete lemniscatic integral by ω . Schlesinger suggested (1912, 184) that Gauss's mostly unpublished theory looked like this by 1828:

The first third was the general theory of functions arising from the [hypergeometric series], the second the theory of the agm and the modular function, and finally the third, which Abel published before Gauss, was the theory of elliptic functions in the strict sense.

Schlesinger might have added that Gauss would have appreciated Abel's consideration of the extent to which the division equations are solvable algebraically, and the investigation of the divisibility of the lemniscate by ruler and compass would have struck him most forcefully, inspired as it was by the hint he had dropped in his *Disquisitiones arithmeticae*. He would have savoured the expansion of the elliptic functions in infinite series and infinite products but would have noted that Abel did not seem to have appreciated their full significance. Gauss would also have seen, however, that what Abel presented was only an account of elliptic functions with a real modulus.

Had Gauss chosen to comment a year later on Jacobi's *Fundamenta nova* instead, he would have seen a general theory of the transformations of elliptic functions surpassing anything he had written down. He would also have found something more like his theory of theta functions, but again only in the context of elliptic functions with real modulus. He might also have become aware of Abel's account of the same power series in Abel's (1828a). In that paper Abel also allowed the modulus to become purely imaginary, but in all respects his theory of a complex variable was, like Jacobi's, entirely formal.

It is the truly complex nature of Gauss's theory that stands out. It is not just of greater scope, it was deeper mathematically. As we discuss further below, Gauss was clear about the importance of the complex domain. Here, let us only recall the geometrical account of how the ratios of the functions $p^2 : q^2 : r^2$ depended on the complex variable t described above.

1.5.2 The Hypergeometric Series and Equation

As Schlesinger noted, it is therefore rather paradoxical that Gauss chose not to follow his own advice when he published the most fully developed of his ideas

about complex functions. Or rather, when he published part of his ideas; the resolution of this inconsistency may well, as Schlesinger suggested, lie in Gauss's habitual reluctance to publish. Gauss's published paper of 1812 deals with the hypergeometric series⁶⁰

$$F(\alpha, \beta, \gamma, x) = 1 + \left(\frac{\alpha \cdot \beta}{1 \cdot \gamma} \right) x + \left(\frac{\alpha(\alpha+1)\beta(\beta+1)}{1 \cdot 2 \cdot \gamma(\gamma+1)} \right) x^2 + \dots \quad (1.70)$$

It is, as Gauss showed, capable of representing many known functions, such as the trigonometric functions. Cases of it arise as the coefficients in the Fourier series expansion of $(a^2 + b^2 - 2ab \cos \theta)^{-n}$, so it is of practical use in astronomy.⁶¹ There are intriguing, simple relationships between what Gauss called contiguous F 's, two F 's where one value of α, β , or γ differ by 1. Finally, the value of $F(\alpha, \beta, \gamma, 1)$ depends on an interesting way on Gauss's factorial function. This is a function which satisfies $\Pi(n) = n!$ for integer n and $\Pi(z+1) = (z+1)\Pi(z)$ and is in fact related to Euler's Gamma function $\Gamma(z)$ by the simple formula $\Pi(z) = \Gamma(z+1)$.⁶² Using his newly developed theory of this function, Gauss (*Werke* 3, 150) gave an immediate proof of formulae that, he said, Euler had worked very hard to obtain, such as the formula that had intrigued Gauss for so long, $A.B = \pi/4$. Gauss still did not reveal any of his theory of the agm and elliptic functions. Moreover, throughout this, the published part of the paper, the only comment on the nature of the variable x is that it must be a complex variable of modulus less than 1 for the series to converge (nor may γ be a negative integer).

Matters are different with the part of this paper that Gauss did not publish.⁶³ The hypergeometric series satisfies this differential equation,⁶⁴ called the hypergeometric equation:

$$(x-x^2) \frac{d^2 F}{dx^2} + (\gamma - (\alpha + \beta + 1)x) \frac{dF}{dx} - \alpha\beta F = 0. \quad (1.71)$$

But conversely, which is what interested Gauss, the hypergeometric equation defines a function of a complex variable x everywhere except at the points $x = 0$, $x = 1$, and $x = \infty$. This function can be represented inside the unit circle by the hypergeometric series, but unlike the series it makes sense outside it. Like the

⁶⁰For its prehistory see Schlesinger's essay and for the later history of this important equation (Gray, 2000a).

⁶¹Gauss used it to deal with the net gravitational effect of a planet in an elliptical orbit in Gauss (1818). See the German translation and commentary by Geppert in 1927.

⁶²Legendre introduced the symbol Γ for the Gamma function in his (1811–1817, 1, 277). Euler had come across the Gamma function in his (1729).

⁶³This is Gauss (1812b), which continues (Gauss, 1812a) and is numbered in consecutive sections.

⁶⁴The observation that the hypergeometric series satisfies the hypergeometric equation had been made by Euler in his *Institutionum calculi integralis*, vol. 2, Part 1, Chaps. 8–11 and later in the paper (Euler, 1778) published posthumously.

series, the equation has a prehistory. A special case of it, with $\alpha = \beta = 1/2$ and $\gamma = 1$, is Legendre's differential equation (1.14) after the substitution $c^2 = x$, and another, $\alpha = -\beta = 1/2$ and $\gamma = 1$, is Legendre's differential equation (1.15) after the substitution $c^2 = x$. Again, like the series, there are simple connections between the hypergeometric equation and its transform under maps such as $x \mapsto 1/x$ and $x \mapsto 1 - x$.

The hypergeometric equation illuminates an understanding of Gauss's ideas about functions of a complex variable in two ways. Gauss made it clear that all solutions to the equation can be expressed as a sum of two of them, but the series expressions he had for the solutions were restricted to their domain of convergence. He put forward many solutions to it in the form $f(y)P(y)$, where $P(y)$ is a suitable hypergeometric series, the complex variable is x , $1 - x$, or $\frac{1}{x}$ and the corresponding $f(y)$ is a power of x , $1 - x$, or $\frac{1}{x}$. This exhibits the solutions to the differential equation as an analytic function valid in a neighbourhood of $x = 0$, $x = 1$, or $x = \infty$ multiplied by a term that captures the behaviour of the solution in that neighbourhood.

Although Gauss gave no special word to those three points in this context, it is clear that he appreciated their role both as setting bounds on the radius of convergence of a hypergeometric series, and in terms of their effect on the nature of the solutions to the hypergeometric equation. This is most apparent in his discussion of the following paradoxical result (his term, Sect. 55). By first setting $x = 4y - y^2$, and $\gamma = \alpha + \beta + 1/2$, when F becomes equal to $F(2\alpha, 2\beta, \alpha + \beta + 1/2, y)$, and then changing y to $1 - y$, Gauss found that his earlier results implied that

$$F\left(2\alpha, 2\beta, \alpha + \beta + \frac{1}{2}, y\right) = F\left(2\alpha, 2\beta, \alpha + \beta + \frac{1}{2}, 1 - y\right) \quad (1.72)$$

“which equation is certainly false” (*Werke* 3, 226). To resolve the paradox, he said, one has to distinguish between F as a function and F as an infinite series. The former is defined for all finite values of the variable except 0 and 1, while the latter is only defined when the variable is less than 1 in absolute value. However, when defined the series takes a unique value for each value of the variable, whereas the function does not. The false equation is in fact meaningless because as a result about series they have distinct domains of convergence, and as a result about functions the functions are many-valued. One could no more deduce a false result here, he remarked, than one could validly infer from $\arcsin(1/2) = 30^\circ$ and $\arcsin(1/2) = 150^\circ$ that $30^\circ = 150^\circ$.

What Gauss did not say was that he had another powerful reason for studying the hypergeometric series, one that derived from his deep study of the complex theory of elliptic functions. Indeed, as he wrote in 1800, the functions $y_1 = \frac{1}{M(1+x, 1-x)}$ (which we denoted by K above, see (1.58)) and $y_2 = \frac{1}{M(1, x)}$ satisfy the differential equation:

$$(x^3 - x) \frac{d^2 y}{dx^2} + (3x^2 - 1) \frac{dy}{dx} + xy = 0. \quad (1.73)$$

This is not the hypergeometric equation, but on making the transformation $x^2 = z$ it becomes the equation

$$z(1-z) \frac{d^2y}{dz^2} + (1-2z) \frac{dy}{dz} - \frac{y}{4} = 0, \quad (1.74)$$

which is the hypergeometric equation with $\alpha = \beta = 1/2, \gamma = 1$ (in fact, Legendre's equation (1.14)).⁶⁵ We can be certain Gauss knew this. Indeed, Schlesinger points out (1917, 91) that Gauss's later derivation of the hypergeometric equation for the hypergeometric series follows exactly his derivation (*Werke* 10.1, 177–180) of the power series for $\frac{1}{M(1+x, 1-x)}$ described above. Schlesinger's plausible conjecture is that Gauss's recognition of the great generality of the hypergeometric equation caused him to change the direction of his research.

What finally does this comparison of the work Gauss with that of Abel and Jacobi establish? Most simply, that there was a way. Gauss and Abel were proceeding in the same direction, as far as the theory of functions was concerned. Secondly, that Abel's theory, as also Jacobi's, could be criticised for lacking a good way of writing the new functions (such as theta series were to provide), a rigorous theory of convergence, and an explanation of double periodicity. Thirdly, that the theory of the modular function $k = k(K/K')$ and the connection with differential equations (such as Legendre's) would be a fruitful area to explore, whether or not it was also connected to the theory of transformations and modular equations. Those three points, so to speak, come from Gauss's side. Our comparison also establishes that the theory of transformations is novel, and better developed by Abel than Gauss (if not in his *Recherches* then in his *Précis*) and that it should or could perhaps be emancipated from the theory of the functions themselves (a task that was to grow in importance until finally it was solved, essentially by Dedekind, in 1877 (see Sect. 8.2.2). These observations were all to be made by nineteenth-century mathematicians, and in later chapters we shall see when and to what effect.

1.5.3 Gauss on Complex Numbers and Complex Functions

In the course of all this highly innovative and focused work Gauss naturally considered what he should or could say about complex numbers and complex functions in general. For much of the eighteenth century mathematicians had thought that there was a hierarchy of different kinds of imaginary quantities, something that Gauss (1799, 14) called “veritable shadow of shadows” (*vera umbrae umbra*). It was only gradually accepted that “generally all imaginary quantities, no matter how complicated they might be, are always reducible to the form $M + N\sqrt{-1}$ ” where M and N are real numbers, as Euler expressed it in the concluding lines

⁶⁵See Gauss's unpublished notes of 1809, *Werke* 10.1, 343 for a mention of this special case.

of his paper (1749) on the fundamental theorem of algebra. No one before 1800 considered that complex variables might enter into equations for loci, although it was generally understood that curves might sustain Bezout's theorem if one allowed that their points on intersection might be complex. This device, however, was not appreciated as a fact of geometrical significance inviting further exploration, but as an accountant's trick to balance the books.

In these respects Gauss differed little from his predecessors, but from his earliest days as a research mathematician he was more willing to promote the geometrical point of view. This is readily apparent in his remarkable discovery that the regular 17-sided polygon is constructible by ruler and compass alone. Remarkable not because it adds to the limited number of figures that are constructible in this way but because of the way it foreshadows elements of Galois theory and because it resolutely presents the non-trivial complex roots of the equation $z^{17} - 1 = 0$ as points in the plane. When the discovery was described anew in his *Disquisitiones arithmeticae* the roots are written out in their real and imaginary parts—each to ten places of decimals.

In the *Disquisitiones arithmeticae* Gauss spoke, as Euler had done, of imaginary quantities, and (see Sect. 337) following Euler used “ i for the imaginary quantity $\sqrt{-1}$ ”. He still spoke of imaginary quantities when he gave his first proof of the fundamental theorem of algebra (see below Sect. 1.5.4) in 1799, and he used that term in his work on the hypergeometric series. But in 1831 there was an interesting shift of emphasis. In the second part of his study of biquadratic residues (1832), he argued that number theory is revealed in its “entire simplicity and natural beauty” (Sect. 30) when the field of arithmetic is extended to the imaginary numbers. He explained that this meant admitting numbers of the form $a + bi$. “Such numbers”, he said, “will be called complex integers”. More precisely, he went on in the next section, the domain of complex numbers $a + bi$ contains the real numbers, for which $b = 0$ and the imaginary numbers, for which b is not zero. Then, in Sect. 32, he set out the arithmetical rules for dealing with complex numbers. We read this as a step away from the idea that i is to be understood or explained as some kind of a square root, and towards the idea that it is some kind of formal expression to be understood more algebraically.

In a report on his own work for 1831 Gauss (1831, 177) commented that “The demonstrability of the intuitive meaning of $\sqrt{-1}$ can now be completely established and more is not required for this quantity to be used in the domain of arithmetic”. Then in 1850 Gauss expressed the opinion that “imaginary quantities are not so much naturalised in mathematics as tolerated, and we are far from putting them on the same level as the real numbers. But there is no longer any ground for such an insult once the metaphysics of such quantities is put in its true light and it is shown that these have their own representational meaning, just as good as the negatives”.⁶⁶ Sadly, as Stäckel (1917, 66–67) went on to note, what we do not have is Gauss's true light on the issue, one that would take us from the issue of the representation

⁶⁶See Gauss (*Werke* 10.1, 404–405).

of complex numbers to their metaphysical nature. We can perhaps feel that what Gauss was contemplating was enlarging, if not indeed abandoning, the prevailing metaphysics of mathematics in this regard.

Instead, we can return to Gauss's ideas about the value of going complex, which he expressed in letters to his close friend Bessel in the 1810s. This also sheds light on Gauss's ideas about the meaning of a complex integral. In this connection, [Schlesinger \(1912, 152–153\)](#) noted that Gauss's discussion of the hypergeometric series, although inspired by Euler's work on it, did not follow Euler into a theory of its representation as an integral. He connected this with Gauss's earlier refusal to invert the general elliptic integral with complex modulus, and put it down to an awareness that a complex integral may well define a many-valued function of its upper end-point.

Gauss's often-cited letter to Bessel of 18 December 1811 sets out what was then a radical position.⁶⁷

Right away, if somebody wishes to introduce a new function into analysis, I will ask him to make clear if he simply wishes to use it for real quantities (real values of the argument of the function), and at the same time will regard the imaginary values of the argument as an appendage [Gauss here spoke of a ganglion, *Überbein*], or if he accedes to my principle that in the domain of quantities the imaginary $a + b\sqrt{-1} = a + bi$ must be regarded as enjoying equal rights with the real. This is not a matter of utility, rather to me analysis is an independent science which, by slighting each imaginary quantity, loses exceptionally in beauty and roundness, and in a moment all truths that otherwise would hold generally, must necessarily suffer highly tiresome restrictions.

After suggesting that he thought Bessel already agreed with him on this point, he turned to the question of the value of a complex integral and wrote:

The integral $\int \varphi x . dx$ will always have the same value along two different paths if it is never the case that $\varphi x = \infty$ in the space between the curves representing the paths. This is a beautiful theorem whose not-too-difficult proof I will give at a suitable opportunity [...]. In any case this makes it immediately clear why a function arising from an integral $\int \varphi x . dx$ can have many values for a single value of x , for one can go round a point where $\varphi x = \infty$ either not at all, or once, or several times. For example, if one defines $\log x$ by $\int \frac{dx}{x}$, starting from $x = 1$, one comes to $\log x$ either without enclosing the point $x = 0$ or by going around it once or several times; each time the constant $+2\pi i$ or $-2\pi i$ enters; so the multiple of logarithms of any number are quite clear.

Although Gauss never found the suitable opportunity, it is clear that he had obtained the first crucial insight into the integration of functions of a complex variable. Much remained to be done, and the credit for discovering and, no less importantly, publishing it, belongs with Cauchy, as we discuss in Chap. 3. Gauss's insight marks a decisive break with a purely formal theory of functions of a complex variable and a turn towards a more sophisticated, geometrical theory. Perhaps unfortunately, as we shall see in the subsequent chapters, this geometric theory had to wait some decades before being developed. Gauss did, however, publish some

⁶⁷For the whole letter see *Werke* 10.1, 365–371, these quotations come from pp. 366–367.

hints about the utility of the geometric interpretation of complex numbers in his proofs of the fundamental theorem of algebra.

1.5.4 Gauss's Proofs of the Fundamental Theorem of Algebra

In this regard, the evolution of Gauss's proofs of the fundamental theorem of algebra is interesting. The first cryptic hint at a proof can be found in an entry of his diary on October 1797 (nr. 80): "Aequationes habere radices imaginarias methodo genuina demonstratum" (Proved with an appropriate method that the equations have imaginary roots). In the course of his life Gauss gave four proofs of this result, of which the first, third and fourth employ complex methods. The first was the subject of his doctoral thesis of 1799, where he devoted more than half of its pages to criticisms of earlier attempts. As Gauss noted, serious investigations of the general polynomial equation of arbitrary degree and with real coefficients seem to have begun with Euler, who wrote to Nicholas I Bernoulli on 1 October, 1742 to suggest that such equations have as many complex roots as their degree indicates. Bernoulli was not convinced and offered a flawed counter-example. Later that year, Euler wrote to Goldbach offering a proof valid for degrees 5 and 6. This time Goldbach was unconvinced, because the restriction on the degree seemed unnatural to him. After these attempts d'Alembert produced a long paper in 1746 in which he tried to prove the general theorem, then Euler tried again in 1749, only to find his argument criticised in 1759 by a student of Lagrange, Daviet François de Foncenex, and then by Lagrange himself in 1772, and their essays spawned criticisms and improvements in turn.⁶⁸

Gauss's first criticism of d'Alembert's argument was that d'Alembert had assumed the existence of the roots and showed only that if they exist then they have to be complex. Gauss argued that d'Alembert should first have proved the roots actually exist and that he had not considered the possibility that the roots did exist but could not be manipulated like numbers; or indeed that the roots did not even exist. That said, Gauss agreed that this problem could be overcome. Gauss also criticised d'Alembert's use of infinite series in his proof and showed by means of an example that it was unsound, again politely admitting that it was perhaps capable of being re-cast in a more reliable form. This criticism notwithstanding, Gauss admitted that "the true strength of the proof seems to me not to have been weakened at all by all the objections", and he even stated that one could build a rigorous proof on that foundation (in *Werke* 3, 11).

Gauss then turned to the second of Euler's arguments, which reduced the problem to the factorisation of polynomials of degree a power of 2, it being generally agreed

⁶⁸For an account of eighteenth century attempts on the FTA including an evaluation of d'Alembert's proof and Gauss' criticism of it, see Gilain (1991) and his introduction in d'Alembert (2007).

that polynomial equations of odd degree must have a real root. If factorisation can be assured, then the fundamental theorem of algebra follows immediately by induction. Gauss observed that not only did Euler's approach tacitly assume that polynomial equations have roots, the proof of factorisation replaced the original equation with a system of quadratic equations for which there is no guarantee that solutions exist, for it produces $2m - 1$ equations in $2m - 2$ unknowns. Gauss then commented that (Lagrange, 1774) had thoroughly resolved some of the objections to Euler's argument, but gaps in the proof remained, notably the assumptions that polynomial equations have roots and that the only problem is to show that they are complex numbers.⁶⁹

Having thus swept aside all known proofs of the theorem, and considerably raised the standards by which attempts on the fundamental theorem of algebra are to be judged, Gauss then offered his own proof. He took a polynomial $f(z) = z^m + a_1 z^{m-1} + \dots + a_m$ with real coefficients, and although he had promised in Sect. 2 of his essay not to use imaginary quantities he looked for its roots in an infinite plane whose points are specified by polar coordinates r, ϕ . This is to use complex numbers in polar form without saying so. Such roots will be the common points of the equation

$$\begin{aligned} U &= r^m \cos m\phi + a_1 r^{m-1} \cos(m-1)\phi + \dots + a_{m-1} r \cos \phi + a_m = 0, \\ T &= r^m \sin m\phi + a_1 r^{m-1} \sin(m-1)\phi + \dots + a_{m-1} r \sin \phi = 0. \end{aligned}$$

These are the curves defined by the real and imaginary parts of the equation $f(z) = 0$: $\operatorname{Re} f(z) = 0$ and $\operatorname{Im} f(z) = 0$, as z varies in the complex plane. Gauss now argued that outside a suitably large circle of radius R centred on the origin each of these curves meets a concentric circle of radius $r > R$ in two disjoint sets of $2m$ distinct points, so these curves consist of $2m$ arcs going off to infinity in the plane. Moreover, when R is suitably large the z^m is so dominant that the curves $\operatorname{Re} f(z) = 0$ and $\operatorname{Im} f(z) = 0$ meet the circle of radius R alternately. Gauss now set to work to show that these curves are each made up of m disjoint "parabola-shaped" pieces (our term, not Gauss's), and so they join up inside the circle and can only do so if a curve $\operatorname{Re} f(z) = 0$ crosses a curve $\operatorname{Im} f(z) = 0$ (a continuity argument was here tacitly assumed by Gauss). At such a crossing point, the equation has a root, and so the fundamental theorem of algebra is proved.

To that effect, Gauss argued that the curves are real algebraic curves, so they consist of m pieces that, as it were, come from and go to infinity; they cannot stop, break apart, or spiral to a point in the fashion of some transcendental curves (Gauss gave the example of $y = 1/\log(x)$). As he put it, if an algebraic curve enters a bounded region of the plane, it also leaves it (Sect. 21, footnote). Nobody had

⁶⁹Remmert has aptly remarked that "the Gaussian objection against the attempts of Euler–Lagrange was invalidated as soon as Algebra was able to guarantee the existence of a splitting field for every polynomial" (in Ebbinghaus et al. 1990, 105). Remmert went on to point out that this had been already observed by Kneser (1888, 21).

raised doubts about this so far, Gauss added, but if someone requested he was “ready to provide a proof free of all doubts on another occasion”. Here he limited himself to state that his argument was based “on the principles of the geometry of position (*geometriae situs*) that are no less valid than the principles of geometry of magnitudes” (Gauss, 1799, 28). Apparently, the promised occasion to offer a rigorous proof never occurred. Gauss’s statement is true, but its proof requires topological arguments that are surely no easier to prove than the fundamental theorem of algebra itself, and to that extent Gauss’s proof is also defective. On the other hand, his argument was systematic in that it dealt with polynomial equations of any degree. Moreover, the topological nature of Gauss’s proof is attractive, and that was what Gauss saw as the heart of the matter. It was a remarkable insight for 1799, even if neither Gauss nor any one else could have provided a clear account of the completeness of the real numbers and a proper distinction between the real numbers and the rational numbers.

We pass over Gauss’s second proof (dated late 1815), which does not illuminate his understanding of complex variables, and turn to his third proof (dated January 1816), which in many ways returns to the ideas of his first. On this occasion, Gauss wanted to show that there is a root of the polynomial $f(z)$ within a circle of suitably large radius R by considering the quantity the square of the modulus of $f(z)$, which is $t^2 + u^2$, where $t = \operatorname{Re} f(z)$ and $u = \operatorname{Im} f(z)$. He introduced the derivatives t', u', t'', u'' and y , a rational function of these, and on the assumption that t and u do not simultaneously vanish, he evaluated the double integral $\int \int y dr d\varphi$ in two ways over the region $0 \leq r \leq R$, $0 \leq \varphi \leq 2\pi$. Integrating first with respect to φ taken round a circle of constant radius r yields $\int y d\varphi = \frac{tu' - t'u}{r(t^2 + u^2)}$, which is zero over the interval $0 \leq \varphi \leq 2\pi$, and therefore the double integral is zero. However, integrating first with respect to r as r goes from 0 to R leads to the integral $\int y dr = \frac{tu' + uu'}{t^2 + u^2}$ and this, Gauss showed, implies that the double integral is a non-zero positive quantity. The only way out of this apparent contradiction was to deny its fundamental assumption that the integral may be evaluated in both orders and give the same result, and therefore Gauss concluded that there were points where $t^2 + u^2 = 0$, and so t and u simultaneously vanish, and so the polynomial has a root. The proof therefore rests on the insight that when a double integral is replaced by a repeated integral the order of integration may matter when the integrand becomes infinite.

Gauss presented his fourth and last proof at a meeting of the Göttingen Gesellschaft der Wissenschaften on 16 July 1849 and published it the following year (Gauss, 1850). It was produced as a *Jubiläumschrift* on the occasion of the 50th anniversary of his first proof, which was marked by a celebration of Gauss’s distinguished career. There Gauss declined to repeat his criticisms of the eighteenth century proofs. He limited himself to remarking that he had two aims in his 1799 paper: first, to show that all the proofs produced hitherto were “unsatisfactory and illusory”, and second to produce a “new, completely rigorous proof” (Gauss, 1850, 73). After noting that Cauchy had given a new proof more recently, he explained that the form of the statement of the theorem he had given in 1799—any polynomial

with real coefficients can be split into real factors of the first and the second degree—had been chosen by him deliberately in order “to avoid any intervention of imaginary quantities. Nowadays, since the concept of complex quantities is familiar to everybody, it seems appropriate to give that form up”, and to state the theorem as the splitting of any polynomial with complex coefficients into linear factors over $\mathbb{C}$.⁷⁰ In this setting Gauss produced a proof by going back to the method he had followed in his first proof. Instead of the systems of curves $\operatorname{Re}f(z) = 0$ and $\operatorname{Im}f(z) = 0$, however, this time he considered the connected regions lying between these curves, where the functions $\operatorname{Re}f(z)$ and $\operatorname{Im}f(z)$ have constant sign. Thus, the consideration of only one system of curves, $\operatorname{Re}f(z) = 0$ say, was enough to conclude the proof. This did not prevent him from discussing the geometrical properties of the curves, and in this respect the proof was as flawed as his first one in 1799. However, as Ostrowski (1920) has shown, in the light of subsequent developments in the theory of conformal mapping and point set topology Gauss’s idea of considering regions instead of their limiting Jordan curves was revealed to be a step of essential importance. In fact, starting from Gauss’s regions one could produce a rigorous proof of the theorem, which makes the consideration of the curves within the circle $|z| = R$ completely superfluous. This, Ostrowski (1920, 9) commented, provided an unexpected confirmation of Gauss’s cryptic statement that “Essentially, the proper content of the whole argument belongs to a higher field of general, abstract theory of magnitudes” that is independent of space, and whose object are “operations on connected quantities according to continuity” (Gauss, 1850, 79). With respect to his 1799 proof, this time Gauss dealt more carefully with the case of multiple roots and showed that all roots occur in the way described.⁷¹ Eventually, he devoted the second part of the paper to the numerical evaluation of the roots of algebraic equations.

We could say in conclusions that Gauss’s proofs, especially the third, could have provoked further reflections about complex functions and their integrals, but hints, like glances across a room, are not always noticed.

1.6 Elliptic Functions, Complex Functions

The simultaneous discovery of elliptic functions by Abel and Jacobi in the late 1820s transformed Legendre’s already rich theory of elliptic integrals into a new branch of

⁷⁰Rimmert, perhaps sticking too closely to Gauss’s words, commented on this that “until 1849 all proofs, including those found in the intervening period by Cauchy, Abel, Jacobi and others, dealt with real polynomials only. It was only in his fourth proof, which is a variant of the first, that Gauss in 1849, the time now being ripe for this step, allowed arbitrary complex polynomials” (in Ebbinghaus et al. 1990, 108). As we will see in Sect. 2.5, however, contrary to this claim the theorem was stated and proved for complex polynomials already by Argand (1806, and 1814–1815) not to mention Cauchy (1821a).

⁷¹For a modern account of Gauss’s first and fourth proof, see Ostrowski (1920).

mathematics. By inverting the integrals and letting the variables become complex, they discovered numerous fascinating results that not only generated a new theory but also had implications for the solution of equations, number theory, and geometry. One can almost imagine that this dazzling display diverted mathematicians from two odd features of this work: that insofar as it was complex it was so in an entirely formal way; and that while some variables had gone complex the modulus was, for no compelling reason, still confined to being real. These oddities reflect a degree of hesitation among the mathematicians at the time over complex numbers. As Chap. 2 will describe in some detail, complex numbers and complex variables appeared in the mathematics of the eighteenth and early nineteenth centuries as artifices, aids to calculation that should not remain in the final answers. In the absence of a developed geometric theory, the only way to handle them was formally, and this formality was a comfortable place to rest for as long as mathematics was somehow essentially about real quantities.

The first great challenge of the work of Abel and Jacobi on elliptic functions was therefore that the new functions were *essentially* complex. Their complex character could not be asked to disappear without the new theory collapsing entirely. The functions had complex periods, their division properties only made sense if they took complex values. This is why elliptic functions were to be such a stimulus to the creation of complex function theory: it was quite clear that a proper elucidation of their theory would require a theory of complex functions.

The obstacles mathematicians faced in creating such a theory were many. As we shall see in Chap. 4, because the square root in the integrand of an elliptic integral made its value ambiguous—grievously so when the variables were complex—many mathematicians decided the situation was hopeless and the theory of elliptic functions would have to be re-created from scratch in some other way. There was no insight into the way a theory of functions (of some sort) from $\mathbb{C}$ to $\mathbb{C}$ could be carved out from a more general theory of maps from $\mathbb{R}^2$ to $\mathbb{R}^2$, and indeed Cauchy's own thorough delineation of a theory of continuity, integrability, and differentiability for real functions was itself a creation of the 1820s. And then, by historical accident, the man who took up the challenge of creating a theory of complex functions, Cauchy, had almost no interest in elliptic function theory, so, as we will see in Chap. 3, his many contributions were not intended to help in that area. Others had to step forward to rescue the foundations of elliptic function theory.

There could have been another history, but the man who saw further than anyone into the new domain chose to publish nothing about it. Gauss's interest in elliptic functions began as early as 1796, when he was 19, and from a deepening study of the lemniscatic integral he drew out more and more of a general theory. He defined the lemniscatic sine and cosine as complex functions of a complex variable, generalised their theory to elliptic functions with an arbitrary real modulus, discovered the connection to the hypergeometric equation, found the corresponding theta functions, and finally, perhaps in the 1820s, embarked on a general theory of elliptic functions with arbitrary periods (corresponding to a real or complex modulus). In so doing he gained insights into the distinction between a complex power series and a complex function, analytic continuation, and the integration of a complex function round

a closed path. Unlike the rich and connected account he gave himself of elliptic functions these insights into general complex functions remained scattered, but they were the first fruits that the geometric theory of complex variables was to bear. For whatever reason, all this remained hidden from contemporary view except for slender hints left in his *Disquisitiones arithmeticae* and in his proofs of the fundamental theorem of algebra.

Appendix: Transformations and Complex Multiplication

We cannot do better than follow Houzel's lead in (Houzel 1978, Sects. 8, 13) and explain in modern terms what is going on. An elliptic function, f , has two distinct periods w_1 and w_2 , say, which may both be complex and which are linearly independent over $\mathbb{R}$ (i.e. $w_1/w_2 \notin \mathbb{R}$). So :

$$f(z + w_1) = f(z) = f(z + w_2),$$

whence

$$f(z + m_1 w_1 + m_2 w_2) = f(z)$$

for all integers m_1 and m_2 . The set $\Lambda = \{m_1 w_1 + m_2 w_2 \mid m_1, m_2 \in \mathbb{Z}\}$ of points in $\mathbb{C}$ is called a lattice, and the quotient space $\mathbb{C}/\Lambda$ is a torus. Since f is periodic it defines a function, also denoted f , on the quotient space, by

$$f(z \bmod \Lambda) = f(z + \text{any element of } \Lambda) = f(z).$$

When these functions arose initially the torus was not considered, and the periods appeared as certain complete integrals. A transformation relates one elliptic function with its periods to another function with different periods, and since the periods in the classical approach were well known to be functions of the moduli, questions about periods became questions about the corresponding moduli too. The modern approach looks first at the periods, and asks, given lattices Λ and Λ' , what holomorphic functions are there $h : \mathbb{C}/\Lambda \rightarrow \mathbb{C}/\Lambda'$?

One can lift any such function to $\tilde{h} : \mathbb{C} \rightarrow \mathbb{C}$ for which $\tilde{h}(z + w) - \tilde{h}(z) \in \Lambda'$ for all $w \in \Lambda$ and for all $z \in \mathbb{C}$. So $\tilde{h}(z + w) - \tilde{h}(z) \in \Lambda'$ is independent of z , since Λ' is discrete and $\mathbb{C}$ is connected. So $\frac{d\tilde{h}}{dz}(z + w) = \frac{d\tilde{h}}{dz}(z)$ and $\frac{d\tilde{h}}{dz}(z)$ is periodic with period lattice Λ . But it is holomorphic, and now, being periodic, it is bounded, so by Liouville's theorem it is constant. So $\frac{d\tilde{h}}{dz}(z) = a$, $\tilde{h}(z) = az + b$, and by a translation of the coordinates it is enough to consider $\tilde{h}(z) = az$. To avoid triviality, suppose $a \neq 0$, and look at the sub-lattice $\tilde{h}(\Lambda)$ of Λ' . We have $\tilde{h}(\Lambda) = a\Lambda$, so $a\Lambda$ is a sub-lattice of Λ' , or, if you prefer, Λ is a sub-lattice of $\frac{1}{a}\Lambda'$. Lattices are abelian groups (abstractly they are just $\mathbb{Z} \oplus \mathbb{Z}$) so we can say Λ is a subgroup of $\frac{1}{a}\Lambda'$ of finite index, denoted n (where $n = a^2$). If we choose w'_1 and w'_2 as generators of Λ' then

$$\begin{pmatrix} w_1 \\ w_2 \end{pmatrix} = \begin{pmatrix} \alpha & \beta \\ \gamma & \delta \end{pmatrix} \begin{pmatrix} w_1 \\ w_2 \end{pmatrix},$$

where α, β, γ , and δ are integers and the determinant $\alpha\delta - \beta\gamma = n$. A suitable choice of bases in domain and codomain can always be found to diagonalise this matrix, when it becomes $\begin{pmatrix} d_1 & 0 \\ 0 & d_2 \end{pmatrix}$ where $d_1 d_2 = n$, and conversely from such a matrix it is easy to obtain a sub-lattice of Λ' of index n . Moreover, each divisor d_1 of n gives rise to d_1 distinct sub-lattices, so there are as many sub-lattices as the sum of the divisors of n , i.e. $\sigma_1(n) = \sum_{d|n} d$.

In classical language, since each sub-lattice corresponds to a transformation, there are $\sigma_1(n)$ transformations of order n . In particular, there are $p + 1$ transformations of order a prime p . The set of transformations always includes the divisions that arise by multiplying one period by n and leaving the other period fixed, but other transformations also exist.

We would expect from the modern point of view that if one looks at the ratio $\frac{w_1}{w_2} = \tau$, it must be mapped to itself, so $\tau = \frac{\gamma\tau + \delta}{\alpha\tau + \beta}$, whence $\tau = \frac{\delta - \alpha \pm \sqrt{(\alpha + \delta)^2 - 4\beta\gamma}}{2\beta}$ and $\alpha = \delta = a^2$. So occasionally lattices admit extra symmetries, which they do when τ takes particular complex values. This is the phenomenon of complex multiplication.

Chapter 2

From Real to Complex Analysis

I have obtained these values by a singular analogy based on the passages from the real to the imaginary, passages that can be considered as a means of discovery

Laplace

2.1 Introduction

On August 22, 1814 Augustin–Louis Cauchy, a young protégé of Laplace, submitted a long *Mémoire* to the Institut de France on the calculus of definite integrals that was to mark a turning point in the history of complex analysis, and a first step towards a theory of complex integration. It was in Cauchy’s hands that calculus with complex quantities began to lose the aura of mystery that had accompanied complex numbers since they had first appeared in the work of Italian algebraists Cardano and Bombelli in the Renaissance. In Chap. 37 of his *Artis magnae, sive de regulis algebraicis liber unus* (Cardano 1545) had set himself the problem of dividing 10 into two parts such that their product is 40. In an astute move he solved it formally by resorting to such “sophistic” entities as $5 + \sqrt{-15}$ and $5 - \sqrt{-15}$, and commented that such a solution is “as refined as it is useless” (*adeo est subtile, ut sit inutile*). Later on Bombelli came across such quantities when tackling the irreducible case of the cubic equation.¹ With a good deal of pragmatism in his *Opera su l’algebra* (1572) he simply stated the rules according to which one had to manipulate them without venturing on any ontological explanation of their nature.

¹This is the case when the three roots of the cubic equation (with real coefficients) are real, but Cardano’s formula gives seemingly complex answers.

Called imaginary as opposite to real quantities by Descartes, these quantities were confined to a mathematical limbo “between Being and non-Being” as Leibniz² put it, until they were rescued by eighteenth-century mathematicians, notably by Euler and d’Alembert.³ Euler echoed the old language as late as 1770 while also moving beyond it when he discussed imaginary numbers in Chap. XIII of his *Algebra*. There he noted⁴ that the square root of a negative number could be neither positive nor negative, so such numbers “must belong to an entirely distinct species of numbers”, and that “they are neither greater nor less than nothing; yet we cannot say, that they are 0”. Accordingly, because “all numbers which it is possible to conceive are either greater or less than 0, or are 0 itself In this manner we are led to the idea of numbers, which from their nature are impossible; and therefore they are usually called *imaginary quantities*, because they exist merely in the imagination”. This view gave Euler the grounds for a formal (non-geometric) theory of complex numbers, and had surely been maintained by him for many years.

Euler’s straightforward and rather modern view of complex numbers as a creation of the mind did not fit very well with a view of mathematics as the science of quantity such as d’Alembert had defended in the entry on mathematics in the *Encyclopédie*, and it is not surprising that the nature of complex quantities was one of the issues involved in the long disagreement between Euler and d’Alembert about the logarithms of negative numbers.⁵ Euler (1747) generalised the problem to defining the logarithm of complex numbers and argued that the logarithm function is the inverse function of the exponential function. Since $e^{a+ib} = e^a e^{ib} = e^a (\cos b + i \sin b)$ is periodic, log is many-valued. As he wrote to Cramer in 1746:⁶

I have finally discovered the true solution: in the same way that to one sine there correspond an infinite number of different angles I have found that it is the same with logarithms, and each number has an infinity of different logarithms, all of them imaginary unless the number is real and positive; there is only one logarithm which is real, and we regard it as its unique logarithm.

Also in the 1740s, Euler had used complex numbers and variables entirely formally in his *Introductio in analysin infinitorum* (1748a). His work led to a considerable development of the relevant techniques and to the discovery of a number of fundamental relations involving complex numbers and functions,

²“So the elegant and remarkable outcome appears in this miracle of analysis, this marvel of the world of ideas, an almost amphibian object between Being and Non-being that we call imaginary root” (*Itaque elegans et mirabile effugium reperit in illo Analyseos miraculo, idealis mundi monstro, pene inter Ens et non-Ens amphibio, quod radicem imaginariam appellamus*) (Leibniz 1702, 357).

³For d’Alembert’s life and work, see Paty (1998) and Michel and Paty (2002), which includes a complete bibliography of his works by A.-M. Chouillet.

⁴Here we follow the English translation, pp. 42–43.

⁵See Taton and Yushkevich (1980). In this book the variant transliterations Youshkevich, Youshkevitch, and Yushkevich have been standardised to Yushkevich.

⁶Quoted in (Speziali 1983, 428).

including the results $\cos v = \frac{e^{iv} + e^{-iv}}{2}$ and $\sin v = \frac{e^{iv} - e^{-iv}}{2i}$ and $e^{iv} = \cos v + i \sin v$, from which the beautiful formula $e^{i\pi} + 1 = 0$ follows immediately—but oddly enough, Euler seems never to have written this down.⁷ As a result, as far as the “nature” of imaginary numbers was concerned, eighteenth-century mathematicians could do no better than follow Euler, and use them in a largely formal, algebraic, and ungeometric manner. Nonetheless, they extensively and successfully employed complex quantities when dealing with problems in (what was then called) “mixed” mathematics, which dealt with topics such as fluid dynamics and cartography. This was done around the middle of the eighteenth century in pioneering works by d’Alembert and Euler. The latter also introduced the use of “imaginary” functions in the evaluation of real (improper) integrals. Later on, this practice was also used often by Laplace who made “the passage from real to imaginary” a powerful tool for evaluating real integrals, and considered this “remarkable artifice” as a kind of induction for discovering new results. A thoughtful, rigorous analysis of the legitimacy of such a “passage” was the starting point of Cauchy’s work in the field.

In this chapter we focus on his earliest work in this area, the *Mémoire* of 1814 (published in 1827), his famous *Cours d’analyse* (1821), and the *Résumé* (1823). These make clear that the origins of Cauchy’s work lie in investigations of problems with real integrals, most significantly, double integrals. His works on definite integrals, including singular integrals, allowed him to obtain a number of results whose full meaning and importance were to be clarified only by his subsequent research. In particular, the *Cours* also shows that for Cauchy, if not for his contemporaries, the problem of imaginaries was not to be resolved by finding the right definition of a complex number. The deep problems lay not with complex variables, but with defining *functions* of a complex variable. The nub is the multi-valued nature of many complex expressions, and it is fair to say that Cauchy never satisfactorily resolved it.

2.2 “The Passage from the Real to the Imaginary”

Cauchy’s early work in the domain of complex analysis dates from 1814 when, as a former *polytechnicien*, he began his academic career by submitting a number of papers to the Mathematical Class of the Institut de France, including a paper on the calculus of definite integrals. This involved “the passage from the real to the imaginary”, the technique which had recently been debated in papers by Laplace and Poisson. This technique, however, had been considered by mathematicians since the time of Clairaut⁸ and Euler, and we turn to their work before considering

⁷In 2005 Ivor Grattan-Guinness challenged the community of historians of mathematics to find it in Euler’s works but the search was unsuccessful.

⁸A child prodigy, Clairaut was elected to the Académie when he was 18, becoming the youngest person ever elected to that Académie. After the success of the expedition to Lapland led by

Cauchy's paper in detail. It will be considered under four headings: the investigation of complete differentials; the motion of fluids; geometrical questions including conformal mappings; and the evaluation of definite integrals.

2.2.1 “Complete” Differentials and “Imaginary” Integrals

In the opening pages of his (1739), read to the French Académie on March 4, 1739, Clairaut stated that if $Adx + Bdy$ is the differential of any function of x, y and constants, then $\frac{\partial A}{\partial y} = \frac{\partial B}{\partial x}$; and vice versa, if $\frac{\partial A}{\partial y} = \frac{\partial B}{\partial x}$ then $Adx + Bdy$ is a “complete” differential, as he called it.⁹ According to Clairaut, one can conclude “at a glance” that the theorem is true by considering the function $ax^m y^n$. As any function can be reduced into a series of such terms, he argued, the theorem being true for one it will be true for infinitely many terms. In the concluding remarks (p. 433) he claimed that his own theorem was “much more simple and natural than M. Fontaine’s”. This is a reference to the memoir *Le calcul intégral. Première méthode* on the integration of the differential equation $dx + ady = 0$ that Fontaine had presented to the French Académie in November 1738, which we know about because of the report that Clairaut and Nicole wrote about it (see [Greenberg 1995](#), 327–328). Apparently Fontaine had been able to derive the sought-for condition for the given differential form to be complete through a cumbersome use of fluxio-differential operators that made his calculations very long and rather obscure.¹⁰

Clairaut restated this theorem in a subsequent *Mémoire* (1740) on the integration (or the construction, as he called it) of ordinary differential equations of the first order. Now, in a footnote, while claiming that he had made his discovery independently he admitted that he was not the only one to have found this theorem. It had been stated, he observed, by Fontaine in a memoir the latter had presented at the French Académie the very same day on which Clairaut had read his own.¹¹ Even Euler had made the very same discovery, and had used it in his “beautiful researches on integral calculus” which, Clairaut added, were currently in press (they form [Euler](#) (1734–1735a, b)). It is worth remarking, however, that the relevant issue of the *Commentarii* of the St. Petersburg Academy was published in 1740, no less than 5 years after the actual reading of Euler’s paper at the Académie. Apparently

Mauvertuis to measure a degree of longitude, in which Clairaut took part, his fame increased quickly and he became the most authoritative French mathematician in the first half of eighteenth century. For a biography of Clairaut, see [Brunet \(1952\)](#) and for Mauvertuis, see [Terrall \(2002\)](#).

⁹[Clairaut \(1739\)](#), 427–428. In a subsequent *Mémoire* [Clairaut \(1740\)](#) used both the terms “complete” and “exact” differential.

¹⁰Fontaine’s memoir was eventually published in ([Fontaine 1764](#), 24–83). For a detailed analysis of Fontaine’s work, see [Greenberg \(1995\)](#).

¹¹See also the rapport written by [d’Alembert and De Gua \(1742\)](#) after the publication of Clairaut’s paper.

Clairaut did not know that “his” discovery, as well as the discovery of the equality of the mixed, second order partial derivatives of a function, had been previously been made by Nicolaus I Bernoulli, as Euler openly recognised in his letters.¹²

In order to prove the theorem in his 1740 *Mémoire* Clairaut first repeated his argument from 1739, calling it “a proof by induction”. He showed that it is true for the quantity $ax^m y^n$, which yields the differential form $max^{m-1}y^ndx + nax^my^{n-1}dy$ where $A = max^{m-1}y^n$ and $B = nax^my^{n-1}$, and claimed that “whatever the quantity or function of x and y ” is, “it is evident that it can be reduced to the sum of infinitely many terms of the form $ax^m y^n$ ”. Then he provided an a priori, “general” proof of the theorem by a direct computation. In the same manner, he claimed, one can also prove the converse: if $\frac{\partial A}{\partial y} = \frac{\partial B}{\partial x}$ then $Adx + Bdy$ is a complete differential.¹³ He proved it by reducing the original two-variable problem to an ordinary differential equation in one variable, and differentiating under the integral sign.

Clairaut returned to his theorem in his most famous book *Théorie de la figure de la Terre, tirée des principes de l’hydrostatique* (1743), devoted, as its title indicates, to the theory of the figure of the Earth considered as a fluid mass. There in Sect. 16 he took a step beyond his previous *Mémoires*, basing his analysis on the claim that the equilibrium of a fluid in a canal does not depend on its curvature, i.e. on the value of $y(x)$, and argued (1743, 37) that accordingly $Pdx + Qdy$ needs to be integrated “without knowing the value of x ”, in other words $Pdx + Qdy$ must be a “complete” differential. (In modern terms this means that Clairaut assumed that the integral of the differential form was independent of the path of integration.) Then Clairaut referred to his (1740, 294) to claim that $\frac{\partial P}{\partial y} = \frac{\partial Q}{\partial x}$ is a (necessary and) sufficient condition for this to be true. As was usual by that time, Clairaut supposed that $\frac{\partial P}{\partial y}$ and $\frac{\partial Q}{\partial x}$ exist and are continuous.¹⁴

D’Alembert objected to this in his (d’Alembert 1768, 13), saying that Clairaut “had stated his rule of equilibrium much too generally”. A former protégé of Clairaut, with whose support he had been elected in 1741 to the Académie as *associé astronome adjoint*, d’Alembert very soon had arguments with him on various matters of “mixed mathematics”, as physics and rational mechanics were called at that time. After 1747 their relationships deteriorated into open hostility, which prevented d’Alembert from becoming a full member of the Académie until Clairaut’s death in 1765, when d’Alembert succeeded him as *pensionnaire mécanicien*. In his (1768) d’Alembert argued that the integral of $Pdx + Qdy$ can be multi-valued just like the trigonometric functions $\arcsin x$ and $\arccos x$, and he picked up the example $\frac{ydx - xdy}{x^2 + y^2}$ mentioned by Clairaut (1740, 297), whose integral is $\arctan \frac{x}{y}$, to show that the condition above is not always sufficient. D’Alembert

¹²For a detailed analysis of Nicolaus I Bernoulli’s and Euler’s relevant papers, see Engelsman (1984).

¹³In a letter to Euler in 1743 Nicolaus I Bernoulli raised doubts about this.

¹⁴Truesdell (1954, XXI) and Greenberg (1995, 458–459) have pointed out that under certain conditions, such as domains that are not simply connected, Clairaut’s statement is not true.

concluded that not only has $Pdx + Qdy$ to be a complete differential, but also its integral “must depend on neither the rectification nor the quadrature of an oval curve.” (1768, 14). However, in these papers neither Clairaut nor d’Alembert ever related complete differentials to complex functions.

Mathematicians of the eighteenth century thought of the integral of a differential form such as $Pdx + Qdy$ as a function whose differential is the given form, whereas nowadays the expression $\int Pdx + Qdy$ is understood as a line integral. Thus, translating d’Alembert’s argument in modern terms, [Smithies \(1997, 7\)](#) boldly interpreted it as stating that the integral of $\frac{ydx - xdy}{x^2 + y^2}$ “round a circle with centre at the origin does not vanish, its value being -2π ”. It should be evident that this modern mathematical “clarification” does much to obscure the complexities in the history of these ideas.

2.2.2 The Motion of Fluids

Manipulation of complete differentials had also occurred in d’Alembert’s *Essai d’une nouvelle théorie de la résistance des fluides* (1752), in which the equations that were to become known under the names of Cauchy and Riemann appeared in print for the first time. The story related to this work is worth telling because it sheds light on the relationship between d’Alembert and Euler in the mid-eighteenth century. A Latin, manuscript version of the *Essai* ([d’Alembert 1749c](#)) was submitted by d’Alembert to the Berlin Académie in 1749 as an entry for the prize competition they had announced in 1748 for a work on “the theory of the resistance experienced by a solid body in its motion through a fluid”.¹⁵ The prize should have been awarded in 1750, but the Commission (consisting of Euler, Kies, and Grischow) charged to judge the five submitted works decided to not allocate the prize because of Euler’s opposition. They postponed their decision to 1752 when the prize was given to a certain Jakob Adami, an amateur mathematician and a protégé of Euler’s. In the meantime d’Alembert had withdrawn his paper and published it in French ([d’Alembert 1752](#)). All this led to a break in the uneasy relationship between the two scholars; their exchange of letters ceased and was not resumed until 1763.

In spite of his notoriously sharp criticism of d’Alembert’s style and work, [Truesdell \(1954, LVII\)](#) recognised that the *Essai* “is a turning point in mathematical physics”. Among its major features is the integration of differential forms involving complex variables that d’Alembert presented in Sect. III, §§ 57–60 of his *Essai*. There d’Alembert discussed the problem of determining the velocity of a fluid at a point under given conditions. The velocity components along the axes x and y are proportional to unknown functions p and q , respectively, and the problem reduces to the determination of these functions. In order to solve this problem “more easily” he began by assuming the “most simple” hypothesis

¹⁵*Nouvelle bibliothèque germanique* 4 (1748), 241.

$$dq = Mdx + Ndz, \quad dp = Ndx - Mdz.$$

As they are complete differentials, so too are

$$(M + iN)(dx - idz), \quad (M - iN)(dx + idz).$$

d’Alembert put $du = dx - idz$ and $dt = dx + idz$, $M + iN = \alpha$ and $M - iN = \beta$, so the expressions αdu and βdt are complete differentials and deduced that $u = F + x - iz$ and $t = G + x + iz$, where F and G are constants.

From this it followed that α must be a function of u , or in other words that $(M + iN)$ must be a function of $F + x - iz$, and β likewise must be a function of t , which is to say that $(M - iN)$ must be a function of $G + x + iz$, which allowed d’Alembert to determine M and N , and consequently, q and p . In Cor. I (1752, 61–62) he re-obtained this result with “a slightly simpler” method. As

$$\frac{\partial p}{\partial z} = -\frac{\partial q}{\partial x}, \quad \frac{\partial p}{\partial x} = \frac{\partial q}{\partial z}, \quad (2.1)$$

consequently, $qdx + pdz$ and $pdx - qdz$ are complete differentials, and d’Alembert could apply the same argument as before. In order that p and q be real, the constants F and G must be real. Accordingly, he determined expressions for p and q in which the imaginary quantities “destroy themselves”, as he put it. D’Alembert made no further use of the “Cauchy-Riemann” equations in the remaining part of his essay. It is also worth emphasising that d’Alembert reasoned in terms of *real* differential forms and their integrals and, consequently, that he did not connect Eq. (2.1) with any development of complex function theory. As Truesdell (1954, LV) has aptly remarked, in d’Alembert’s passage there is just “the hint of their connection with functions of a complex variable”. Accordingly, we cannot agree with Darrigol (2005, 22) who went too far in commenting that there “d’Alembert discovered the Cauchy–Riemann condition for u and $-v$ to be the real and imaginary components, respectively, of an analytic function in the complex plane, as well as a powerful method to solve Laplace’s equation $\Delta u = 0$ in two dimensions”. One could add that in this passage d’Alembert also hinted at what were to become known as complex potentials. According to Truesdell (1954, LVII), d’Alembert’s claim that these equations hold “i.e. that all flows are potential flows (...) began a controversy that lasted for a century”.

Formal manipulations of exact differentials and complex functions similar to d’Alembert’s can also be found in papers by Euler (1755) and by Lagrange (1762–1765, Sects. 20–24) on fluid dynamics,¹⁶ as well as in Euler’s papers on orthogonal trajectories and conformal mappings. Thus, for example, in (1755, §§ 70–73)

¹⁶In spite of their importance in the history of fluid dynamics (see Truesdell (1953) and Truesdell (1954)), in our view these papers by d’Alembert, Euler, and Lagrange made no substantial contribution to the development of complex function theory, contrary to a widely-held opinion. See Euler (1983b) and also the Editor’s Introduction to Euler’s *Opera Omnia* I, 19.

dealing with plane fluids Euler needed the differentials $udx + vdy$ and $udy - vdx$ to be integrable “which can be achieved with d’Alembert’s very ingenious method” (1755, 128). Accordingly, he remarked that the linear combinations

$$u(dx + idy) - iv(dx + idy) \quad \text{and} \quad u(dx - idy) + iv(dx - idy)$$

have also to be integrable, which forces

$$(u - iv)(dx + idy) \quad \text{and} \quad (u + iv)(dx - idy)$$

to be integrable. This condition is satisfied by taking $(u - iv)$ to be “any function” of $(x + iy)$, and analogously for $(u + iv)$ “any function” of $(x - iy)$. But, Euler added, such functions should be chosen in such a way that u and v be real, and “the imaginary be destroyed”. As [Truesdell \(1954, IC\)](#) has observed, in doing this, Euler succeeded in expressing a pair of conjugate harmonic functions in terms of a series of real harmonics, and the corresponding stream-line pattern, in Euler’s words, was a general equation for all the curves which can be described by the particles of the fluid under the hypothesis that $udx + vdy$ is integrable. Commenting on this in his (1755, Sect. 74) Euler stated that even though the functions of $(x \pm iy)$ were irrational or transcendental one could always expand them in power series in $(x \pm iy)$, and obtain expressions similar to the ones he had given. Thus, “while the statements regarding analytic functions in Sect. 74 are not precise, they contain the first appearance of the complex potential for a source, sink or vortex” ([Truesdell 1954, IC](#)).

D’Alembert repeated the same reasoning based on the “Cauchy–Riemann” equations (2.1) in his paper (1761b) on the laws of motion of fluids. Assuming (2.1), then $pdx + qdy$ and $pdy - qdx$ must be complete and he showed that consequently their integrals will be resp. $(\Gamma(x + iz) - \Xi(x - iz))/i$ and $\Gamma(x + iz) + \Xi(x - iz)$ with Γ and Ξ arbitrary functions and $\Gamma + \Xi$ is the stream-function. In § 12 of this paper d’Alembert considered the case of a steady flow when “the fluid moves along a line which re-enters upon itself”¹⁷ without being subject to any accelerating force. Thus $(pdx - qdz) \left(\frac{dq}{dz} - \frac{dp}{dx} \right)$ is a complete differential, but so is the first factor and so $pdx - qdz = K(b)db$, where b is a parameter that is constant on each stream-line. It follows that $\frac{dq}{dz} - \frac{dp}{dx} = f(b)$ is constant on each stream-line. “In modern terms”, [Truesdell \(1954, CXIV\)](#) commented, “d’Alembert has shown that in a steady plane circulation-preserving flow of an incompressible substance, the vorticity is constant on each stream-line. This theorem is usually attributed either to Stokes or to Helmholtz”. In addition, as $pdx - qdz = 0$ on each stream-line, there exists a function ω which is constant on each stream-line. The function ω is such that $p = \frac{d\omega}{dx}$, $q = -\frac{d\omega}{dz}$, and satisfies $d(d^2\omega/dx^2) = d(-d^2\omega/dz^2)$. Truesdell, whose

¹⁷As the editors of [d’Alembert \(2008\)](#) remark, this means that it flows along a closed curve. Quoted in ([Truesdell 1954, CXIV](#)).

account we have followed here, commented that “d’Alembert has here introduced the stream-function, usually attributed to Lagrange (1783)” which is in itself “a very important discovery” (1954, CXIV).

The “passage from the real to the imaginary” was an analytical trick he used in various contexts. Thus, for instance, in the very same year, in a paper on vibrating strings, d’Alembert (1761a) considered the case of a vibrating spring (*ressort*) fixed at one end. In this case he obtained the second order partial differential equation

$$-\frac{\partial^2 y}{\partial t^2} = \frac{\partial^2 y}{\partial x^2}, \quad (2.2)$$

which later became known as Laplace’s equation because it can be found in Laplace’s papers on the force of attraction exerted by solids of revolution, published in the 1780s (as we discuss in Sect. 7.2.2). D’Alembert was able to integrate it by putting $it = u$, thus reducing (2.2) to the more familiar form

$$\frac{\partial^2 y}{\partial u^2} = \frac{\partial^2 y}{\partial x^2}$$

whose integral $y = \Phi(x+u) + \Delta(x-u)$ he had known since 1747.¹⁸ Consequently, $y = \Phi(x+it) + \Delta(x-it)$ is the integral of (2.2). With boundary conditions that one end of the spring is fixed, d’Alembert claimed that the general solution is $y = \Phi(x+it) + \Phi(x-it)$ for an odd function Φ ; this condition makes y a real-valued function.

Lagrange (1762–1765, 498) took up d’Alembert’s reasoning on the vibrating spring in the context of fluid dynamics.¹⁹ He supposed that a homogeneous, inelastic fluid moving in a vessel of arbitrary form has reached a permanent state, called p and q the velocity components of any fluid “particle”, and x and t the coordinates of the “particle” with respect to orthogonal axes. Referring to his previous (1760–1761b, 440) he observed that p and q satisfy the equations

$$\frac{\partial p}{\partial t} + \frac{\partial q}{\partial x} = 0, \quad \frac{\partial p}{\partial x} - \frac{\partial q}{\partial t} = 0.$$

From this he obtained

$$\frac{\partial^2 p}{\partial t^2} = -\frac{\partial^2 p}{\partial x^2},$$

¹⁸See d’Alembert (1747a,b). For d’Alembert’s contributions to the theory of partial differential equations, see Demidov (1982).

¹⁹When this paper appeared in print Lagrange, a *protégé* of d’Alembert, was about to leave his native Turin to succeed Euler as the Director of the Mathematical Class of the Berlin Académie. He stayed there until 1788 when he moved to Paris, where he spent the rest of his life.

whose integral, as d'Alembert had shown, is $p = \varphi(t + ix) + \psi(t - ix)$, where φ and ψ are “any functions”. Then the equation $\frac{\partial p}{\partial x} = \frac{\partial q}{\partial t}$ yields $-iq = \varphi(t + ix) - \psi(t - ix)$.

2.2.3 Geometrical Questions

“Imaginary” functions also appeared in papers by Euler with a geometrical content. He tackled the problem of finding orthogonal trajectories to a given family of curves $f(x, y, p)$ depending on a parameter p in (1768a), which he submitted to the St. Petersburg Academy on August 18, 1768. He introduced imaginary variables and functions in Sect. X when dealing with the problem of integrating the differential forms $dx = PRdp + QSdq$ and $dy = PSdp - QRdq$, where P, Q, R, S denote functions of p and q . He now followed the route he had taken in his (1755). Assuming that

$$dx + idy = (R + iS)(Pdp - iQdq) \quad \text{and}$$

$$dx - idy = (R - iS)(Pdp + iQdq),$$

“whatever” the functions P and Q are, he argued that one can always find a multiplier M such that $Pdp \mp iQdq$ is integrable. Let $\int M(Pdp \mp iQdq) = T \pm iV$, then $R \pm iS$ can be assumed to be functions of $T \pm iV$ too, and eventually, $x + iy = \Gamma(T + iV)$ and $x - iy = \Delta(T - iV)$. From this he obtained x and y “which can always be reduced to be real whatever be the functions Γ and Δ ”. The same trick was also used by Euler later in this paper.

Some years later Euler used complex functions again in the integration of differential forms in a paper (1775a) devoted to the representation of the surface of a sphere on the plane. Submitted to the St. Petersburg Academy on September 4, 1775, this was the first of a series of three papers on cartography (Euler 1775a,b,c),²⁰ which Euler wrote possibly as a response to Lambert's (1772). Euler devoted §§ 24–51 of his (1775a) to the study of representations of the Earth on a plane that are similar in “the smallest parts” (*regiones minimae*)—the now-customary term conformal, from *projectio conformis*, was first introduced by the St. Petersburg academician F.-T. Schubert in 1789. The required condition is that there are functions $p(t, u)$ and $r(t, u)$ which make the differential forms $dx = pdu + r \cos u dt$ and $dy = rdu - p \cos u dt$ exact. In §§ 29–40 he showed how to get the general solution of these equations as a linear combination of particular solutions. Then, in order to obtain the general solution directly, in § 41 he introduced a “general method” which is essentially the same he had introduced in his earlier (1755) and (1768a). As usual, at the end (§ 44) he stated that the expressions of x and y thus obtained as functions of complex variables “could always be reduced to be real”.

²⁰See also their German translation published by Wangerin as (Euler 1898).

On the same day Euler presented a second paper (Euler 1775b) where he investigated the principles of stereographic projection of a sphere onto a plane. He began by restating the general formulae he had obtained in his previous paper. Given any point V on the sphere whose distance from the pole is v , and where t is the length of the arc of a meridian joining V with a determinate origin of the arcs, the coordinates x, y of the projection of V on the plane are given by

$$x = \Delta \left(\log \cot \left(\frac{v}{2} \right) + it \right) + \Delta \left(\log \cot \left(\frac{v}{2} \right) - it \right) \quad (2.3)$$

$$iy = \Delta \left(\log \cot \left(\frac{v}{2} \right) + it \right) - \Delta \left(\log \cot \left(\frac{v}{2} \right) - it \right), \quad (2.4)$$

where Δ is an undetermined function. Then he went on to treat Mercator’s projection in detail, by assuming that the plane of the map is tangent to the equator of the sphere and the point of projection is diametrically opposite to the point of tangency. At the end of the paper Euler asked about the form that Δ has to assume in order for it to be possible to derive the special formulae of the Mercator projection from the general formulae above. In doing this Euler came across to what were later to be known as Möbius transformations. Indeed he gave Δ the “general form”

$$\Delta = \frac{a + bz}{c + dz},$$

where $z = \tan \left(\frac{v}{2} \right) (\cos t \pm i \sin t)$. After the substitution $\tan \left(\frac{v}{2} \right) = \frac{\sin v}{1 + \cos v}$ he obtained

$$\Delta = \frac{a(1 + \cos v) + b \sin v (\cos t \pm i \sin t)}{c(1 + \cos v) + d \sin v (\cos t \pm i \sin t)}$$

which, following Euler, can be written as

$$\Delta = \frac{P \pm iQ}{R \pm iS}.$$

Accordingly, x and y could be expressed as

$$x = \frac{P + iQ}{R + iS} + \frac{P - iQ}{R - iS} \quad (2.5)$$

$$iy = \frac{P + iQ}{R + iS} - \frac{P - iQ}{R - iS} \quad (2.6)$$

or also

$$x = \frac{2PR + 2QS}{R^2 + S^2} \quad (2.7)$$

$$y = \frac{2QR - 2PS}{R^2 + S^2}. \quad (2.8)$$

Then, he substituted for P, Q, R, S their values above, and determined the conditions to be satisfied by a, b, c, d in order to re-obtain the expression for x and y he had found in the case of the Mercator projection. In doing this he found in particular that the numerator of the two expressions for y equal each other when $ad - bc = 1$.

When Lagrange turned to write about cartography in his memoir of 1779 (published 1781) he too used complex methods. His figure of the Earth was a surface of revolution obtained by rotating an arc with equation $Z = \Psi(X)$ around the Z axis. This enabled him to take account of the slight flattening of the Earth at the poles, which Lambert and Euler had not considered. Accordingly, he gave a much more general account of how the surface of the Earth can be usefully mapped onto a plane. Lagrange's coordinates were arc length along the prime meridian above or below the equator, denoted s , and the angle of longitude t . Therefore two points with coordinates (s, t) and $(s + ds, t + dt)$ on the sphere are a distance $\sqrt{ds^2 + X^2 dt^2}$ apart. The corresponding points on the plane have coordinates (x, y) and $(x + dx, y + dy)$ respectively, which are, of course, a distance $\sqrt{dx^2 + dy^2}$ apart. Now Lagrange observed rather naively that the best map would be one in which these distances were the same—which would surely make the maps infeasibly large!—but, to be completely general it was better to suppose that the line elements were proportional:

$$dx^2 + dy^2 = m^2(ds^2 + X^2 dt^2).$$

He then set $ds = X du$ and $mX = n$ and rewrote the equation as

$$dx^2 + dy^2 = n^2(du^2 + dt^2).$$

A few pages later it emerges explicitly for the first time that Lagrange recognised that n and therefore m must depend on position, and so similarity can only hold for infinitesimal figures.

To solve the equations for x and y as functions of u and t , Lagrange introduced a new variable ω defined by the equations $dx = n(\sin \omega du - \cos \omega dt)$ and $dy = n(\cos \omega du + \sin \omega dt)$. By setting $n \sin \omega = \alpha$, $n \cos \omega = \beta$ he obtained the equations

$$dx = \alpha du - \beta dt, \quad dy = \beta du + \alpha dt.$$

These equations, he said, have to be integrable and the method of d'Alembert can be applied to them. This involves introducing $\sqrt{-1}$, which we here replace with i , and arguing formally that

$$dx + idy = (\alpha + i\beta)(du + idt),$$

$$dx - idy = (\alpha - i\beta)(du - idt),$$

so $\alpha + i\beta$ must be a function f' of $(u + it)$, and $\alpha - i\beta$ must be a function F' of $(u - it)$, f' and F' denoting the differentials of “any indeterminate functions” f , and F . Thus, on integrating, Lagrange obtained

$$x + iy = f(u + it)$$

and

$$x - iy = F(u - it),$$

which easily yielded the expressions for x and y he was looking for.

Lagrange also gave the corresponding expression for m . A little more algebra, which involved writing the prime meridian (given by $t = 0$) as $x = \phi(u)$, $y = \Phi(u)$, yielded expressions²¹ for x and y in terms of $\phi(u \pm it)$ and $\Phi(u \pm it)$ and, said [Lagrange \(1781, 648\)](#), “these expressions have the advantage that the imaginaries destroy themselves”. It seems to us, therefore, that Lagrange’s work is another example of a practice in which imaginary variables play a strictly ancillary role and may appear only on condition that they vanish in the end. To describe Lagrange’s achievement, as ([Darrigol 2005](#), 163) does by saying that Lagrange “thus reduced the problem of conformal mapping to taking the real part of any regular function of a complex variable” is to modernise it too much, and the addition “This is why such functions are now called ‘conformal transformations’” is at best unsupported.

2.2.4 Definite Integrals

As for the use of complex variables in integration, a further step was taken by Euler when he was 70. In 1777 he presented a series of nine papers on this topic to the St. Petersburg Academy which, however, had little impact, if any, because they were published only posthumously between 1793 and 1805.²² In the first of these, (1777a), presented to the Academy on March 20, 1777 Euler considered the integral Δz of “any arbitrarily given” differential form Zdz . He observed that by putting $z = x + iy$ the function Z becomes $M + iN$, and correspondingly, the differential form Zdz becomes $(dx + idy)(M + iN)$ which yields $Mdx - Ndy$ and $Ndx + Mdy$, respectively, for its real and imaginary parts. These differential forms, Euler stated (1777a, 1), are “always integrable”. Indeed, “for, a known criterion of integrability will be”

$$\frac{\partial M}{\partial y} = -\frac{\partial N}{\partial x} \quad \text{and} \quad \frac{\partial N}{\partial y} = \frac{\partial M}{\partial x}.$$

Thus $P = \int (Mdx - Ndy)$ and $Q = \int (Ndx + Mdy)$. He went on to apply the substitution $z = x + iy$ to a series of examples which were particular case of the

²¹The use of the notation $\pm$ is ours.

²²See Euler *O.O.* (1) 19 and [Euler \(1983b\)](#).

differential form $\frac{z^{m-1}dz}{1+z^n}$ with $(m, n \in \mathbb{Z})$, which he showed how integrate by putting $z = x + iy$ in trigonometric form (see (§§ 43–56). He used the same substitution in the Supplement (Euler 1777b) he added to the paper on the very same day. The next day (!) he presented the paper (1777c) where he re-stated the previous results—including the remarkable properties (*insignes proprietates*) expressed by the (Cauchy–Riemann) equations above—for a function $Z(z)$ such that $Z(z) = M + iN$ when $z = x + iy$ and $Z(z) = M - iN$ when $z = x - iy$, i.e. $Z(z)$ takes real values on the real axis. However, as Yushkevich (in Euler 1983b, 257) has remarked, Euler did not foresee the role that the (Cauchy–Riemann) equations were to play in Cauchy’s and Riemann’s hands. Actually, those equations played no particular role in the remainder of the paper, which was devoted to the evaluation of integrals of the more general form $\int \frac{z^{m-1}dz}{(a \pm bz^n)^\lambda}$ by means of the substitution $z = v(\cos \theta + i \sin \theta)$. He first considered cases in which m and n are positive integers, then in the last part of the paper (§§ 13–24) he discussed in detail the most remarkable case when $\lambda = \frac{m}{n}$, for in this case the integration can be performed using logarithms and elementary trigonometric functions. In the remaining papers of the series Euler applied these results to the evaluation of various integrals by means of suitable substitutions.

Euler again used imaginary substitutions in a paper presented to the St. Petersburg Academy on April 30, 1781, which appeared in *Institutionum calculi integralis* (see Euler 1781). There he began by recalling his previous result $\int_0^\infty \frac{x^{m-1}dx}{1+x^n} = \frac{\pi}{n \sin \frac{m\pi}{n}}$, then he went on to state that he had found formulae of the same kind but involving transcendental functions in his attempts at solving the geometrical problem of finding the curve whose osculating radius r is always inversely proportional to the arc length along s of curve, such that $rs = a^2$. An easy drawing shows that the curve winds in upon itself like a spiral, say, ending after infinitely many turns in a point that Euler called the pole of the curve. In analytical terms the problem reduces to the evaluation of the integrals

$$\int_0^\infty \frac{\cos \varphi}{\sqrt{\varphi}} d\varphi = \sqrt{\frac{\pi}{2}} = \int_0^\infty \frac{\sin \varphi}{\sqrt{\varphi}} d\varphi, \quad (2.9)$$

which he found “by a lucky accident” when looking at the evaluation of the integral²³ $\Delta(n) = \int_0^\infty x^{n-1}e^{-x}dx$. A direct computation led Euler to remark that $\Delta(n) = (n-1)!$ and $\Delta(\frac{1}{2}) = \sqrt{\pi}$.

Then he made the substitution $x = ky$ ($k > 0$) to obtain $\int_0^\infty y^{n-1}e^{-ky}dy = \frac{\Delta(n)}{k^n}$, and then gave k complex values, $k = p + iq$ ($p > 0$). Writing k in trigonometric form as $f(\cos \theta + i \sin \theta)$, and splitting the resulting equations into real and imaginary parts he obtained:

²³In the notation that Legendre introduced, $\Delta(n) = \Gamma(n)$.

$$\int_0^{\infty} y^{n-1} e^{-py} \cos qy dy = \frac{\Delta(n) \cos n\theta}{f^n} \quad (2.10)$$

$$\int_0^{\infty} y^{n-1} e^{-py} \sin qy dy = \frac{\Delta(n) \sin n\theta}{f^n}. \quad (2.11)$$

As a special case, by putting $n = \frac{1}{2}$, $p = 0$, $q = 1$ and, therefore, $f = 1$ and $\tan \theta = \infty$ i.e. $\theta = \frac{\pi}{2}$ and $\cos n\theta = \frac{1}{\sqrt{2}} = \sin n\theta$ he found the Eq. (2.9). Then, assuming that p and q are positive, and $\tan \theta = \frac{q}{p}$ he obtained the “very remarkable” integral $\int_0^{\infty} \frac{dx}{x} e^{-px} \sin qx = \theta$, and eventually as a particular case ($p = 0$, $q = 1$)

$$\int_0^{\infty} \frac{\sin x}{x} dx = \frac{\pi}{2}. \quad (2.12)$$

2.2.5 Laplace and Poisson on Definite Integrals

Independently of Euler, but at almost the same time, Laplace re-obtained some of Euler’s results on integration in a paper on approximations to functions of large numbers which he presented to the French Académie in 1782 but was published only 3 years later as (Laplace 1785a). That paper arose from numerical problems dealing with probability theory, where the formulae become intractable for large numbers. Laplace was a former protégé of d’Alembert, who supported his election to the Paris Académie in 1773. In the 1780s Laplace produced his deepest and most important results in analysis, celestial mechanics, and probability theory, and these eventually made him one of the most influential French mathematicians, the veritable *patron* of French mathematics.²⁴

Among Laplace’s findings (1785a, 221–223) there was a particular case ($m = 1$) of Euler’s integral $\int_0^{\infty} \frac{x^{m-1} dx}{1+x^n} = \frac{\pi}{n \sin \frac{m\pi}{n}}$ from which he deduced what he called “the remarkable result” $\int_0^{\infty} e^{-t^2} dt = \frac{1}{2} \sqrt{\pi}$ which he had already obtained in a memoir communicated to the Académie on July 19, 1780 by a method based on the evaluation of the double integral

$$\int_0^{\infty} \int_0^{\infty} e^{-s(1+x^2)} ds dx.$$

with respect first to s then to x (Laplace 1781, 447).

²⁴For an account of Laplace’s life and work, see Fox et al. (1978).

Laplace referred to that paper as late as 1809 when he used the “remarkable artifice” of imaginary substitutions in papers which started a lively debate with Poisson. In the third section of (Laplace 1809, 193–203) he took up the problem of the “reciprocal passage of real results to imaginary results” by observing that when the results are expressed in terms of indeterminate quantities (i.e. in terms of variables) “the generality of notation” includes both the real and imaginary cases. “Analysis has taken great advantage of this extension” Laplace commented, referring to Euler’s exponential form of the trigonometric functions as a typical case in point. Then he referred to his (1785a) to justify the claim that the “passage from the real to the imaginary” could take place even when the results are expressed in terms of determinate quantities, i.e. of constant parameters. He went on to provide further applications of “this remarkable artifice” by considering the integral $\int_0^\infty x^{-\alpha} e^{ix} dx$, $0 < \alpha < 1$, where he made the substitution $x = it^{1/(1-\alpha)}$ to obtain $\frac{i^{1-\alpha}}{1-\alpha} \int_0^\infty e^{-t^{1/(1-\alpha)}} dt$. He then put $k = \int_0^\infty e^{-t^{1/(1-\alpha)}} dt$ and rewrote the integral $\int_0^\infty x^{-\alpha} e^{ix} dx$ as $\frac{i^{1-\alpha}}{1-\alpha} k$. Even though this seems to be no more than a merely formal transformation, it allowed Laplace to evaluate improper integrals. As a particular case he obtained the equality (2.12).

Then, setting $\alpha = 1/2$ he deduced $k = \int_0^\infty e^{-t^2} dt = \frac{1}{2}\sqrt{\pi}$, as he had shown in his (1781) and (1785a). From this he deduced the equality (2.9), pointing out (1809, 197) the mistake Mascheroni had made in the first volume of his (1790–1792) where it is stated that $\int_0^\infty \frac{\cos \varphi}{\sqrt{\varphi}} d\varphi = \sqrt{2\pi}$.

As $\int_0^\infty x^{-\alpha} e^{ix} dx = \int_0^\infty x^{-\alpha} \cos x dx + i \int_0^\infty x^{-\alpha} \sin x dx$, by comparing separately the real quantities and the imaginary ones, and by choosing appropriate values of $(-1)^{(1-\alpha)/2} = \cos \varphi + i \sin \varphi$, Laplace eventually obtained

$$\int_0^\infty x^{-\alpha} \cos x dx = \frac{k}{1-\alpha} \cos \varphi, \quad \int_0^\infty x^{-\alpha} \sin x dx = \frac{k}{1-\alpha} \sin \varphi,$$

where $k = \int_0^\infty e^{-t^{1/(1-\alpha)}} dt$. In the last part of the paper he applied these results to the study of an elastic lamina wound on itself in the form of a spiral.

The following year Laplace used the same analytic technique in a paper on the approximations of the formulae which are function of very large numbers that he presented to the Institut on April 9, 1810. There he commented (1810, 304) that these approximations are deduced “very simply” from the “reciprocal passage of the imaginary to the real results” as he had shown in his previous papers (Laplace 1781, 1785a, 1809). This passage, he explained, is analogous to the passage from whole, positive numbers to negative and rational numbers which allowed geometers to deduce important theorems “by induction”. If used “with reserve” it becomes a fruitful means of discovery, and more and more displays “the generality of analysis”. Thus, in the end, it was “the generality of analysis” that was used to justify the use of the artifice.

Poisson reacted to (Laplace 1810) by publishing a short summary of it (Poisson 1810, 132–136). Then, in his (1811a) Poisson commented that Laplace’s method

for evaluating integrals involving exponentials like e^{-x^2} was based on “a kind of induction” based on the “passage from the real to the imaginary quantities” and showed how Laplace’s results could be obtained by an alternative method which avoided any recourse to imaginary substitutions. This was essentially the method of changing the order of integration in double integrals which Laplace himself had used in his (1781).

In turn, in the next issue of the *Bulletin* Laplace (1811a) replied with a note in which, by using the more direct double-integral method, he re-obtained such results as

$$\int_0^\infty \frac{\cos \alpha x}{1+x^2} dx = \int_0^\infty \frac{x \sin \alpha x}{1+x^2} dx = \frac{\pi}{2} e^{-\alpha} \quad (\alpha > 0),$$

which, for $\alpha = 1$, agreed with one of his results in his (1785a, 261) obtained through imaginary substitutions. This note was included in a long paper by Laplace (1811b) which he communicated to the Institut on April 29, 1811. There he again mentioned his own results of 1785 on the evaluation of improper integrals and explained that he had obtained these values “by a singular analogy based on the passage from the real to the imaginary”. This can be considered as a “means of discovery” but, he added, although used with “great care and discretion” like induction it still requires a direct proof of the results so obtained (Laplace 1811b, 361). Then he referred to (Poisson 1811a), which had just appeared in the March issue of the *Bulletin*, where some of these integrals had been derived. Laplace’s proposal was now to obtain these and some more general results “directly”.

To this end, he considered the integral $\int_0^\infty x^{-\omega} e^{-(a+ir)x} dx$, $\omega < 1$ and $a > 0$, which Euler (1781) had also considered. In order to avoid imaginary substitutions he expanded e^{-irx} as a power series, and evaluated the integral by term-by-term integration. In the same paper, however, Laplace followed essentially the same method as in 1809 to obtain the equality (2.9), and commented that Euler had obtained it “by considering the passage from the real to the imaginary” (1811b, 365). He then evaluated some more integrals in the same way, warning the reader that, as he had stated in the introduction to his paper, the evaluation of these integrals “is not at all a simple game of analysis”, and he then applied his “analysis” to the calculus of probability. Poisson published a short summary of (Laplace 1811b) in the October issue of the *Bulletin* (see Poisson 1811b), in which he remarked that Laplace’s papers offered new examples of the passage from the real to imaginary, the kind of induction Laplace had often used in his papers and insisted on the need of more direct methods, such as those he was to provide in the next issue of the *Bulletin* (Poisson 1811c).

Laplace summarised all of these results in his celebrated book *Théorie analytique des probabilités* (1812a). There he repeated almost word-for-word what he had written in his (1811b) on the “passages from the real to the imaginary” and the need to prove the results by resorting to direct methods that will confirm “the great generality of analysis” (1812a, 88). He referred once more to his (1781) and (1785a) and added that even Euler had considered these passages “at the same time as

me” and had made “several curious applications” of them, but, Laplace concluded, somehow claiming priority, they appeared only after the publication of his own papers. As an application, after once more obtaining the equality $\int_0^\infty e^{-t^2} dt = \frac{1}{2}\sqrt{\pi}$ he again claimed (1812a, 96) that his results could be extended to imaginary t “because of the generality of analysis”.

Laplace remarked that in doing so he had been led to consider imaginary limits of integration in the integrals as if they were real, and to justify this “passage” he showed how one could obtain the same result in an alternative manner by differentiating the integral with respect to a parameter, and then by integrating the resulting integral by parts. The recourse to this technique was exactly the starting point of the long *Mémoire sur les intégrales définies* by Poisson (1813), “continuations” of which were published between 1815 and 1823. Using this method he was able to obtain some of the formulae Euler (1781) had obtained “by a kind of induction”, i.e. the questionable “passage from real to imaginary quantities”. But even Poisson allowed that this induction might be employed as a “means of discovery” whose results needed to be confirmed by “direct and rigorous” methods (Poisson 1813, 219). He did this for a number of integrals evaluated by Euler, Laplace, and Lacroix and added some new results of his own.

Poisson’s repeated criticisms should have persuaded Laplace to be more careful when dealing with “the passage from the real to the imaginary”. But in the introduction to his *Essai philosophique sur les probabilités* (1814) Laplace merely emphasised (1814, xl) that these methods had been employed first “with extreme caution” by geometers, and then “with complete confidence” because of the great number of examples that confirmed their results. Then in the second edition of his *Théorie analytique des probabilités* published that very same year he repeated that “great circumspection” was needed in the use of these “means of invention which derive from the generality of analysis” (1812a, 480). Even though he kept maintaining that the “generality of analysis” was a sufficient guarantee for the legitimacy of the discussed “passage”, he had apparently begun to think that a deeper investigation was needed in order to understand the nature of that analytical trick.

According to Belhoste (1991, 107–108), at this time Laplace “interested his protégé Cauchy in the investigation of this question”. It is also possible, as Smithies (1997, 22) has conjectured, that Laplace had become more cautious after discussing the matter with Cauchy, whose memoir on definite integrals “containing a completely new approach to the problem” was submitted to the French Institute on August 1814. This and later work by Cauchy notwithstanding, Laplace apparently remained worried about the use of the “imaginary” in analysis all his life, for in the *Supplement* to the fifth volume his *Mécanique celeste*, published posthumously from a manuscript found among his papers on the convergence of Lagrange series for expanding the “elliptic elements” in planetary orbits (see Sect. 3.3.2), he still warned once more of the “great circumspection” needed in “the passage from the real to the imaginary” (Laplace 1828, 489). By that time Cauchy had already produced the impressive amount of work on what he thought was a rigorous theory of the imaginary, to which we now turn.

2.2.6 Cauchy’s *Mémoire on Definite Integrals*

Cauchy was born in Paris as the eldest son of Louis–François Cauchy and his wife Marie–Madeleine Desestre on August 21, in the revolutionary year 1789, a few weeks after a revolutionary council had been installed in Paris and the Bastille, the symbol of the absolutism, had been attacked and destroyed by the Parisian insurgents.²⁵ During the Old Regime Cauchy’s father had served as the principal commissioner of the Lieutenant Général de Police, a rather compromising position in the eyes of the revolutionaries and which among other jobs put him in charge of controlling theatres and the book trade and looking after the state’s prisons, including the Bastille.²⁶ According to Valson, after the Revolution broke out Louis–François obtained a position of Chief of the Bureau of Almshouses and Charity Workshops, and kept it until the period of the Terror. Then, fearing for his own life and the lives of his family, he fled Paris to the country house in Arcueil near Paris that he had been able to buy with his wife’s dowry. There the Cauchy family had Laplace as a country neighbour, for he had also left Paris for the safer environment of Arcueil. During the stay there Cauchy’s first education was provided by his father, who introduced him to the classical languages, Latin and Greek without neglecting “humanities” and sciences. The Cauchy family’s hardest times came to an end with the coup d’état of 18 Brumaire (November 10, 1799), which overthrew the Directory and established Bonaparte as the First Consul. Soon thereafter Louis–François was named General Secretary of the Senate, and in this capacity he had the opportunity of meeting many times with both Chancellor of the Senate, Count Laplace, and Count Lagrange, who also was a member of the Senate.

After his move to Paris at the eve of the Revolution, Lagrange had come through the subsequent turmoil safely. Like Laplace and other scientists, he was involved in official duties such as the Commission on Weights and Measures and, to his dislike, in teaching at the newly founded École Normale Supérieure²⁷ and École Polytechnique. His lectures were published in the influential volumes (Lagrange 1797, 1798, 1801) which joined the volumes of Laplace’s *Traité de mécanique céleste* (the first two in 1799) in giving a wide picture of the achievements of eighteenth-century mathematics. Both members of the First Class of the Institut National des Sciences et des Arts, which in 1795 replaced the French Académie that had been abolished in 1793, Lagrange and Laplace rose to the height of the public fame under Napoleon, who made both of them Counts of the Empire.²⁸ Apparently they also took an interest in the young Cauchy, and Valson tells anecdotes about that. Thus, for instance, according to Valson (1868, 18) Lagrange was struck by

²⁵For biographies of Cauchy, see Valson (1868) and, better, Belhoste (1991).

²⁶On the importance of illegal books and pamphlets in Paris in these years, see Darnton (1995).

²⁷Lectures there by Laplace, Lagrange, and Monge have been published in (Dhombres 1992a).

²⁸Laplace’s political ability, or, perhaps better, opportunism, enabled him to be named a Marquis in 1817 after Napoleon’s fall and the restoration of the Bourbons.

the young Augustin's "attitude for the questions of analysis", and it seems that he, worried by the danger of this precocious attitudes, used to suggest that Mr. Cauchy prevent the young Augustin from touching any mathematics book until he had reached the age of 17. Once, referring to the 12-year-old Augustin, Lagrange told some colleagues that "this young man, well, he will replace everything we have done in mathematics".

Valson's account is an uncritical paean to Cauchy's virtues, which Valson saw as much in Cauchy's ardent Catholicism as in his mathematics, but it may be that it was following a suggestion of Lagrange's that Cauchy enrolled in the École Centrale du Pantheon in 1802 to complete his studies in the humanities. Three years later he entered the École Polytechnique, ranking second out of 293 applicants. The École Polytechnique had been founded by Monge in 1794 and was the outstanding scientific institution in France.²⁹ In 1805 it had just been converted by Napoleon to a military school and rehoused in the Collège de Navarre; Cauchy was a cadet under the command on Charles-Émile Laplace, the son of the mathematician. As a student there, Cauchy attended courses by Lacroix and Poisson in analysis, Monge and Hachette in descriptive geometry, and de Prony in mechanics. He had Ampère as a tutor in analysis and mechanics and Binet in descriptive geometry.

In 1807 Cauchy graduated third from the École Polytechnique and enrolled at the École des Ponts et Chaussées where he again proved himself to be a brilliant student. In January 1810 he completed his studies and was appointed a junior engineer and assigned to Cherbourg to work on the completion of the Port Napoleon. Very soon he became involved in the direction of the project but, in spite of his success as an engineer, he became increasingly attracted more by abstract mathematical studies than by the concrete problems of the engineers.

Already in Cherbourg Cauchy had produced interesting papers on various mathematical subjects which had drawn the attention of the Parisian mathematical milieu to him. He felt isolated in Cherbourg, and was "in a weakened and depressed condition" as his mother reported after visiting him there. Because of his poor health Cauchy was eventually allowed to return to Paris in the autumn of 1812. Less than 2 years later he wrote his first paper on analysis, the *Mémoire* on definite integrals (Cauchy 1814) which also was his first work related to complex analysis.

In its opening lines Cauchy recalled the papers by Laplace and Poisson discussed above as well as Legendre's *Exercices de calcul intégral*, the first volume of which had appeared in 1811.³⁰ As we have discussed, many of the integrals these eminent mathematicians had been able to evaluate had been obtained "by means of a kind of induction" based on "the passage from the real to the imaginary", as Cauchy (1814, 329) remarked, repeating Laplace's 1811 statement word for word. For his

²⁹See Fourcy (1828). For a historical evaluation of the role of the École Polytechnique, see Belhoste et al. (1994), Belhoste et al. (1995), and Belhoste (2003).

³⁰In this book Legendre studied integrals related to his Γ -function by means of imaginary substitutions.

part Cauchy was looking for a “direct and rigorous analysis” to establish such a “passage”.

In the first part of his *Mémoire*, “On the equations which permit the passage from the real to the imaginary”, Cauchy began by considering the equation

$$\frac{\partial}{\partial z} \left[f(y) \frac{\partial y}{\partial x} \right] = \frac{\partial}{\partial x} \left[f(y) \frac{\partial y}{\partial z} \right], \quad (2.13)$$

which could be verified, he said, “directly by a single differentiation” (Cauchy 1814, 337). It is worth remarking that Cauchy made no hypotheses about the function $f(y)$ but tacitly assumed it to be differentiable as many times as needed. Equality (2.13) is also valid when

$$y = M(x, z) + iN(x, z) \text{ and } f(y) = P' + iP''.$$

By substitution in (2.13) Cauchy obtained the equations

$$\frac{\partial S}{\partial z} = \frac{\partial U}{\partial x}, \quad \frac{\partial T}{\partial z} = \frac{\partial V}{\partial x}, \quad (2.14)$$

where S, T, U, V are the following expressions involving P', P'' and the partial derivatives of M and N :

$$S = P' \frac{\partial M}{\partial x} - P'' \frac{\partial N}{\partial x}, \quad U = P' \frac{\partial M}{\partial z} - P'' \frac{\partial N}{\partial z} \quad (2.15)$$

$$T = P' \frac{\partial N}{\partial x} + P'' \frac{\partial M}{\partial x}, \quad V = P' \frac{\partial N}{\partial z} + P'' \frac{\partial M}{\partial z}. \quad (2.16)$$

Equations (2.14)—said Cauchy (1814, 338)—“contain all the theory of the passage from the real to the imaginary”.

In the first part of the paper he showed how to use these equations. Assuming that the functions S, T, U, V are all regular (they “keep a determinate value”) within the limits of integration, he multiplied equations (2.14) by $dx dz$ and integrated them between real limits. In particular, by taking $[0, x]$ and $[0, z]$ as the intervals of integration, he obtained

$$\begin{aligned} \int_0^x S(x, z) dx - \int_0^x S(x, 0) dx &= \int_0^z U(x, z) dz - \int_0^z U(0, z) dz, \\ \int_0^x T(x, z) dx - \int_0^x T(x, 0) dx &= \int_0^z V(x, z) dz - \int_0^z V(0, z) dz. \end{aligned} \quad (2.17)$$

Then he applied this result to a number of particular cases, beginning by setting $M(x, z) = x$ and $N(x, z) = z$. Contrary to Belhoste’s (1991, 108) claim, in doing

this Cauchy did not even remark that from (2.14) one could “deduce the classical equations”

$$\frac{\partial P'}{\partial x} = \frac{\partial P''}{\partial z}, \quad \frac{\partial P'}{\partial z} = -\frac{\partial P''}{\partial x}. \quad (2.18)$$

Instead, he rewrote (2.17) in this particular case and used it to evaluate particular integrals (Cauchy 1814, 339–349), which, as he recognised, were already known to Legendre (1811–1817, 1, 262). In the next sections Cauchy applied the formulae (2.17) to other particular choices of the functions $M(x, z)$ and $N(x, z)$.

As for the “Cauchy–Riemann” equations (2.18), he was to emphasise their role in papers written many years later on the theory of the functions of geometric quantities (see Sect. 3.5.8 below). However, in a short paper on the integration of differential equations published in 1818, Cauchy (1818, 233) merely observed that “it is easy to see” that $Pdy - Qdx$ and $Pdx + Qdy$ will be complete differentials if both P and Q denote two real functions of x and y related by an equation of the form $\phi(x + yi) = P - Qi$. Actually, on this hypothesis one has

$$\frac{\partial P}{\partial y} - \frac{\partial Q}{\partial y}i = i\phi'(x + yi) = \frac{\partial P}{\partial x}i + \frac{\partial Q}{\partial x}$$

and therefore

$$\frac{\partial P}{\partial y} = \frac{\partial Q}{\partial x}, \quad \frac{\partial P}{\partial x} = -\frac{\partial Q}{\partial y}.$$

When commenting on Cauchy’s results in modern terms, and expressing them in a geometrical language, Smithies (1997, 28–29) stated that in the special case $y = M + Ni = x + zi$ Eq. (2.17) can be interpreted as expressing integrals along the edges of a rectangle. “We immediately recognise”, he claimed, “that this is just ‘Cauchy’s theorem’ for the special case of a rectangle”. In the general case $y = M(x, z) + iN(x, z)$ one can interpret the functions $u = M(x, z)$, $v = N(x, z)$ as mapping the (x, z) -plane into the (u, v) -plane, and Eq. (2.17) as expressing “Cauchy’s theorem” for a curvilinear quadrilateral in the (u, v) -plane. Even though Smithies himself recognised that Cauchy “scrupulously avoids” using any geometrical setting, nonetheless he concluded that “the form in which Cauchy has expressed his results here does, however, tend to obscure the essential features of the situation”. Admittedly, Smithies added, “to what extent Cauchy thought of these results in geometrical terms we do not know”, and “there is some evidence that in 1814 Cauchy did not yet have a clear understanding of what was going on”. Nonetheless, Smithies (1997, 29) claimed that “at that stage Cauchy is already in possession of a powerful tool that he can use to obtain many of the results for which today we should employ the full extent of Cauchy’s theorem as we know it”. This seems to be a rather misleading interpretation, because it tends to attribute to Cauchy at this early stage of his work results that he was to achieve much later, and only after deep changes in his understanding of complex functions. Indeed, “to express everything in purely analytical terms” did not merely respond to “the fashion of the time”, as

Smithies seems to have believed but, as we will see, it corresponded to Cauchy’s deepest convictions about the “nature” of complex quantities.

It is not surprising that in their report on (Cauchy 1814) to the Institut de France, Lacroix and Legendre (1814) remarked that Cauchy’s use of the imaginary in the first part of his *Mémoire* was completely in agreement with the ordinary rules of analysis.³¹ The same opinion was shared by Poisson (1814, 186) who in a rather dry review of the *Mémoire* remarked that in spite of Cauchy’s claim, all of his examples were already well known. According to Poisson, the reason for this lay without any doubt in the fact that Cauchy’s procedure, even though “very general and uniform”, was not essentially different from the ones which had been used

so far. As Lacroix and Legendre explained in their report, $\int_a^{a'} \int_b^{b'} v dx dz$ represents an area of a given curved surface whose projection on the plane x, z is a given rectangle. Cauchy considers the integrals for which the first integration with respect to either x or z could immediately be performed. “It is easy”, said Lacroix and Legendre, to find a function v satisfying this condition. It is enough to take a complete differential $pdx + qdz$ and then to take v equal to one of the members of the “equation of condition” $\frac{\partial p}{\partial z} = \frac{\partial q}{\partial x}$, as had been known since Clairaut’s times. This is a general method, Lacroix and Legendre went on, but Cauchy resorts to particular procedure. First he supposes $y = y(x, z)$, then he considers $Y = Y(y)$. The product Ydy is a complete differential which yields the required “equation of condition” for the coefficients of dx and dz . Then, by putting $y = M + iN$ the equation above splits into two further equations because of the imaginaries. Each of them can be taken as the function v . By integrating this with respect to x and to z Cauchy obtained two equations between definite integrals which provide a way to evaluate a great number of definite integrals. And this was what Cauchy actually did.

Cauchy spelled out the implications of Eqs. (2.14)–(2.17), which had escaped both Lacroix and Legendre as well as Poisson, in a number of footnotes that he added to the paper in 1825 when it was eventually presented to the Académie for publication. (The actual publication, however, was delayed until 1827.) Thus, for example, in a footnote appended to formula (2.17) he remarked that those two equations could be replaced by the “imaginary formula”

$$\begin{aligned} & \int_0^x (S(x, z) + iT(x, z)) dx - \int_0^x (S(x, 0) + iT(x, 0)) dx \\ &= \int_0^z (U(x, z) + iV(x, z)) dz - \int_0^z (U(0, z) + iT(0, z)) dz. \end{aligned} \quad (2.19)$$

³¹The report is reprinted in (Cauchy 1814, 321–327).

The same remark, Cauchy added, could be applied quite generally to all the systems he had discussed in the subsequent paragraphs. By 1825, however, Cauchy had already developed (and published) a much more profound theory of complex integration (see Sect. 3.2 below).

In the second part of the paper Cauchy discussed the possibility of inverting the order of integration of a double integral and, in both Lacroix's and Legendre's and Poisson's view, this was the most original part of the *Mémoire*. The essential discovery made by Cauchy was that when one evaluated a double integral of a function that became infinite or indeterminate for certain values of the variables within the domain of integration, one obtained two distinct and well-determined values depending on the order of integration. In this way Cauchy was led to consider "a particular kind of definite integrals" that he called singular integrals (this is the famous theory of singular integrals, which concerns the integrals of functions that become infinite, and which today is introduced as a part of real analysis), for which "the limits relative to each variable are infinitely close to each other without the integrals being zero" (Cauchy 1814, 334). A large part of his paper was devoted to their study. The results that Cauchy obtained provide a glimpse of the developments that these researches would lead to a decade later with his creation of the theory of complex integration and the theory of residues.

He first supposed that the singular value of the integrand was in a corner, then on a side of the rectangle of integration and finally, inside it. Thus, in the first case the evaluation of such an integral as

$$\int_{a'}^{a''} \int_{b'}^{b''} \frac{\partial K}{\partial z} dx dz,$$

where $K = \phi(x, z)$ becomes indeterminate for $x = a', z = b'$, yields

$$\int_{a'}^{a''} \int_{b'}^{b''} \frac{\partial K}{\partial z} dx dz = \int_{a'}^{a''} \int_{b'}^{b''} \frac{\partial K}{\partial z} dz dx + A,$$

A being the "singular" integral $A = - \int_0^\varepsilon \phi(a' + \xi, b' + \zeta) d\xi$, ε being a "very small" quantity and ξ equal to zero after the integration. As an example, Cauchy considered $K = \frac{z}{x^2 + z^2}$ and the integral $\int_0^1 \int_0^1 \frac{\partial K}{\partial z} dx dz$.

By first integrating with respect to z and then with respect to x he obtained

$$\int_0^1 \int_0^1 \frac{\partial K}{\partial z} dx dz = \int_0^1 \frac{dx}{1 + x^2} = \pi/4.$$

But, on inverting the order of integration, he found it was necessary to add the singular integral

$$A = - \int_0^\varepsilon \frac{\zeta}{\xi^2 + \zeta^2} d\xi = - \arctan \varepsilon / \zeta,$$

which for $\zeta = 0$ equals $-\pi/2$. Accordingly, the value of the integral was $\pi/4 + A = -\pi/4$.

Then Cauchy showed that for the general case, when the singular point (X, Z) is inside the rectangle of integration,³² one has

$$A = \int_0^\varepsilon [\phi(X - \xi, Z - \zeta) + \phi(X + \xi, Z - \zeta) - \phi(X - \xi, Z + \zeta) - \phi(X + \xi, Z + \zeta)] d\xi, \quad (2.20)$$

and he applied this result to the evaluation of a number of integrals.

As Cauchy had already pointed out in the introduction to his *Mémoire*, in the case of one variable the equality $\int_a^b \phi'(x) dx = \phi(b) - \phi(a)$ ceases to hold when the function is discontinuous in the interval of integration (Cauchy 1814, 332):

If, when one allows the variable to increase by insensible degrees, the function is found to pass abruptly from one value to another, the variable always being included between the limits of integration, the difference of these two values must be subtracted from the definite integral taken in the ordinary way, and each of the abrupt jumps that the resulting function can make will necessitate a correction of the same type.

In Sect. III of Part II Cauchy returned to this problem and asked how to determine the value of the definite integral $\int_{b'}^{b''} \phi'(x) dx$ once the value of the indefinite integral $\phi(x)$ is known up to a constant. If the function $\phi'(x)$ is continuous in the interval $b' \leq x \leq b''$, then the value of the integral will be $\phi(b'') - \phi(b')$, as usual. If, on the contrary, “the function $\phi'(x)$ passes abruptly from a determinate value to another value sensibly different from the first” in such a way that $\phi(Z + \zeta) - \phi(Z - \zeta) = \Delta$, ζ designating a very small quantity, then the ordinary value of the definite integral must be diminished by the quantity Δ . In a footnote (added to the paper in 1825) he made it clear that the value he was looking for with this method was what he had called (in his *Résumé*, see Sect. 2.4.1 below) the “principal value” of the integral which, in modern terms, can be written as

$$p.v. \int_{b'}^{b''} \phi'(x) dx = \lim_{\zeta \rightarrow 0} \left(\int_{b'}^{Z-\zeta} \phi'(x) dx + \int_{Z+\zeta}^{b''} \phi'(x) dx \right),$$

where Z is the point of discontinuity.³³ If the function $\phi'(x)$ has several points of discontinuity $Z', Z'', Z''', \dots$, then the value of the definite integral must be diminished by the corresponding quantities $\Delta', \Delta'', \Delta''', \dots$. As an example he considered the integral

$$\int_{-2}^4 \frac{dz}{z} = \log(4) - \log(-2) - \Delta,$$

³²Needless to say, Cauchy avoided any reference to a geometrical setting.

³³The concept of (dis)continuity Cauchy that was implicitly referring to foreshadows the one he was to define in his *Cours d'analyse* (1821a).

where $\Delta = \log(-1)$. Thus, one has

$$\int_{-2}^4 \frac{dz}{z} = \log(4) - \log(2) = \int_2^4 \frac{dz}{z},$$

and, therefore $\int_{-2}^2 \frac{dyz}{z} = 0$, which “is evident because all the elements [whose sum amounts to the integral] are two by two equal and opposite in sign” (Cauchy 1814, 404–05).³⁴

In Sect. IV of Part II he first summarised the previous results, then he returned to the equations $y = M(x, z) + iN(x, z)$ and $f(y) = P' + iP''$ considered in Sect. I, and the first term of the related equations

$$S = P' \frac{\partial M}{\partial x} - P'' \frac{\partial N}{\partial x}, \quad T = P' \frac{\partial N}{\partial x} + P'' \frac{\partial M}{\partial x},$$

and evaluated the correction term for the double integrals

$$\int_{a'}^{a''} \int_{b'}^{b''} \frac{\partial S}{\partial z} dx dz, \quad \int_{a'}^{a''} \int_{b'}^{b''} \frac{\partial T}{\partial z} dx dz,$$

by considering the case of a rational function $f(y) = \frac{G(y)}{F(y)}$ where $F(y)$ has only simple (complex) zeros. Then he remarked (1814, 413) that his formulae “become illusory” when there are multiple zeros. He promised to discuss this later, but in fact he did not do so until 1823 (see Sect. 2.4.1 below). In the final sections of the paper, as well as in the two Supplements he wrote at the request of the Commissioners Lacroix and Legendre, he applied his theory of singular integrals to a number of particular cases. Applications of this theory can also be found in his great *Mémoire couronnée* on the propagation of waves (Cauchy 1815a), which he enriched in 1824–1825 with 20 Notes amounting to the twice the number of pages in the *Mémoire* itself, and which was eventually published in 1827. He referred to his (1814) even in the long *Mémoire* that he presented to the Académie on January 2, 1815 and it remained in manuscript for some 30 years until he published it in the *Journal* of the École Polytechnique (Cauchy 1844i). There he dealt with the evaluation of definite integrals by means of double integrals. He re-obtained some results related to Lacroix’s Γ -function as well as a “direct proof” of several formulae that Laplace had deduced in (1812a) from the “passage from real to imaginary” and rigorously proved in the recent additions added to the second edition of it (Laplace 1814).

Cauchy (1814) was a research paper and it contained no rigorous theory of imaginaries. Cauchy used them according to the common mathematical practice at that time. Nor did he say anything about the geometrical interpretation of complex numbers (and variables). Even in Part II he avoided mentioning the possibility of

³⁴As we will see in Sect. 2.4.1 Cauchy changed his mind in 1823 after introducing the concept of principal value of an integral and his theory of singular integrals.

interpreting his calculus with double integrals in terms of line integrals $\int f(z)dz$ along the boundaries of a rectangle in the complex plane.

Yet, according to Cauchy’s own much later account (1849b, 175), during his stay in Cherbourg he had heard that as early as 1786 a certain Henri-Dominique Truel had found a way of representing complex numbers in a plane. The same idea was presented in the anonymous pamphlet *Essai sur une manière de représenter les quantités imaginaires dans les constructions géométriques* that appeared in Paris in 1806. Its author was Jean-Robert Argand, an amateur mathematician born in Geneva in 1768 and by 1813 living in Paris as a bookkeeper. The geometrical interpretation of complex numbers had also been proposed—indeed taken for granted—in 1797 by the surveyor Caspar Wessel in a paper published by the Danish Royal Academy, which went completely unnoticed (see [Lützen 2001a](#)).

After Cauchy’s return to Paris, a lively discussion took place in Gergonne’s *Annales* in 1813 and 1814 concerning Argand’s geometrical interpretation of complex quantities. Several mathematicians participated, including Gergonne, J. F. Français, J. F. Servois, Lacroix, and Argand himself, who was recognised to have been the author of the anonymous pamphlet and who, in 1815, published a definitive paper where his ideas were explained in detail and, as an example of their utility, provided a new proof of the fundamental theorem of algebra (see the appendix to this chapter).

Even though it seems rather unlikely, it has been maintained (in [Petrova \(1974, 259\)](#) that Cauchy was completely ignorant of this affair. Instead, he may simply have found Argand’s geometrical arguments to be insufficiently rigorous. To be sure, when he gave a proof of the same theorem ([Cauchy 1817a,b](#)) he did not mention Argand’s work. Instead, in ([Cauchy 1817b](#)) he mentioned papers by [Gauss \(1816\)](#), Lagrange and Laplace. Nor did he refer to it in his *Cours d’analyse* ([Cauchy 1821a](#)), when he set out the foundations of the theory of “imaginaries” following a completely different approach.³⁵

As for the use of complex numbers, Poinsoth in his (1818) was perhaps more to the point. In this *Mémoire*, read to the Académie on April 27, 1818 with Cauchy in attendance³⁶ Poinsoth had used “a formula for the imaginary roots of unity” in order to find the primitive roots of the congruence $x^n \equiv 1(\text{mod } \theta)$. According to ([Del Centina 2008](#)) this paper provided Sophie Germain with a new impulse to resume her work on Fermat’s Last Theorem. Poinsoth’s approach seemed very fruitful to her, and in a letter to him she wrote a couple of months later she stated that “The use of imaginary roots is a great lamp placed on the great route: it sheds light on the by-roads”.³⁷ Maybe Cauchy remembered this paper

³⁵Cauchy first referred to Argand’s geometrical interpretation and his proof of the fundamental theorem of algebra only in 1849 in his (1849b, 175). In particular, on p. 200 he referred to Argand’s paper in Gergonne’s *Annales*, adding in a footnote that he presently had under his eyes a copy of the original, anonymous 1806 pamphlet with Argand’s name handwritten on the cover page.

³⁶*Procès-verbaux des séances de l’Académie* 6 (1915), 310.

³⁷Quoted in ([Del Centina 2005](#), 63).

by Poinot when he dealt with the same problem in his lectures at the École Polytechnique.

2.3 Cauchy's *Cours d'analyse*

Cauchy began teaching at the École Polytechnique in November 1815, when he replaced Poinot. The next year he was appointed a regular professor, and he taught there until 1830 when the Bourbon monarchy was overthrown in the “July Revolution”. Cauchy had the support of such powerful men as Laplace but even more importantly the political events following Napoleon’s defeat at Waterloo and his abdication in June were to work in his favour. In July 1815 King Louis XVIII re-entered Paris and a new political era began for France. The Restoration which followed the Emperor’s fall accorded well with Cauchy’s political views. Cauchy had belonged to the *Congrégation* since 1808. This was a religious society founded in 1801 by the Jesuit Bourdier–Delpuits to bring “young people of good families” together in prayer and to unite them against atheism and secularism (Belhoste 1991, 16). Over the years, however, this religious society acquired a political role, becoming a centre of royalists, and consequently, of opposition to the government. Politically Cauchy was a legitimist, who enjoyed the fall of the Empire of Napoleon the Usurper and the Restoration of the monarchy.

The period of reaction, which followed the return of the Bourbon, also affected French scientific institutions and turned out to be decisive for Cauchy’s academic career. The École Polytechnique was reformed and demilitarised but, according to Belhoste (1991, 48), this was merely a pretext for getting rid of professors such as Monge, the founder of the École, and Lacroix, who had taken a politically active part in the Revolution and the Empire, and they were forced to resign. Among the new professors who replaced them were the conservative Cauchy and his friend Binet, who was also a member of the *Congrégation*.

For political reasons Monge was even removed from the Institut de France, after a royal ordinance had re-established the Académie des sciences in March 1816. Cauchy, who previously had been an unsuccessful candidate for vacant positions, was appointed in Monge’s place by Louis XVIII. This royal appointment was seen as an affront by the Académie, which used to choose its members through an autonomous procedure of elections. Although he opposed the tradition of that learned society, Cauchy accepted his appointment, and his insensitivity towards such a prestigious figure as Monge occasioned harsh comments. As Bertrand wrote in his *Éloges académiques* “Cauchy found few defenders. He has seen more than one friend who, though naturally tolerant and decent, turned away and refused to call him ‘brother’” (quoted in Belhoste 1991, 46–47).

In 1818 Cauchy married Aloïse de Bure, who belonged to an old family of booksellers and publishers. This turned out to be of some interest even for mathematics. After the return of the Bourbon, Aloïse’s brothers became “libraires

du Roi", and in this capacity in the 1820s they printed many of Cauchy's most famous works, including not only the *Cours d'analyse* and the *Résumé* but also some "private" publications of his such as the 1825 memoir on complex integration and the *Exercices de mathématique*.

In the course of the 1820s Cauchy combined his teaching at the École with a professorship at the Faculté des sciences where he substituted for Poisson. From 1824 to 1830 he also taught at the Collège de France. These academic positions and his outstanding mathematical qualities together established Cauchy in less than 10 years as the leading figure of mathematics in France—indeed in Abel's words, as "the mathematician who knows how mathematics must be done" (Abel 1902, 45–46).

As Belhoste (1991, 113) has remarked, once Cauchy was absorbed in his teaching duties between 1817 and 1821 his mathematical productivity declined sharply. All in all, in this period he submitted six papers to the Académie, and none at all between November 1819 and October 1821 when he was working on his *Cours d'analyse*. As was usual for the professors of the École Polytechnique, Cauchy had indeed been required by the *Conseil de Perfectionnement* of the École to provide his students with a textbook of his course. Accordingly, in 1821 he published the *Cours d'analyse* (Cauchy 1821a), devoted to algebraic analysis,³⁸ followed 2 years later by the *Résumé des leçons sur le calcul infinitésimal* (1823a), where the differential and integral calculus was presented. Cauchy took the opportunity of stating his methodological views in the introduction to the *Cours* where he openly advocated the need of ever-greater rigour in analysis (Cauchy 1821a, ii-iii):

As for methods, I have sought to give them all the rigour that one demands in geometry, in such a way as never to revert to reasoning drawn from the generality of algebra. Reasoning of this kind, although commonly admitted, particularly in the passage from convergent to divergent series and from real quantities to imaginary expressions, can, it seems to me, only occasionally be considered as inductions suitable for presenting the truth, since they accord so little with the precision so esteemed in the mathematical sciences. We must at the same time observe that they tend to attribute an indefinite extension to algebraic formulas, whereas in reality the larger part of these formulas exist only under certain conditions and for certain values of the quantities that they contain. In determining these conditions and these values, I have abolished all uncertainty.

Cauchy's criticism was mainly directed against Lagrange's algebraic approach to the foundations of the calculus, which was based on the expansion of any function in power series, as the latter had expounded it in the *Théorie des fonctions analytiques* (1797). In the opening lines of his introduction Cauchy acknowledged his debt to Laplace and Poisson, who had supported him in the beginning of his academic career. However, an attentive reader could recognise that Cauchy's critical remarks on the recourse to the "generality of algebra" to justify the passage from convergent to divergent series, and from real quantities to complex ones, could also be aimed at the conceptions they had expressed many times in their writings.

³⁸For a detailed study of Cauchy's book, see Bottazzini (1992a).

The *Cours* is divided into two parts. Looking at the structure of Cauchy's book one immediately realises that the binomial theorem and the related expansions in series represent the keystone of the first part, devoted to real analysis. He presented the proof of the binomial theorem at the end of Chap. VI as the first of three Problems, the second and the third ones being the power series expansions of A^x and $\log(1+x)$, respectively (Cauchy 1821a, 146–152). With this in mind one can better understand the role of the concepts Cauchy introduced throughout his *Cours*, starting with the definition of a continuous function. This crucial concept is related to several of the main results Cauchy presented in the whole of his mathematical work.

Apparently Cauchy first introduced his definition of a continuous function in 1817 in his lectures to the first-year students of the École Polytechnique. In the printed version of these lectures, he first defined [Def 1] continuity for a single-valued function $f(x)$ in a given interval (“between two given limits”) when “for every value of x between these limits, the numerical value (i.e. the absolute value, as we would say nowadays) of the difference $f(x+\alpha) - f(x)$ decreases indefinitely with that of α ” (1821a, 43). Then he reformulated it in terms of infinitesimals [Def 2] stating that “the function $f(x)$ will remain continuous with respect to x within the given limits if, within these limits, an infinitely small increase of the variable always produces an infinitely small increase of the function itself” (Cauchy 1821a, 43). (Cauchy had earlier defined an infinitesimal as a variable whose “numerical value” decreases below any given number, that is, as a variable which has zero as its limit). The condition that the function is single-valued is essential. In Cauchy's opinion in the *Résumé*, functions like $\sqrt{x}$ “become discontinuous in passing from the real to the imaginary as the variable x decreases and passes through zero” (Cauchy 1823a, 39). The meaning of Cauchy's statement becomes clearer when one observes that at the point $x = 0$ this function has a branch-point. He was to re-state his opinion in a letter to Coriolis published in the *Comptes rendus* of the French Académie in 1837 (see below Sect. 3.4), when he pointed out (Cauchy 1837a, 39) that “a function that does not become infinite in general only ceases to be continuous by becoming multiple”. Then he extended the definition of continuity to single-valued functions of two, and several variables $f(x, y, z, \dots)$. Such a function was called continuous if it was a continuous function with respect to each variable separately.

Cauchy solved functional equations involving continuous functions and proved his celebrated, questionable theorem that the sum of a convergent series of continuous functions is continuous.³⁹ Cauchy first needed (a particular case of) it to establish that the sum of a convergent power series is a continuous function (1821a, 144–145). Then he proved a uniqueness theorem for the expansion of a continuous function in power series by a direct inspection of the coefficients. Had been this the sole application, the theorem could have been stated for the safe domain of

³⁹For a discussion of Cauchy's concept of continuity and the theorem just mentioned, see Bottazzini (1992a), lxxxi–xcv.

power series only. Instead, both these results were preliminary to his proof of the binomial expansion, and the related expansions of e^x and $\log(1+x)$ in series, where he made an essential use of his questionable theorem. In order to prove the binomial expansion (1821a, 146–147) he used that theorem to establish the continuity of the function $\Phi(\mu)$ defined by the series

$$\Phi(\mu) = 1 + \mu x + \frac{\mu(\mu-1)}{2!}x^2 + \dots + \frac{\mu(\mu-1)\dots(\mu-n+1)}{n!}x^n + \dots, \quad (2.21)$$

which is convergent when $|x| < 1$. Then, by applying a corollary to a previously stated theorem⁴⁰ he obtained the following functional equation:

$$\Phi(\mu)\Phi(\mu') = \Phi(\mu + \mu'), \quad (2.22)$$

which he had already solved in Chap. V (1821a, 110–112) under the hypothesis that the function $\Phi(\mu)$ is continuous. The solution of (2.22) is given by

$$\Phi(\mu) = [\Phi(1)]^\mu = (1+x)^\mu, \quad (2.23)$$

which, together with (2.21), proves the binomial theorem for any real exponent μ .⁴¹

The expansion of e^x in series was obtained by Cauchy in two ways. He first derived it from (2.23) in the manner of Euler by substituting $1/\alpha$ for μ and αx for x , where α is an infinitesimal, and then calculating the limit for indefinitely decreasing values of $|\alpha|$. Then he re-obtained it by a direct application of the functional method, determining the function represented by the series

$$\Phi(x) = 1 + \frac{x}{1} + \frac{x^2}{1.2} + \frac{x^3}{1.2.3} + \dots$$

and satisfying Eq. (2.23). As a corollary he eventually obtained the expansion of $\log(1+x)$.

Looking from this point back at the plan of the *Cours*, one can better appreciate the remarkable connections intertwining the basic concepts and results presented by Cauchy, from the infinitesimals and the continuity up to the solutions of functional equations, the convergence of series (including in particular his “theorem”) and eventually the theorems concerning power series. An analogous structure can be recognised in the second part of the *Cours*, devoted to complex algebraic analysis.

Historians have slighted the second part of Cauchy's *Cours* while paying great attention to the first part of it. This corresponds to the exceedingly large amount of attention they have paid to real analysis and its foundations, by comparison with complex analysis. When considering Cauchy's work, such an unbalanced attitude

⁴⁰There is a misprint in (1821a, 147). The correct reference is to Theorem 4, Cor. 4.

⁴¹The proof of the binomial theorem for any complex value μ was to be given by Abel (1826b). It was in this paper that Abel first pointed out that Cauchy's statement of his theorem about the limit of a series of continuous functions “admits exceptions”.

is particularly regrettable for it prevents one from understanding some of its major mathematical features.

Chapters VII–XII of the *Cours* contain what Cauchy called “a new theory of the imaginaries”. Imaginary numbers were introduced by Cauchy in a purely formal manner as “symbolical expressions” such as $\alpha + i\beta$, where α and β are real quantities (1821a, 153–155). This way of thinking of imaginaries was deeply rooted in the mathematical tradition. However, Cauchy took the ontological problem concerning imaginaries much more seriously than anybody else had done before him. Assuming, as he did, that a symbolical expression as such does not signify anything, one has to define mathematical concepts and operations for these “expressions”. It took him no less than 50 pages to define algebraic operations on imaginary expressions in a rigorous way, and to establish their properties. All of what Cauchy presented in Chap. VII of the *Cours d’analyse* is today a matter of routine for undergraduate students, and a superficial reader might think that the same was true in Cauchy’s day. On the contrary, this was not the case; this chapter should be considered as one of the places where Cauchy best displayed his concept of rigour.

One of the Cauchy’s major concerns was the treatment of the multi-valuedness occurring in the rational powers of imaginary numbers. His way of dealing with this was beyond comparison in the contemporary mathematical literature—for instance, his discussion of the roots of unity alone required some 20 pages! In the early decade of the nineteenth century multi-valuedness had been the subject of a long discussion raised by Poisson in relation to the generalisation of the expansions⁴²

$$(2\cos x)^m = \sum_{k=0}^m \frac{m!}{(m-k)!k!} \cos(m-2k)x, \quad (2.24)$$

$$0 = \sum_{k=0}^m \frac{m!}{(m-k)!k!} \sin(m-2k)x \quad (2.25)$$

from positive integral m to negative or rational m . The trigonometric series arising from Eq. (2.24) in this case had been considered by Euler in various papers⁴³ as well as by Lagrange. In the 11th of his *Leçons Lagrange* (1801, 118–119) had been pleased to remark that “one of the principal advantages of derived functions is to make powers and radicals disappear from equations”. Given for instance $y = \cos^m x$, by differentiating it Lagrange obtained

$$y' = -m \cos^{m-1} x \sin x$$

and, by division

⁴²These are obtained by taking real and imaginary parts of the binomial expansion of $(2\cos x)^m = (e^{ix} + e^{-ix})^m$.

⁴³See Burkhardt (1914–1915) and Jahnke (1987).

$$\frac{y'}{y} = -m \frac{\sin x}{\cos x}, \quad (2.26)$$

where the power $\cos^m x$ has disappeared.

Then Lagrange supposed that y could be expanded in series of cosines of multiple arcs as

$$y = A \cos mx + B \cos(m-1)x + C \cos(m-2)x + \dots$$

By substituting this into Eq. (2.26), Lagrange obtained the following recursive expressions for the coefficients $B, C, D, E, \dots$:

$$B = 0, C = mA, D = 0, E = \frac{m(m-1)}{1 \cdot 2} A, \dots$$

The condition $x = 0$ yields $A = 2^m$.

Lagrange thought he had solved the problem in its full generality, for any (real, rational, or irrational) m , and he was pleased to remark at this point: "I believe it was necessary to enter into this detail above all to show that, if analysis appears sometimes to err, it is always from a failure to envisage a sufficiently extended way of treating it with all the generality of which it is capable" (Lagrange 1801, 128).

A survey of the results obtained until then, including those of Lagrange, was given by Lacroix in the Introduction to his *Traité* (1810, 76–93, 274–275). The following year, however, Lagrange's over-optimistic opinion was to be contradicted by a counter-example that his former pupil Poisson obtained by considering the case $m = 1/3$ and $x = \pi$. In fact, from $2^m \cos^m x$ one obtains $2^{1/3}(-1)^{1/3}$ and therefore, according to De Moivre's formula, three complex values, different from each other:

$$2^{1/3} \frac{(1+i\sqrt{3})}{2}, -2^{1/3}, 2^{1/3} \frac{(1-i\sqrt{3})}{2}.$$

The corresponding development in series of $2^m \cos^m x$ according to (2.24) gives yet another different value: $\frac{1}{2} (2^{1/3})$, which is the arithmetic mean of the first and third of the previous values.

The way Poisson (1811d) suggested of avoiding the contradiction was inspired by Euler. In his paper (1753) Euler had derived Eq. (2.24) by expanding $2^m \cos^m x$ as

$$\begin{aligned} (2 \cos x)^m &= \sum_{k=0}^{\infty} \frac{m!}{(m-k)!k!} \cos(m-2k)x \\ &\pm i \sum_{k=0}^{\infty} \frac{m!}{(m-k)!k!} \sin(m-2k)x = X \pm iY \end{aligned} \quad (2.27)$$

as Poisson wrote it. He then argued that when m is an integer, one could obtain the expansions (2.24) from (2.27) by simple algebra. When m is a rational number, but not an integer, $2^m \cos^m x$ is a multi-valued function and $X + iY, X - iY$ are two

branches of it. Therefore, Poisson concluded that X did not represent the “general” expansion of $2^m \cos^m x$, as Lagrange thought, but only the real part of it.

Even though Poisson had convincingly pointed to Lagrange’s mistake, he was not able to clarify where and why Lagrange was wrong. Moreover, the question of finding the sums of the series was left open. In the following decades the problem was tackled by a number of mathematicians, including Abel in his (1826b).⁴⁴ In the *Corrections and Additions* appended to vol. 3 of his *Traité* Lacroix listed Poisson’s example among the paradoxes of the calculus that were waiting for “a particular interpretation” (Lacroix 1819, 605–611, 616–623). When the final volume of Lacroix’s *Traité* appeared in print, Cauchy was working on the edition of his *Cours*. So the troubles mathematicians faced when dealing with multi-valued functions such as $2^m \cos^m x$ (m rational) as well as the controversies to which they gave rise seem very likely to have played a role in convincing Cauchy of the need to establish the concept of single-valued function rigorously. In the case of such multi-valued functions like $\arcsin x$, $\arccos x$, and so on, he also introduced the special symbol $(())$ for denoting any of its values (Cauchy 1821a, 21–25).

In Chap. VIII of his *Cours*, Cauchy introduced imaginary variables and imaginary functions. After defining the concepts of imaginary variable and that of limit, Cauchy explicitly remarked (1821a, 204) that

when the constants or variables contained in a given function, having been considered real, are then supposed to be imaginary, *the notation with which one expresses the function concerned can only be conserved in the calculation by means of new conventions appropriate to fix the sense of this notation on the latter hypothesis* [our emphasis].

This is a crucial point which explains the way in which Cauchy introduced complex functions of complex variables. In fact, before doing so, and consistently with his basic assumptions in Chap. VII, he had rigorously established the meaning in the “imaginary” case of the notations he had used in the real case. According to previously established conventions, he could easily define rational functions of an imaginary variable as well as the power x^a (a being a rational number). But these conventions “did not yet suffice to fix the meaning of the expressions A^x , $\log x$, $\sin x$, $\cos x$, $\arcsin x$, $\arccos x$ in the case where the variable x becomes imaginary” (1821a, 209).

According to Cauchy, imaginary series provided the “easier” way to perform the task and, therefore, their introduction had to be postponed until the theory of the series with imaginary terms was developed in Chap. IX. Under these limitations he introduced the concept of what he called an imaginary function of one (or several) real variable(s) (1821a, 209–210). In modern terms, Cauchy here introduced the concept of a function $f: \mathbb{R} \rightarrow \mathbb{C}$. For these functions alone he defined the concept of continuity according to the definition he had previously introduced for real functions of real variables. He also sketched the extension to imaginary functions of the definitions and of the results that he had presented in Chaps. III–V of his *Cours*,

⁴⁴For a detailed historical account, see Burkhardt (1914–1915, 837–856).

including the solutions of such functional equations as $f(x+y) = f(x) + f(y)$ and $f(x+y) = f(x)f(y)$.⁴⁵ As he had done in the real case, he first considered the solution for rational values of the variable, then he extended it to the irrational ones by making an essential use of the hypothesis of continuity of the solution (1821a, 220–229). In this regard, it is essential to keep in mind that the solutions that he was looking for were given by imaginary functions $f(x)$ of a single real variable x .

Chapter IX played a crucial role in the second part of the *Cours*, just as Chap. VI had done in the first part, where he had dealt with infinite series and proved the binomial theorem. Here Cauchy (1821a, 274) introduced the concept of imaginary series, and extended the concept of convergence to them in the obvious way. Then, by considering the pair of real series $\sum z^n \cos n\theta$, $\sum z^n \sin n\theta$, he proved that the geometric series with imaginary terms $\sum z^n (\cos n\theta + i \sin n\theta)$ is convergent when $|z| < 1$ and in a concluding remark he added that its sum is a continuous function of z in the given interval, by appealing to his theorem on the series of continuous functions. Generalising this remark, at this point he stated the companion of his celebrated “theorem” in the case of imaginary series. More precisely, he stated it for series $\sum (p_n + iq_n)$ of imaginary, continuous functions of a real variable z (1821a, 234).

The convergence tests and the theorems on power series could easily be established for imaginary series taking into account the moduli $\rho_n = (p_n^2 + q_n^2)^{1/2}$ (Cauchy 1821a, 234–242). In particular, he stated the Cauchy-(Hadamard) theorem that the series $\sum a_n x^n = \sum a_n z^n (\cos n\theta + i \sin n\theta)$ is convergent for all the values of z such that $|z| < 1/A$ where $|A|$ is the greatest of the limits to which $|a_n|^{1/n}$ converges as $n \rightarrow \infty$. So the series is convergent or divergent according as $|Az| < 1$ or $|Az| > 1$. This allowed him to extend his proof of the binomial theorem to imaginary variables $x = z(\cos \theta + i \sin \theta)$, $|z| < 1$ and real exponents μ .⁴⁶ In doing so Cauchy had to tackle the delicate question of the multi-valuedness of (rational or irrational) powers of imaginary expressions. He had discussed the matter thoroughly in Chap. VII of his *Cours*. There, after defining the meaning one had to attribute to $(\alpha + \beta i)^a$, when a is a rational number (Cauchy 1821a, 186–194), he extended it “by analogy” to irrational values of a (under the hypothesis $\alpha > 0$) when, on the contrary, “one cannot see, even on supposing that the numerical value of a is fractional, what the value of the expression $((\alpha + i\beta))^a$ is that one can distinguish from the others and denote by the notation $(\alpha + i\beta)^{a''}$ ” (Cauchy 1821a, 195).

All this was essential to Cauchy's proof. In fact, under the stated condition on z , $1 + z \cos \theta$ is positive and he could rigorously define the meaning of the expression $(1 + z(\cos \theta + i \sin \theta))^\mu$ involved in the proof. He treated the exponential and logarithmic series in an analogous way.

At this point Cauchy could answer the still-open question of giving a definite meaning to the expressions A^x , $\sin x$, and $\cos x$ for imaginary values of x . This

⁴⁵Discussed in (Dhombres 1992b).

⁴⁶For a detailed discussion of Cauchy's proof of the binomial theorem, see Dhombres (1986), 157–163.

was provided by their expansions in convergent series (Cauchy 1821a, 256–257). The definition of the corresponding inverse functions $\log x$, $\arcsin x$, $\arccos x$ was discussed in the final part of the chapter.

Thus, by following a rigorous if cumbersome procedure Cauchy was able to define algebraic and elementary transcendental functions of a complex variable. One could imagine extending his procedure “by analogy” to any real function $f(x)$ represented by a convergent power series $\sum a_n x^n$ by considering the imaginary function $f(x + iy)$ as defined by the corresponding imaginary series $\sum_n a_n (x + iy)^n$, provided that it was convergent, and indeed Cauchy occasionally referred to this as late as 1846 (see Cauchy 1846c, 75). In his *Cours*, however, Cauchy kept silent about this as well as about the possibility of giving a general definition of the concept of an imaginary function of an imaginary variable. This was to remain as an unanswered question in the background of his mathematical research for many years to come.

Eventually, in Chap. X (1821a, 274–281) he gave a proof of the fundamental theorem of algebra along the same lines as the proof he had published in 1820 in a paper in the *Journal* of the École Polytechnique (Cauchy 1820), see the Appendix at the end of this chapter. Cauchy based it on a continuity argument that he took to be evident. Denoting by $F(u, v)$ the square of the modulus of the polynomial $f(x) = \phi(u, v) + i\chi(u, v)$, it follows that $F(u, v)$ is a continuous function of u and v . According to Cauchy (1821a, 278), “it is clear that $F(u, v) \dots$ cannot fall below zero and will attain one or more times a certain lower limit which it cannot never go beyond.”⁴⁷ Then by *reductio ad absurdum* he concluded that this lower limit must be zero. The corresponding value of $x = u + iv$ is the required root of the polynomial equation $f(x) = 0$. As a corollary of the theorem, he proved that a polynomial of degree n in $\mathbb{C}$ can be split into the product of n linear factors, and in the next chapter he applied this to splitting the rational functions into simple fractions. This was the same problem, which is preliminary to the evaluation of integrals of rational functions, that had motivated d’Alembert in 1746 to give the first proof of the fundamental theorem of algebra.

2.4 Complex Functions and Integrals

The problem of extending the definition of a real function $f(x)$ to a complex function $f : \mathbb{C} \rightarrow \mathbb{C}$ was occasionally studied by Cauchy in 1821 in connection with “a calculating trick” related to the evaluation of indeterminate forms of multiple integrals. He did this in the second of the two-part paper (Cauchy 1821b) that he presented to the Académie on October 8, 1821⁴⁸ and then quickly published in the

⁴⁷Cauchy’s confusion between minimum and lower bound was shared by everyone at that time, with the possible exception of Bolzano.

⁴⁸*Procès-verbaux des séances de l’Académie* 7 (1916), 231.

Bulletin de la Société Philomatique. There Cauchy tackled two problems, the first being the presentation of the solutions of linear partial differential equations in a form more suitable for applications to physical matters, such as the propagation of heat and the motion of waves, which had been treated, as he noted, in papers by Fourier, Poisson, and himself.⁴⁹ We are interested in the second part dealing with possible simplifications of the integrals involved in some particular cases. At the end Cauchy (1821b, 272) considered the Laplace equation

$$\frac{\partial^2 \phi}{\partial t^2} + \frac{\partial^2 \phi}{\partial x^2} = 0 \quad (2.28)$$

with the initial conditions $\phi = f_0(x)$, $\frac{\partial \phi}{\partial t} = f_1(x)$ at $t = 0$. By using Fourier transform methods he formally obtained its “general integral” as a complicated sum of two integrals, each of which appeared to be indeterminate. After introducing a convergence factor for the integrals and some further work with Fourier transforms, Cauchy claimed that one could obtain the following expression for the function ϕ :

$$\phi = \frac{f_0(x+it) + f_0(x-it)}{2} + \int \frac{f_1(x+it) + f_1(x-it)}{2} dx. \quad (2.29)$$

Then, by putting $f_0(x) = f(x)$ and $f_1(x) = if'(x)$, and after yet more calculations involving Fourier transforms, he obtained an expression, about which he said that it could be considered “as serving to define the function $f(x+it)$ when one knows the function $f(x)$ ” (1821b, 275).

Eventually, in a concluding *Post Scriptum* he referred to his new memoir (Cauchy 1822a), which he had just read to the Académie⁵⁰ on January 22, 1822. There he had pointed out that different functions could be associated to the same expansion in MacLaurin series, thus showing both the “uncertainty” of some of his previous results, and the lack of means for establishing “at the present state of analysis” whether a given expression is the general integral of a partial differential equation.

Two years later he took up the point again and remarked that from the way in which the expression (2.29) had been obtained “one does not know how to consider it in general as long as one has not given the imaginary expression $f(x+it)$ a definition independent of the form of the function $f(x)$ supposed real” (Cauchy 1823b, 330). To tell the truth, Cauchy continued, one could define this imaginary expression equally well by agreeing that the notation $f(x+it)$ represents a function $\phi(x, t)$, continuous with respect to x and t and satisfying both the condition $\phi(x, 0) = f(x)$ and the equation $\frac{\partial \phi}{\partial t} = \frac{\partial \phi}{\partial x} i$ (which is the complex form of the Cauchy–Riemann equations). In this case, he said, one could easily verify that ϕ satisfies Laplace’s equation (2.28) as well as the equations $\phi(x, 0) = f(x)$ and $\frac{\partial \phi}{\partial t}|_{t=0} = 0$. But in doing

⁴⁹For an account of this paper and related matters, see Burkhardt (1908), esp. pp. 671–680. See also Grattan–Guinness (1990), esp. pp. 690–694.

⁵⁰*Procès-verbaux des séances de l’Académie* 7 (1916), 271.

so, Cauchy concluded, one was led back to the integration of Laplace's equation, and one could not give the expression (2.29) as its integral without falling into a vicious circle (Cauchy 1823b, 330).

Cauchy then discussed the procedure followed by Poisson (1822b, 137–138), which led to the same difficulties as Cauchy's, and "one can say as much about all the formulae in which imaginary expressions are found contained in arbitrary expressions" (Cauchy 1823b, 333). Cauchy was not the only one to find the question difficult at this time. Fourier too fell into grave error here in his *Théorie*.⁵¹

For Cauchy the analogy with the real case apparently served as a guide for a long time when he dealt with imaginary "objects" in his research papers. With this in mind, his approach to complex integration and to the calculus of residues, which he elaborated in the middle of the 1820s, is easier to understand. In particular, it helps to explain his apparent failure to see the full implications of some of his discoveries.

2.4.1 The Résumé

Cauchy replaced Biot at the Collège de France in 1817. In his lectures there he presented some advances in integration that improved on his methods of 1814. He also investigated further the properties and applications of the Fourier transforms and their related inversion formulae that he had studied in his *Mémoire* of 1815 on the propagation of waves, and which Poisson had also obtained independently. Following this, in November 1819 he submitted a paper to the Académie which was based on his theory of singular integrals. The paper was never published,⁵² but a (rather cryptic) hint about its content can be found in the summary published in the *Mémoires* of the Académie (1819–1820) in 1824 (Cauchy 1824). There Cauchy began by remarking that Ruffini (1813) had definitely proved the impossibility of solving "by radicals" the general, algebraic equation of degree greater than four.⁵³

The question asked by Cauchy was whether a root of such an equation could be expressed in a finite form by means of integrals. He referred to a paper on Lagrange series, (Parseval 1806a), which was based on the consideration of series whose "convergence is not assured" and therefore the results are not rigorous, and then summarised his method in a few words.⁵⁴ Following this, Cauchy wrote the

⁵¹See his (1822, 506) and the footnote there by Darboux.

⁵²It is his "Sur la résolution analytique des équations de tous les degrés par le moyen des intégrales définies".

⁵³The same statement had already been made by Cauchy in his (1815b, 62), where he referred explicitly to a couple of works by Ruffini (1805) and (1799), which he mistakenly quoted as *Théorie des équations numériques*.

⁵⁴Further details can be found in a paper he published in 1822 (see below) as well as in the "general remarks and additions" appended to (Cauchy 1823b).

paper “Recherches sur les intégrales définies qui renferment des exponentielles imaginaires” (On definite integrals containing imaginary exponentials), which was only presented to the Académie 3 years later, on September 16, 1822, and was also never published. Eventually, he submitted to the Académie on October 28, 1822 a paper on definite integrals where he summarised all his research work on singular integrals, quickly followed by a “very short analysis” of it (Cauchy 1822b) in the *Bulletin de la Société Philomatique*. There Cauchy began by recalling that he had expounded some of the principles on which his “new formulae” were based in an unpublished paper presented to the Académie on May 13, 1816. After summarising the definition of a singular integral, he considered the integral $\int_{x'}^{x''} f(x)dx$ where $f(x)$ becomes infinite or indeterminate at the value x_0 .⁵⁵ If k denotes an infinitely small number,⁵⁶ and f_0 the “true value” of $kf(x_0 + k)$ corresponding to $k = 0$, then the singular integral $\int_{x_0 - k\alpha'}^{x_0 + k\alpha''} f(x)dx$ (α', α'' being constants) equals $f_0 \log \frac{\alpha''}{\alpha'}$. He extended this to functions becoming infinite or indeterminate at n values $x' < x_0 < x_1 < \dots < x_{n-1} < x''$. Thus $\int_{x'}^{x''} f(x)dx$ will be equivalent to the sum A of singular integrals

$$A = \int_{x'}^{x_0 - k\alpha'} f(x)dx + \int_{x_0 + k\alpha''}^{x_1 - k\beta'} f(x)dx + \dots + \int_{x_{n-1} + k\epsilon''}^{x''} f(x)dx$$

If $\alpha' = \alpha'' = \beta' = \beta'' = \dots = \epsilon'' = 1$, then one gets “the principal value” B of the integral $\int_{x'}^{x''} f(x)dx$, whose “general value” will be given by $A = B + f_0 \log \frac{\alpha''}{\alpha'} + f_1 \log \frac{\beta''}{\beta'} + \dots + f_{n-1} \log \frac{\epsilon''}{\epsilon'}$ thus depending on the constants $\alpha', \alpha'', \beta', \beta'', \dots, \epsilon''$. Thus the condition that $A - B$ becomes 0 for infinitely small values of k is necessary and sufficient for the value of the integral to be finite and determinate.

Then he extended the previous result to complex functions becoming infinite at values $x_0, x_1, \dots, x_{n-1}$, whose real parts lie in the interval x', x'' and whose imaginary parts lie in y', y'' . If $f_0, f_1, \dots, f_{n-1}$ are what he called the “true values” of $kf(x_0 + k)$, $kf(x_1 + k)$, $\dots$, $kf(x_{n-1} + k)$ corresponding to $k = 0$, then Cauchy stated (without proof) that

$$\int_{x'}^{x''} [f(x + iy'') - f(x + iy')]dx = \\ i \int_{y'}^{y''} [f(x'' + iy) - f(x' + iy)]dy - 2\pi i(f_0 + f_1 + \dots + f_{n-1}),$$

⁵⁵Cauchy supposed x_0 to be a simple infinity i.e., as Cauchy used to say, a simple root of $1/f(x)$, as he had done in his 1814 *Mémoire*.

⁵⁶In spite of the care Cauchy showed when defining infinitely small quantities as “variables having zero as their limit” in the *Cours d’analyse*, here and elsewhere in his research papers he kept using the old language of infinitesimals.

thus rewriting in a more perspicuous and compact form a formula which he had obtained in his 1814 paper by considering the real and imaginary parts separately.⁵⁷ He also pointed out that the contribution to the sum $(f_0 + f_1 + \dots + f_{n-1})$ must be halved for terms corresponding to values whose real (resp. imaginary) parts equal either x' or x'' (resp. y' or y''). Then he generalised this result by supposing $x = P + iR$, P and R being real functions of two real variables p, r . Assuming that $x_0, x_1, \dots, x_{n-1}$ are the (simple) infinities of $f(x)$ for which the corresponding values of p, r lie between p' and p'' and resp. between r', r'' , he wrote

$$\chi(p, r) = f(P + iR) \frac{\partial(P + iR)}{\partial p}, \quad \psi(p, r) = f(P + iR) \frac{\partial(P + iR)}{\partial r},$$

and stated, without proof, that one “generally” has

$$\int_{p'}^{p''} [\chi(p, r'') - \chi(p, r')] dp = \int_{r'}^{r''} [\psi(p'', r) - \psi(p', r)] dr - 2\pi i (\pm f_0 \pm f_1 \pm \dots \pm f_{n-1}), \quad (2.30)$$

where the sign $\pm$ is given by the sign of the real function $\frac{\partial P}{\partial p} \frac{\partial R}{\partial r} - \frac{\partial P}{\partial r} \frac{\partial R}{\partial p}$ (i. e. the Jacobian of the transformation) at the corresponding values of p and r . This enabled him to write the “general” formula which was included in his unpublished 1819 *Mémoire*

$$\begin{aligned} & \int_0^\pi \frac{f(\cos p + i \sin p)}{F(\cos p + i \sin p)} dp = \\ & i \int_{-1}^1 \frac{f(r)}{rF(r)} dr + \pi \left(\frac{f(0)}{F(0)} + \frac{f(a)}{aF'(a)} + \frac{f(a')}{a'F'(a')} + \dots \right) + \\ & 2\pi \left(\frac{f(\alpha + i\beta)}{(\alpha + i\beta)F'(\alpha + i\beta)} + \frac{f(\alpha' + i\beta')}{(\alpha' + i\beta')F'(\alpha' + i\beta')} + \dots \right) \end{aligned} \quad (2.31)$$

$a, a', \dots$ being the real roots of $F(x) = 0$ with “numerical value” < 1 , and $\alpha + i\beta, \alpha' + i\beta', \dots$ the complex roots of it, with modulus < 1 and positive imaginary part. According to Cauchy, this formula can be obtained from (2.30) by replacing $f(x)$ by $\frac{f(x)}{xF(x)}$ and by putting $P + iR = r(\cos p + i \sin p)$ with $p' = 0, p'' = \pi, r' = 0, r'' = 1$. Certainly, as [Smithies](#) (1997, 70) did, one could remark that the formula above “is equivalent to a case of the residue theorem for the semicircular domain bounded by the upper half of the unit circle and the interval $[-1, 1]$ of the real axis, and the integral on the right-hand side of [the formula above] being interpreted as a principal value if necessary”. But, perhaps more interestingly from a historical

⁵⁷ Actually, this formula is contained in a footnote Cauchy added in 1825 to his 1814 *Mémoire*, see [Cauchy](#) (1814, 422–423).

viewpoint, one has also to remark that Cauchy considered the formula above, and the following ones, just as the results of formal transformations, and that he obtained a clear and definite idea of the residue (and the integral) theorem only some years later.

Among the formulae that he had derived from (2.31) in his 1819 *Mémoire* he mentioned the following

$$\int_0^\pi e^{ip} \frac{f(e^{ip})}{e^{ip} - a} dp = \pi f(a) + \int_{-1}^1 \frac{f(r)}{r - a} dr,$$

where $0 < a < 1$ and the function $f(re^{pi})$ does not become indeterminate or infinite between the limits $p' = 0$, $p'' = \pi$, $r' = 0$, $r'' = 1$. From this, he claimed (without proof) that one can “easily” obtain the equations

$$\frac{1}{2} \int_{-\pi}^\pi e^{-npi} f(b + e^{pi}) dp = \frac{\pi}{n!} f^{(n)}(b) \quad (2.32)$$

$$\frac{1}{2} \int_{-\pi}^\pi e^{-npi + he^{pi}} dp = \frac{\pi h^n}{n!} \quad (2.33)$$

$$\Delta^m h^m = \frac{n!}{2\pi} \int_{-\pi}^\pi (e^{e^{pi}} - 1)^m e^{-npi + he^{pi}} dp, \quad (2.34)$$

where m and n are (positive) integers and b and h are (real) arbitrary constants. (One could recognise in the first of these the expression of the n th derivative of an analytic function at the centre of a circle in terms of its values on the circumference, a result that Cauchy was to state in detail only in his 1831 memoir, see Sect. 3.4 below). Then Cauchy listed (again without proof) a series of formulae which he had obtained from the previous ones by taking their real parts. Thus, for instance, he found

$$\frac{1}{2} \int_0^\pi \left(\frac{f(e^{pi})}{1 - ae^{-pi}} + \frac{f(e^{-pi})}{1 - ae^{pi}} \right) dp = \pi f(a), \quad 0 < a < 1, \quad (2.35)$$

which can be recognised as Cauchy’s integral formula in the special case of the unit disk. In this vein, by putting $F(r) = 1 - ar$, ($0 < a < 1$) from (2.31) he obtained

$$\frac{1}{2} \int_0^\pi \left(\frac{f(e^{pi})}{1 - ae^{pi}} + \frac{f(e^{-pi})}{1 - ae^{-pi}} \right) dp = \pi f(0),$$

and the corresponding ones for $a > 1$ and for $a = 1$ from which he also derived the remarkable “mean value” formula

$$\frac{1}{2} \int_0^\pi (f(e^{pi}) + f(e^{-pi})) dp = \pi f(0). \quad (2.36)$$

At this point Cauchy made an interesting remark that helps to explain his motivation for introducing the concept of definite integral the way he did in his *Résumé*. He said that he considered each definite integral, taken between real limits,

as nothing other than the sum of the values of the differentials corresponding to the various values of the variable between the given limits. This way of thinking of a definite integral, it seems to me, must be preferred because it leads equally to all cases, even those in which one does not know at all how to pass generally from the function placed under the integral sign to the primitive function (Cauchy 1822b, 295–296).

Thus, his research work on definite integrals, including in particular his theory of singular integrals, apparently persuaded him to abandon the prevailing view at the time that the definite integral should be defined via the primitive function and, instead, to define the definite integral as limits of partial sums. In addition, Cauchy stated, this view “has the advantage” of always providing real values for integrals of real functions, as well as allowing one to separate “easily” any imaginary equation into its real and the imaginary parts. This will not be the case when considering a definite integral as the difference of the values taken by a (discontinuous) primitive function at the limits of integration, or when allowing the variable to take imaginary values. The latter was the technique Poisson had used in his 1820 paper on definite integrals. There Poisson observed that by evaluating the integral $\int_{-1}^1 \frac{dx}{x^2}$ according to the usual rules one has

$$\int_{-1}^1 \frac{dx}{x^2} = \left[-\frac{1}{x} \right]_{-1}^1 = -2.$$

Surprisingly, a negative value, despite the fact that $\frac{dx}{x^2}$ is positive throughout the interval. Analogously, one has

$$\int_{-1}^1 \frac{dx}{x} = [\log x]_{-1}^1 = -\log(-1) = (2n+1)\pi i.$$

How is it possible that the sum of real elements $\frac{dx}{x}$ has infinitely many imaginary values? Poisson’s answer was that the theorem $\int_a^b f(x)dx = F(b) - F(a)$ where $dF(x) = f(x)dx$ ceases to hold if $f(x)$ becomes infinite in the interval of integration. To resolve the difficulty he suggested making an appropriate change of variables. Thus, in the last example, by setting $x = -(\cos z + i \sin z)$ and integrating from $z = 0$ to $z = (2n+1)\pi$ one avoids passing through 0 and obtains

$$\int_{-1}^1 \frac{dx}{x} = -i \int_0^{(2n+1)\pi} dz = (2n+1)\pi i,$$

which is what it should be.

In his (1822b) Cauchy referred to Poisson’s paper and considered the similar example

$$\int_{-1}^{-2} \frac{dx}{x} = \int_{-1}^2 \frac{xdx}{x^2} = \int_{-1}^2 \frac{\frac{1}{2}d(x^2)}{x^2}$$

to point out that it can happen that several primitive functions correspond to the same integral, as happens in this example by taking as primitive function either $\log x$ supposed to be both real and imaginary or $\frac{1}{2} \log(x^2)$ supposed to be always

real. According to Cauchy, the way to get a correct result was to resort to his own theory of singular integrals and principal values.

Eventually, in a postscript to the paper, Cauchy showed how all of his formulae could be obtained “directly” as an application of Parseval’s (1806b) theorem on the sum of series, which Cauchy stated as follows: Assuming that

$$\varphi(x) = a_0 + a_1x + a_2x^2 + \dots$$

$$\chi(x) = b_0 + b_1x + b_2x^2 + \dots$$

and

$$\psi(x) = a_0b_0 + a_1b_1x + a_2b_2x^2 + \dots$$

one has

$$\begin{aligned}\psi(xy) &= \frac{1}{2\pi} \int_0^\pi [\varphi(xe^{pi})\chi(ye^{-pi}) + \varphi(xe^{-pi})\chi(ye^{pi})]dp \\ &= \frac{1}{2\pi} \int_{-\pi}^\pi [\varphi(xe^{pi})\chi(ye^{-pi})]dp.\end{aligned}$$

This theorem could be proved “immediately” by developing the functions φ and χ in power series. In the particular case $x = y = 1$ one has

$$\pi\psi(1) = \frac{1}{2} \int_0^\pi (\varphi(e^{pi})\chi(e^{-pi}) + \varphi(e^{-pi})\chi(e^{pi})) dp \quad (2.37)$$

from which all of his formulae from (2.31) to (2.36) could be derived under the additional hypothesis that the MacLaurin expansion of the function involved, $f(x)$, given by

$$f(0) + \frac{x}{1}f'(0) + \frac{x^2}{2!}f''(0) + \dots,$$

converged to $f(x)$ for $x < 1$ (but in fact he needed $|x| \leq 1$).

The last condition was far from being incidental. On January 22 of that very same year Cauchy had presented to the Académie and published in the April issue of the *Bulletin de la Société Philomatique*, a remarkable discovery concerning the MacLaurin expansion of functions. Cauchy must have attributed a great importance to his discovery because he republished it as a separate pamphlet.⁵⁸ In fact, what he discovered was (in modern terms) the first example of a function which is non-analytic at a point.

He was led to this discovery by his study of Lagrange’s (and Laplace’s) theory of singular solutions of differential equations.⁵⁹ Although his criticism was chiefly

⁵⁸A copy of this rather rare pamphlet, placed in a volume with other papers by Cauchy, is kept in the “Fond Mandelbrojt” of the mathematical centre at Luminy.

⁵⁹For a detailed account of this matter, see Bottazzini (1992a), lxvii–lxxii.

addressed to “Lagrange’s method” of assuming the expansion of functions in power series as “the principal basis” of infinitesimal calculus, it was also extended to the common practice of resorting to (power) series expansions in order to study the main properties of functions. When replacing a function with a series, Cauchy observed, one implicitly assumes that the function is “completely characterized” by an expansion in infinite terms. Thus, for instance, by replacing a function $f(x)$ with its MacLaurin expansion

$$f(x) = f(0) + \frac{x}{1}f'(0) + \frac{x^2}{2!}f''(0) + \dots,$$

one supposes that a unique value of $f(x)$ corresponds to a given (infinite) system of finite values for $f(0), f'(0), f''(0), \dots$. If they are all equal to 0, then one should conclude that $f(x)$ itself vanishes identically. However, Cauchy stated, “this conclusion can be not exact” as is shown by the function $f(x) = e^{-1/x^2}$ for which $f(0) = f'(0) = f''(0) = \dots = 0$. The same holds true for $f(x) = e^{-1/\sin^2 x}$ and for infinitely many similar functions. Even the uniqueness of the expansion of functions in convergent series was put into question. Indeed, the functions e^{-x^2} and $e^{-x^2} + e^{-1/x^2}$ have the common expansion in convergent series $1 - \frac{x^2}{1!} + \frac{x^4}{2!} - \frac{x^6}{3!} + \dots$ whose sum, however, is equivalent to e^{-x^2} alone. Thus, [Cauchy \(1822a, 278\)](#) concluded “one is not allowed to substitute series for functions indiscriminately”. In order to avoid any mistake one must make sure that the series converge to the given function. In all other cases the series could be employed “with complete confidence” only by reducing them to a finite number of terms, and by completing them with a remainder of which one knows the exact or approximate value.⁶⁰ This was exactly what he stated in the *Avertissement* that opens his *Résumé*:

my principal aim has been to reconcile the rigour which I had made a law for myself in my *Cours d'analyse*, with the simplicity that results from the direct considerations of infinitesimals. For this reason, I have believed it necessary to reject the expansion of functions in infinite series whenever the series thus obtained is not convergent; and I have been forced to postpone Taylor’s formula to the integral calculus since this formula can no longer be admitted as general unless the series that it contains is reduced to a finite number of terms, and completed by a definite integral. I am well aware that the illustrious author of the *Mécanique analytique* took the formula in question as basis of his theory of *derived functions*. But, despite all the respect that such a great authority requires, the majority of geometers now agree in recognising the uncertainty of the results to which one can be led by the use of divergent series, and we add that in many cases Taylor’s theorem seems to provide the expansion of a function in convergent series, even though the sum of the series essentially differs from the proposed function.

His counter-example $f(x) = e^{-1/x^2}$, briefly mentioned at the end of Lesson 38, was just such a case in point.

⁶⁰[Grattan-Guinness \(1990, 735–736\)](#) points out that Poisson tried to refute Cauchy’s argument in a paper that appeared in the June issue of the *Bulletin*.

Cauchy introduced the concept of definite integral as the limit of partial sums in Lesson 21. Given a function $f(x)$ of a real variable x , continuous in the interval $[x_0, X]$, he considered $n - 1$ values $x_0 < x_1 < x_2 < \dots < x_{n-1} < X$, and the sum $S = \sum_j (x_j - x_{j-1})f(x_{j-1})$ which depends on both the number of intervals (x_j, x_{j-1}) and the way of dividing $[x_0, X]$. Then Cauchy proved⁶¹ that with increasing n the value of S “will end by attaining a certain limit [the definite integral] that will depend uniquely on the form of the function $f(x)$ and the extreme values x_0, X attributed to the variable” (Cauchy 1823a, 125). In Lesson 23 he stated that the definition applies to imaginary functions $f(u + iv)$ by remarking that one has

$$\int_{x_0}^X f(u + iv)dx = \int_{x_0}^X udx + i \int_{x_0}^X vdx.$$

In the subsequent Lesson he dealt with definite integrals whose value is infinite or indeterminate, and on this occasion he took up the example $\int_{-1}^1 \frac{dx}{x}$ and observed that, if ε denotes an infinitely small number and μ, v arbitrary constants, one has

$$\int_{-1}^1 \frac{dx}{x} = \lim \left(\int_{-1}^{-\varepsilon\mu} \frac{dx}{x} + \int_{\varepsilon v}^1 \frac{dx}{x} \right) = \lim \left(\log(\varepsilon\mu) + \log \left(\frac{1}{\varepsilon v} \right) \right) = \log \left(\frac{\mu}{v} \right),$$

which is “completely indeterminate” depending on the log of the arbitrary constants $\frac{\mu}{v}$. This introduced the definition of singular integrals [Lesson 25] and the principal value of an integral.

In Lesson 34 Cauchy re-obtained the same expression for the correction term Δ that he had published in his (1823b, 338) paper, $\Delta = 2\pi i(f_1 + f_2 + \dots + f_n)$ where f_i , ($i = 1, \dots, n$) is the “true value” of $(x - x_i)f(x)$ at x_i , $x_1, x_2, \dots, x_n$ being the infinities of $f(x)$ with positive imaginary part, and observed that $\int_{-\infty}^{+\infty} f(x)dx = \Delta$ if $f(x + iy) = 0$ for $x = \pm\infty$ for any y , and for $y = +\infty$ for any x . This unsatisfactory condition at infinity was one Cauchy had stated earlier, in his 1814 memoir, and repeated elsewhere in other papers and notes of the period. On the other hand, at the end of Lesson 25, when considering the integral $\int_{-\infty}^{+\infty} \frac{f(x)}{F(x)}dx$, he had correctly stated (without proof) that a necessary and sufficient condition for the integral to have a “finite and determinate” value is that

1. $F(x) = 0$ has no real roots
2. $\deg F(x) \geq \deg f(x) + 2$.

Before the full text of the *Résumé* appeared in print in August 1823 Cauchy republished some of his results on integration of complex functions in the *Observations générales et additions* appended to his (1823b). There, once again, he referred to his (still unpublished) 1814 memoir, to the (unpublished) memoirs he had presented

⁶¹By resorting tacitly to what nowadays is called the uniform continuity of $f(x)$.

to the Académie in 1816, 1819, and 1822, and to the *Résumé*.⁶² As special cases of his formulae Cauchy re-obtained some of the integrals already found by Euler and Laplace (and discussed above). Of particular interest is a footnote which Cauchy (1823b, 337) added when introducing formula (2.30). There he stated (without proof) that if $1/f(x)$ has a zero of order p at x_0 , then the value of f_0 , which is the contribution to the correction term corresponding to x_0 , is the “true value” of

$$\frac{1}{(p-1)!} \frac{d^{p-1}[k^p f(x_0 + k)]}{dk^{(p-1)}},$$

when $k = 0$. Apparently, he was in possession of the formula for the residue of a function at what he called “a multiple point of infinity” a long time before he revealed in print how to obtain it.

2.5 Practice Without Theory

To trace the early developments of the ideas that eventually led Cauchy to his preliminary contributions to complex function theory, as we have just done, is to tell the story of the occasional success of various practices that became increasingly common in mathematics from the middle of the eighteenth century. The pioneering work by d’Alembert (1752), who first introduced imaginary expressions to integrate differential forms, is a case in point. In the course of this work d’Alembert wrote down the equations (2.1) that later were to become known as the Cauchy–Riemann equations. However, contrary to what is often stated, it would be quite inaccurate from a historical point of view to claim that complex function theory actually began with d’Alembert’s *Essai*. In fact, he did not connect these equations with any development of complex function theory. In his hands resorting to “imaginary” expressions was nothing more than a trick of calculation. He wanted the integrals of the differential forms that were involved to be real, and at the end of his reasoning he was pleased to remark that the imaginary quantities had disappeared. In short, we can label his working with imaginary variables as a skilful, successful practice without a coherent and satisfactory theory.

d’Alembert’s “metaphysics”—to adopt a term widely used by eighteenth-century mathematicians—was shared by Euler and Lagrange in their manipulation of the differential forms that occurred in various fields of “mixed” mathematics, from fluid dynamics to orthogonal trajectories and cartography. In particular, imaginary functions occurred in a number of papers by Euler on conformal mappings of the

⁶²This seemingly chronological incongruence can be explained by thinking of the peculiar structure of the *Résumé*. Each Lesson in it amounts to exactly four printed pages, i.e. a printed double-face folio, and it seems that each Lesson was printed and distributed separately to the students during the academic year 1823 before being collected into a book.

Earth on a plane, but even then, like d'Alembert, Euler emphasised that at the end of the process of integration the solution functions could always be made to be real.

Euler, followed by Laplace, contributed in essential ways to the evaluation of improper real integrals by systematically applying the method of “imaginary substitutions”. This “passage from real to imaginary quantities” gave rise to an argument between Laplace and Poisson, who regarded the passage as at best a discovery method lacking the rigour of a proof, which he supplied using the method of changing the order of integration in double integrals. Laplace probably suggested to the young Cauchy that he should tackle this problem, and this he did by providing a “direct and rigorous analysis” of the results obtained by the questionable “passage”.

Cauchy's first contribution to this area, the 1814 *Mémoire* on definite integrals, provided a wealth of new ideas and results and became a lasting point of reference for Cauchy himself in the subsequent decades. In it, he established the Eq. (2.14) that in his view “contain all the theory of the passage from the real to the imaginary” (Cauchy 1814, 338), and applied them to particular cases—without ever remarking that the Cauchy–Riemann equations could easily be obtained as a special case. Like his eighteenth-century mathematical predecessors, Cauchy simply considered it a matter of routine that the functions $P(x,y)$ and $Q(x,y)$ satisfy those equations in order that $Pdy - Qdx$ and $Pdx + Qdy$ be complete differentials.

Cauchy's contemporary readers, Lacroix, Legendre, and Poisson, all agreed that the most original part of Cauchy's memoir was his discussion of the possibility of inverting the order of integration of a double integral, and that his main discovery was that one obtains different values depending on the order of integration when the function becomes infinite or indeterminate at points inside the domain of integration (this is the origin of Cauchy's theory of singular integrals).

Cauchy's results prefigured developments a decade later in his creation of the theory of “integrals between imaginary limits” and the theory of residues. From a modern point of view they could be easily interpreted as (particular cases of) the integral theorem and the residue theorem, and by the time this memoir was eventually published in 1827 Cauchy himself enriched his original text with extensive footnotes emphasising the link between his former results and his new achievements. From a historical standpoint, however, it is misleading to claim that in 1814 Cauchy obtained results that “can be seen to be equivalent” to the Cauchy integral theorem for a particular class of quadrilaterals, or to his residue theorem for functions having only simple poles, as both Belhoste (1991, 109–111) and Smithies (1997, 190–191) have done. This attitude prevents one from properly evaluating the changes in Cauchy's understanding of complex functions which took place in the 1820s. In addition, as Smithies himself recognised, in 1814 Cauchy carefully avoided any geometrical setting, and this corresponded to his deepest convictions about the rigorous treatment of imaginary quantities, as he was to explain in his *Cours d'analyse* in 1821.

Perhaps this celebrated book scarcely served as a textbook for students, but it certainly played a major role in introducing new standard of rigour in analysis, as Abel was quick to recognise. The concepts it introduced guided Cauchy himself up

to the 1850s, and not always productively—the lasting trouble Cauchy was to have with multi-valued algebraic functions has its origins in his definition of continuity. On the other hand, the solution of functional equations, the convergence criteria for series, and the uniqueness theorem for the expansion of a continuous function in a power series were among the main features of the “real” part of the *Cours* that “translated” more successfully to the complex case in the second part. There Cauchy gave a remarkably careful treatment of “imaginary expressions” from the methodological and ontological points of view. In our opinion, in defining algebraic operations with imaginary expressions and establishing their properties Cauchy displayed his concept of rigour at its best.

Cauchy introduced imaginary quantities in a purely formal way as symbolic expressions that “taken literally are inexact or have no meaning” and require “new conventions” to be accepted. Hence the need for suitable, appropriate conventions to establish the passage from real to imaginary quantities. Accordingly, he was able to define algebraic functions of imaginary variables, but this was not enough to define elementary transcendental functions of an imaginary variable, a task that he accomplished only by resorting to imaginary power series. However, as we have emphasised, Cauchy was silent in the *Cours* about the possibility of providing a general definition of an arbitrary function of an imaginary variable. This question remained unanswered even after his later attempts in 1823. Lacking an appropriate conception of what a complex function of a complex variable is, Cauchy could only handle imaginary “quantities” by analogy with the real case.

In the early 1820s the study of definite integrals involving imaginary functions became a major focus of Cauchy’s research work. Among the new results Cauchy was able to obtain was a formula which turned out to be the expression of the residue of a function at a point of infinity of any finite order. Again it is worth emphasising that Cauchy failed to explain the meaning of his formulae, which could be better understood in the light of his later results in the 1830s. So, although there is hardly any doubt that by the early 1820s Cauchy was in possession of a wealth of results that, in an incoherent way, prefigured his later achievements, a satisfactory theory of complex functions was still lacking. An essential step towards such a theory was his decision to get rid of the “old” definition of definite integrals based on primitive functions, and instead to define them as limits of partial sums. This will be discussed in the next chapter.

Appendix: Proofs of the Fundamental Theorem of Algebra

Around 1816 when Gauss produced his second and third proofs of the fundamental theorem of algebra, this theorem had come independently to the attention of Parisian mathematicians. Unmentioned by Gauss, an algebraic proof had been offered by Laplace in 1795 in his *Leçons* at the École Normale. In the Notes IX and X appended to his treatise (1798) Lagrange had dealt extensively with the fundamental theorem of algebra and discussed in detail its various proofs, from d’Alembert’s up

to Laplace's, with the remarkable exception of (Gauss 1799) which was apparently unknown to him. According to Lagrange (1798, 234), Laplace's proof was all that one could wish for in a simple demonstration even though it would be impossible to perform the required calculations in practice.⁶³ Because of its algebraic character, which does not illuminate Laplace's view of complex quantities, we pass over this proof to consider Argand's and Cauchy's various proofs.

In the concluding paragraph of his (1806) Argand sketched a proof based on an (implicit) minimum argument and an inequality that in modern terms can be stated as follows: given the (non-constant) polynomial $f(z)$, for every $p \in \mathbb{C}$ there exist a $p' \in \mathbb{C}$ such that $|f(p')| < |f(p)|$. More precisely, Argand stated that any complex polynomial $Y(X) = X^m + aX^{m-1} + bX^{m-2} + \dots + fX + g$ where $a, b, \dots, f, g$ are complex numbers, can be split into factors of the first degree. To this aim, he supposed $X = p$ and $X = p + \rho h$, where h is an arbitrary number and ρ is any root of the unity, and considered the corresponding values $Y(p)$ and $Y(p + \rho h) = Y(p) + hpQ + h^2\rho^2R + h^3\rho^3S + \dots$, where $Q, R, S, \dots$ are known quantities obtained by expanding the powers of $p + \rho h$. By supposing h to be infinitely small the terms involving $h^2, h^3, \dots$ "disappear" and one gets $Y(p + \rho h) = Y(p) + hpQ$. Thinking of the complex quantities in geometric terms as vectors in the plane, Argand observed that by an appropriate choice of the direction of ρ one had $|Y(p + \rho h)| < |Y(p)|$. By iterating this reasoning (and by implicitly resorting to a minimum argument)⁶⁴ Argand concluded that eventually one obtained a value for X such that $Y(X) = 0$. Later on, in his (1814–1815) he offered essentially the same proof. However, in order to avoid "every kind of cloud" (*toute espèce de nuage*) due to the use of infinitesimals, he reformulated his inequality in purely algebraic terms.⁶⁵

As we have seen, the minimum theorem played an essential role in the proof of the fundamental theorem of algebra that Cauchy gave in his *Cours d'analyse* (see Sect. 2.3). However, the basic idea of his proof went back to 1817, when Cauchy published two different proofs of the theorem. It is interesting to remark that by then Cauchy was well aware of Gauss's 1799 proof—as Cauchy himself explicitly acknowledged, his first proof (Cauchy 1817a) uses methods analogous to Gauss's. This fact, generally missed by historians, shows that Cauchy was a more attentive reader than is commonly believed. It also provides evidence that Gauss's criticism had penetrated the other side of the Rhine as early as 1817.

In his (1817a) Cauchy wanted to prove that any (real) polynomial equation $x^n + a_1x^{n-1} + a_2x^{n-2} + \dots + a_{n-1}x + a_n = 0$ with no real solutions will always have a

⁶³See the modern account of it given by Remmert in (Ebbinghaus et al. 1990, 120–122).

⁶⁴As was already observed by Servois who commented acutely "it seems to me that it is not enough to find values of x that give continually decreasing values to the polynomial, it is also needed that the law of the decrements necessarily leads the polynomial to zero" (quoted in Lebesgue 1937, 152).

⁶⁵After presenting the proof of Argand's inequality in modern terms, Remmert remarked that it is a special case of the "open mapping theorem" according to which (nonconstant) holomorphic functions map open sets onto open sets (in Ebbinghaus et al. 1990, 120–122).

solution of the form $x = r(\cos \varphi \pm i \sin \varphi)$. In other terms, one could always find r, φ such that

$$r^n \cos n\varphi + a_1 r^{n-1} \cos(n-1)\varphi + \dots + a_{n-1} r \cos \varphi + a_n = 0$$

and

$$r^n \sin n\varphi + a_1 r^{n-1} \sin(n-1)\varphi + \dots + a_{n-1} r \sin \varphi = 0$$

are satisfied. To this end he proved the following lemmas:

1. If $y = b$ is a non-multiple real root of the (real) equation $f(y) = 0$, then for positive, sufficiently small δ and any v such that $0 < v \leq \delta$ the functions $f(b+v)$ and $f(b-v)$ have opposite sign.
2. If $f(x, y) = 0$ is a (real) polynomial equation which for a given x has distinct roots with respect to y , the roots will vary continuously with respect to x , and none of them will vanish unless $f(x, y) = 0$ has multiple roots.

Then, by putting $\cos \varphi = s$ he rewrote the equations above as

$$f_n(r, s) = 0 \quad \text{and} \quad r(1 - s^2)^{1/2} f_{n-1}(r, s) = 0.$$

In order to prove his theorem he simply needed to prove that values of r and s exist such that the equations

$$f_n(r, s) = 0 \quad \text{and} \quad f_{n-1}(r, s) = 0$$

are satisfied. As [Gauss \(1799\)](#) had done, he observed that for $r = \infty$ they reduce to

$$\cos n\varphi = 0 \quad \text{and} \quad \sin n\varphi = 0,$$

whose roots are $s = \cos \frac{(2k-1)\pi}{2n}$ ($k = 1, \dots, n$) and $s = \cos \frac{(k-1)\pi}{n}$, ($k = 1, \dots, n$) respectively.

It follows from Lemma 2 that the roots will all remain distinct for $r > R$ for some large enough R . Cauchy then let r decrease to 0 and, by a rather obscure argument, he concluded that for a suitable value of r (at least) two roots s of $f_n(r, s) = 0$ $f_{n-1}(r, s) = 0$ coincide.

This paper was followed by a second Note (1817b), re-published in (1820). There Cauchy presented an alternative proof of the theorem based on the existence of a minimum of $R^2 = |f(u + iv)|$, which was essentially the same as the proof that he included in his *Cours d'analyse*.

Chapter 3

Cauchy's "Modern Analysis"

The great law limiting the existence of formulae is the law of continuity.

Cauchy

3.1 Introduction

Cauchy's early contributions to complex analysis can properly be dated to the 1820s when he obtained the famous integral and residue theorems and indeed introduced the name the "calculus of residues" (Fig 3.1). Following this, in the 1830s he made such substantial contributions as the integral formula now named after him and the *calcul des limites* (the method of majorants, as it is called nowadays), which he applied to the integration of differential equations in a complex domain.

Because of these and other discoveries made towards the end of his life, Cauchy is rightly remembered as one of the three founders of complex function theory, together with Riemann and Weierstrass. That said, his work in this area has often been criticised for its obscurity. In part, these criticisms are justified, but partly, too, they derive from imposing modern sensitivities and judgements about what is real and what is complex analysis. Granted, Cauchy did seem to many to forget his earlier discoveries and often to fail to see the implications of his own results. Certainly the reception of his work was hindered by the lack of a systematic presentation. But when Cauchy created many of the first major theorems of complex analysis there was, of course, no possibility of making a distinction between real and complex analysis. Indeed, one of his greatest discoveries is surely that there was a theory of functions of a complex variable distinct from, and much richer than, any contemporary theory of functions of two real variables, or maps from $\mathbb{R}^2$ to $\mathbb{R}^2$. Cauchy's many equivocations, ambiguities, and evasions, as well as his profound insights show precisely how tortuous was the path towards "modern analysis", as he used to call his own analytical work.

Fig. 3.1 Augustin-Louis Cauchy (portrait by Roller, in Belhoste *Augustin-Louis Cauchy: a biography*, Springer ©1991, frontispiece)



However, his failure to think systematically in a geometrical way may have blinded him to the implications of this work, as we discuss in Sect. 3.5.1, and he only began to see its full import in the later 1840s as other, younger mathematicians came to the subject. So it is only in Sect. 3.6 that we see how Cauchy, having turned 60, first rigorously introduced the general concept of a complex function of a geometrical complex quantity $z = x + iy$, something he had failed to provide in 1821. It remained for him to isolate the crucial defining concept of an analytic function. We naturally look for the Cauchy–Riemann equations and some indication of the importance of differentiability. But it was only after 20 years of going backwards and forwards over the ground that connects a holomorphic function in an open disk with its expansion there in a convergent Taylor series that Cauchy achieved any degree of clarity.

Cauchy’s legacy in this area was, therefore, mixed and not easy for his readers to follow. Although he occasionally referred to the theory of elliptic functions, his own work proceeded in virtual isolation from contemporary developments in that theory. His own life, with a prolonged period of self-exile in Turin and later Prague from 1830 to 1838, further separated him from other mathematicians, who perhaps saw him more as a rival than as a colleague. For all the profundity of his discoveries, he left to others the task of presenting the material in coherent form and displaying its riches.

3.2 Cauchy's 1825 *Mémoire*

Cauchy's studies on integration culminated in a *Mémoire* on integrals in a complex domain (Cauchy 1825a) that in many ways can be considered as his masterwork. It originated in a memoir that Cauchy presented to the Académie on February 28, 1825. In the wake of its publication, he published a short resumé of it in the *Bulletin de Férussac* in April 1825 (Cauchy 1825b). Curiously enough, he then published this celebrated paper twice as a separate pamphlet.¹ Then, in October 1825 he summarised its content and presented a number of results concerning particular cases in a paper that appeared (in two parts) in the *Annales de Gergonne* (Cauchy 1825c, 1826b).

In the opening lines of his *Mémoire* (1825a) Cauchy hinted at his reasons for embarking on new investigations. He referred to his unpublished memoir of October 28, 1822 as well as to Cauchy (1823a,b) where he had established “generally” the meaning of the notation

$$\int_{x_0}^X f(x)dx, \quad (3.1)$$

x_0 and X being real values and $f(x)$ a real or imaginary function. As for integrals between imaginary limits, he mentioned in passing the “remarkable” results of Laplace, and recent work by Brisson.² According to Cauchy, Brisson told him that he had applied such integrals and their transformations “with success” to the expansion of functions in series of exponentials. Then Cauchy referred to the work of Ostrogradskii, a “very talented” young mathematician who had come to Paris in 1822 from St. Petersburg.

In an (unpublished) paper submitted to the Académie on July 24, 1824 Ostrogradskii had echoed Cauchy's criticisms of Poisson's way of dealing with integrals of functions that become infinite in the interval of integration. In a subsequent paper (also unpublished) submitted to the Académie on August 7, 1824 Ostrogradskii had obtained the same formula for the “residue” at a multiple point of infinity that Cauchy (1823b, 337) had published, and he now communicated the main results of his investigations to Cauchy (see Yushkevich 1965). At the same time another young Russian, V.J. Buniakowskii, attended Cauchy's lectures at the Collège de France and

¹This paper is incorrectly said to have been rare since the time of its publication. Kline (1972, 636–637) even claimed that it “was not published until 1874”. Instead, there is evidence that Cauchy had it printed twice in a few months. The second edition appeared in August 1825 and differed from the first only in the number of Cauchy's academic titles listed on the cover page, and in the improved version of the errata corrige. This second version was reprinted in 1874–1875, before being included in his *Oeuvres* (2) 15, 41–89.

²At the time, Brisson was at the centre of an affair that involved Cauchy and Poisson on opposite sides; see Belhoste (1991, 51–52). In November 1823 Brisson had submitted a memoir on the theory of linear partial differential equations to the Académie, but the commission appointed by the Académie to evaluate it, which included Cauchy, Fourier, Laplace, and Poisson, had not been able to reach an agreement. Eventually a new commission, which excluded Poisson, was appointed and Cauchy's report was approved by the Académie in June 1825. According to Belhoste, this affair led to a deterioration in the relationship between Cauchy and Poisson.

in 1825 wrote his doctoral thesis on the subject of celestial mechanics. Apparently, Buniakowskii made no original contribution, but limited himself to a skillful application of Cauchy's calculus of residues to various problems including the determination of the radius vector in the elliptic movement of planets.³ According to Cauchy, however, neither Ostrogradskii's work nor any of the other published memoirs "has fixed the degree of generality" which is involved in a definite integral between imaginary limits.⁴

Apparently led by analogy, Cauchy extended the definition of (3.1) to the complex case by resorting to the calculus of variations and the theory of singular integrals, without worrying too much about the meaning one had to give to the symbol $f(z)$, when z was a complex variable: "In order to include in the same definition integrals taken between real limits and integrals taken between imaginary limits", Cauchy (1825a, 42–43) argued, "it is convenient to represent by the notation

$$\int_{x_0+iy_0}^{X+iY} f(z)dz \quad (3.2)$$

the limit, or one of the limits, to which the sum of the products of the form

$$\sum_n (x_n - x_{n-1} + i(y_n - y_{n-1})) f(x_{n-1} + iy_{n-1})$$

converges" with increasing n .

The sequences of values (x_n) and (y_n) could be easily obtained by considering two real functions

$$x = \phi(t), y = \chi(t), \quad (3.3)$$

that are monotonic and continuous in the interval $[t_0, T]$ and such that

$$\phi(t_0) = x_0, \phi(T) = X; \chi(t_0) = y_0, \chi(T) = Y,$$

and the values

$$\phi(t_i) = x_i, \chi(t_i) = y_i,$$

correspond to $t_0 < t_1 < t_2 < \dots < T$.

By substituting this into (3.2) Cauchy obtained

$$A + iB = \int_{t_0}^T [\phi'(t) + i\chi'(t)] f[\phi(t) + i\chi(t)] dt, \quad (3.4)$$

i.e. an integral of an imaginary function between real limits.

³The Russian translation of Buniakowskii's Dissertation was published in 1985 (see *Istor.-Math. Issled.* 28, 247–2609 with a commentary by Kirsanov (1985). See also Ermolaeva (1985).

⁴As Stäckel (1900, 73) pointed out, after his return to Russia Ostrogradskii dealt with the theory of integrals with imaginary limits in a note read at St. Petersburg Academy on October 29, 1828 and published 3 years later (Ostrogradskii 1831).

Under the hypothesis that $f(x + iy)$ is “finite and continuous” between the bounds of integration Cauchy demonstrated using the calculus of variations that the value $A + iB$ of the integral (3.4) “is independent of the nature of the functions $\phi(t), \chi(t)$ ” (Cauchy 1825a, 44).⁵ More precisely, Cauchy considered infinitesimal variations εu and εv of the functions $\phi(t)$ and $\chi(t)$, ε being an infinitely small number and $u(t) = v(t) = 0$ for $t = t_0$ and T . Then he developed the corresponding variation of the integral (3.4) in power series of ε and proved by integration by parts that the coefficient of ε given by

$$\int_{t_0}^T [(u + iv)(x' + iy')f'(x + iy) + (u' + iv')f(x + iy)]dt$$

is equal to 0. This amounts to saying that the variation of the integral equals 0.

In this form Cauchy stated the integral theorem that is nowadays named after him. In view of the importance of this result for the subsequent development of the theory, it is worth noting that this way of proving it was to be abandoned. It is also worth remarking that when stating the theorem Cauchy referred to continuous functions of an imaginary variable, a concept that cannot be found explicitly defined in any of his previous papers. Apparently by this he meant that the function was single-valued and had no points of infinity. One might suppose that he implicitly extended the definition of continuity given in his *Cours* for functions of two (real) variables to the function $f(x + iy)$, as he had suggested in Cauchy (1823b, 330). In addition, in his proof Cauchy tacitly assumed the existence and continuity of the derived function $f'(x + iy)$.

In this connection Remmert (1991, 196) has remarked, following Kline (1972, 637), that “this reflects the conviction, going back to the Euler tradition and also held by Cauchy – at least in the early years of his work – that continuous functions are perforce given by analytic expressions and are therefore differentiable according to the rules of the differential calculus”.

However, we cannot agree with Kline's and Remmert's claims. Even Cauchy's early writings show that he did not share Euler's conception of a continuous function, and a fortiori there is no evidence that Cauchy did so as late as 1825. On the contrary, his counter-example $f(x) = e^{-1/x^2}$ should have made him aware of the care needed in dealing with analytic expressions. Instead, one could argue that Cauchy tacitly assumed that a (continuous) function of an imaginary variable “generally” satisfies the (Cauchy–Riemann) equations. Such an attitude can be found, for instance, not only in his (1818) mentioned above but also in Cauchy (1825c, 346). There he proved a theorem that he had mentioned in a footnote he had just added to his 1814 *Mémoire* (see Cauchy 1814, 422–423). He considered the

⁵As we will see, the phrases “finite and continuous” or “continuous and finite” were to be used repeatedly by Cauchy to refer to a class of appropriate complex functions that he was unable to characterise more precisely until the late 1840s.

integral $\int_{-X}^X f(x)dx$, where $f(x)$ is a “finite and continuous” function, and remarked that when replacing x by $x + iy$ “generally” one has:

$$\frac{\partial f(x + iy)}{\partial y} = \frac{\partial f(x + iy)}{\partial x} i,$$

i.e. the complex form of the Cauchy–Riemann equations.

On the assumption that the function $f(x + iy)$ becomes infinite for values $x = a, y = b$ corresponding to the value $t = \tau$, [Cauchy \(1825a, 45\)](#) considered the limit

$$\mathbf{f} = \lim_{x \rightarrow a, y \rightarrow b} (x - a + i(y - b))f(x + iy)$$

or, “without noticeable error”

$$\mathbf{f} = \varepsilon f(a + ib + \varepsilon)$$

ε being an infinitely small number.

As he had done before, he considered a small variation of x and y . In order to evaluate the corresponding variation of the integral (3.4) he resorted to the theory of the principal value of an integral and proved that this value is $2\pi i\mathbf{f}$.⁶ Then he provided a “method” for obtaining the same result, by splitting the integrand into two parts

$$f(z) = \frac{\mathbf{f}}{z - a - ib} + \omega(z),$$

where $\omega(z)$ does not become infinite at $z = a + ib$ and, therefore, its contribution to the value of the integral is zero. This device, which was apparently used by Ostrogradskii in his unpublished (1824), was also used by Cauchy in the case of multiple infinities (or, in modern terms, of a pole of order m) to prove the formula that he had already published in 1823:

$$\mathbf{f} = \frac{f^{(m-1)}(a + ib)}{1 \cdot 2 \cdot 3 \cdots (m-1)}$$

or also

$$\mathbf{f} = \frac{1}{1 \cdot 2 \cdot 3 \cdots (m-1)} \frac{d^{(m-1)} \varepsilon^m f(a + ib + \varepsilon)}{d\varepsilon^{(m-1)}}.$$

It was then a matter of easy calculation to extend this to the case of a finite number m of (simple) infinities for the function. Accordingly, the variation of the integral was given by $2\pi i \sum_{j=1}^m \mathbf{f}_j$.

⁶Cauchy actually wrote $\pm 2\pi i\mathbf{f}$. Not having introduced the idea of a path in the complex domain and parameterisations of it, he had carefully to make precise how the sign had to be taken.

Integration between imaginary limits provided the conceptual framework for all the results on integration that Cauchy had obtained since 1814. In the second part of his *Mémoire* he showed how they could be derived from his fundamental formulae by means of appropriate transformations and changes of variables.

In modern terms, Cauchy's integral theorem is stated in various ways, all of them referring to its *geometrical* content.⁷ It could even be said that the theorem has no meaning without the geometrical interpretation of complex numbers (and variables). Cauchy's conditions (3.3) can properly be thought of as the parametric equations of a curve in the (x, y) plane joining the points (x_0, y_0) and (X, Y) . Accordingly, to any curve is associated a particular value of the integral (3.4). If one considers two different paths within the rectangle $(x_0, y_0), (X, Y)$ and the function $f(x + iy)$ does not become infinite for values (x, y) lying within the domain included by the paths, then the corresponding values of the integral (3.4) are equal, i.e. the integral itself is independent of the path of integration. This was actually stated by Cauchy (1825a, 55–58), who suddenly resorted to geometric language and restated his results in geometric terms by observing, in particular, that a path may be made up of parts defined by different functions.

Certainly one might argue that in so doing, Cauchy implicitly suggested associating the complex value $x_0 + iy_0$ with the real point (x_0, y_0) (and analogously a path in the complex plane with a path in the ordinary plane). In our opinion, however, referring to the real (x, y) plane, as Cauchy did, does not mean that he thought of the complex plane, and referring to pairs of real numbers (x_0, y_0) and to pairs of real functions $\varphi(x, y)$, $\psi(x, y)$ does not mean that he thought of the complex variable $(x_0 + iy_0)$ and of the complex function $f(z)$.⁸

In the autumn of 1825 Cauchy published a paper in Gergonne's *Annales* which "is essentially a pendant" to Cauchy (1825a), as Smithies (1997, 106) put it. There Cauchy returned to the formula

$$\int_{-\infty}^{\infty} f(x)dx = 2\pi i(f_1 + f_2 + \cdots + f_n)$$

and proved it under more appropriate conditions about $f(x)$ at infinity than the ones he had assumed in his *Résumé* and in his (1823b) (where he had supposed that $f(x + iy)$ vanishes when $x = \pm\infty$ for all y , and when $y = +\infty$ for all x). In particular he discussed in some detail the case where $1/f(x) = 0$ has multiple roots in the upper half-plane. In the second part of the paper (Cauchy 1826b) he applied his results to the evaluation of a large number (almost 200) definite integrals. Among his formulae one finds

⁷Thus, for instance, in the homology form it is stated as follows: Let Ω be a simply connected open set in $\mathbb{C}$. If f is a holomorphic function in Ω , then $\int_{\gamma} f dz = 0$ for every closed curve γ which is homologous to zero in Ω .

⁸Therefore, we cannot agree with Kline's (1972, 637) claim that "here $x + iy$ is definitely a point of the complex plane and the integral is over a complex path".

$$f(r) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{f(e^{ip})}{1 - re^{ip}} dp \quad (0 < r < 1),$$

which, as [Smithies \(1997, 110\)](#) remarked, “is essentially” Cauchy’s formula for $f(z)$ at an arbitrary point $z = r$ within the unit disk. We agree with Smithies’ statement that “it does not appear, however, that he was as yet thinking in terms of integration round a closed curve”.

In our opinion, indeed, Cauchy’s poor understanding of the geometrical structure of the complex field prevented him for a long time from fully realising the importance of his integral theorem, which today belongs to the basic theorems of complex function theory. Strange as it may seem for a man like Cauchy, who seldom missed an occasion to quote his own papers again and again and to claim priority over his contemporaries, he apparently neither used nor quoted from his 1825 paper and his integral theorem for decades. In relation to this “utterly strange and hard to explain” circumstance [Freudenthal \(1971, 139\)](#) asked: “Did Cauchy not trust the variational method of proof? Was he bothered by the (unnecessary) condition he had imposed on the paths, staying within a rectangle? Did he not notice that the statement could be transformed into the one about closed paths that he most needed? Or had he simply forgotten about that *Mémoire détaché*? In any case, for more than 25 years he restricted himself to rectangular paths or circular–annular ones (deriving from the rectangular kind by mapping), thus relying on the outdated 1814 *Mémoire* rather than on that of 1825”. In our view the paradox arises only when reading history with modern lenses, as one is easily led to do on interpreting Cauchy’s 1825 paper according to modern standards. As a matter of fact, Cauchy was to take up the topic again only in 1846, more than 20 years later, and even then he still stuck to the theory of imaginaries as symbolic expressions that he had presented in the *Cours d’analyse* (see Sect. [3.5.7](#)).

3.3 The *Exercices de mathématiques*

On January 31, 1825, one month before he presented his memoir on definite integrals between imaginary limits, Cauchy had presented another memoir to the Académie.⁹ Cauchy exploited what he saw as an analogy between the fact that in infinitesimal calculus when expanding $f(x + \varepsilon)$ one focuses on the coefficient of ε (i.e. the differential coefficient) in the expansion, while when expanding $f(x_0 + \varepsilon)$, where x_0 is a point of infinity of the function (a pole, in modern language) one has negative powers of ε , and in this “new” kind of calculus one focuses on the coefficient of $\frac{1}{\varepsilon}$. Without any further explanation, he called this coefficient the “residue” of the function at that point and showed that it could be determined in essentially the same way he had done in his 1825 paper. He also introduced the

⁹It was to be published as [Cauchy \(1826a\)](#).

special symbol $\mathcal{E}$ for indicating “the extraction of the residue” of a function (i.e. the expressions for $\mathbf{f}$ given above) and called the “integral residue” between given limits the sum of the residues corresponding to roots of $\frac{1}{f(x)} = 0$ whose real and imaginary parts lie between these limits. Then he established some formal properties of this calculus (e.g. $\mathcal{E}(f(x) + g(x)) = \mathcal{E}(f(x)) + \mathcal{E}(g(x))$) and applied it to some special examples. The memoir appeared in print more than one year later when Cauchy started publishing the *Exercices de mathématiques*, which functioned as something like a personal journal whose issues appeared monthly until 1830.¹⁰

3.3.1 The “New Calculus” of the Residues

From the very first issue onwards, Cauchy systematically developed the “new calculus” he had introduced and devoted a number of papers to its refinement.

In this respect, a paper (1827a) presented to the Académie on November 5, 1827 and published in the November 1827 issue of the *Exercices* is of particular interest. There Cauchy once more took up the geometric language he had occasionally used in his (1825a) *Mémoire* and went a step further by considering integrals taken along closed curves, with circles as special cases. He began by recalling a result he had already obtained in his (1826c, d): Let $f(z)$ be a function vanishing for infinite (real or imaginary) values of z . If $zf(z) \rightarrow F$ for $z \rightarrow \infty$, then the integral residue of $f(z)$ is $\mathcal{E}(f(z)) = F$.

In order, as he put it, “to fix the meaning” of this formula, Cauchy first considered the case in which the equation $\frac{1}{f(z)} = 0$ has infinitely many roots. Then the integral residue denotes the limit of the sum of partial residues of $f(z)$ for an increasing number of terms. As an example he considered the function $f(z) = \frac{\pi \cos \pi z}{z \sin \pi z}$ whose residues are $\pm 1, \pm \frac{1}{2}, \pm \frac{1}{3}, \dots, \pm \frac{1}{n}, \dots$, and a double pole at the origin with residue zero. Then, the integral residue $\mathcal{E}(f(z)) = \mathcal{E}\left(\frac{\pi \cos \pi z}{z \sin \pi z}\right)$ will have “an indeterminate value” because the sum of the series of the partial residues will be finite or infinite, positive or negative depending on the order in which the terms of the series are taken.¹¹ Turning to the general case Cauchy observed that the equation $\frac{1}{f(z)} = 0$ may well have imaginary solutions. Any imaginary number, he stated, can be written as $z = re^{pi}$ where r is the modulus and $-\pi \leq p \leq \pi$. The real variables r and p represent polar coordinates, namely “the radius vector of a mobile point” with a fixed origin and forming an angle p with a fixed axis through the origin. In this geometrical

¹⁰In his letters from Paris Abel wrote that during his stay there in 1826 he bought them and read them “assiduously”. By that time Abel had completed “a long paper on a certain class of transcendental functions”, which is the paper containing Abel’s theorem (see Sect. 4.4). “I showed it to Cauchy, but he scarcely wished to glance at it. And I dare to say without bragging that it is good. I am anxious to know the judgement of the Institut [de France]” Abel wrote confidently in his letters (see Abel 1902, 46)—but he would wait for it in vain.

¹¹Apparently, this is the first occurrence in print of the concept of conditionally convergent series.

setting Cauchy considered a closed curve (or a closed contour) around the origin, whose points recede further and further from the origin, and the residues of $f(z)$ at points (r, p) corresponding to solutions of $\frac{1}{f(z)} = 0$ lying inside the varying curve. The limit of the sum of the partial residues as the curve moves towards infinity could depend on the form of the curve, and the integral residue $\mathcal{E}(f(z))$ will generally be indeterminate. In order to avoid any indeterminacy, instead of arbitrary curves Cauchy considered circles centred at the origin with radii R . The corresponding sum of partial residues for increasing R could converge toward a well-determined limit which, if it exists, is a particular value of the integral residue, which Cauchy called its *principal value*. He went back to the example of the function $f(z) = \frac{\pi \cos \pi z}{z \sin \pi z}$ to state that its integral residue is indeterminate whereas the principal value of it is zero. Then he stated that in the formula $\mathcal{E}(f(z)) = F$ the integral residue $\mathcal{E}(f(z))$ should “always be reduced to its principal value”. The remainder of the paper is devoted to the proof of this statement and to illustrative examples.

There is hardly any doubt that in this paper Cauchy showed that he was well aware of the geometrical interpretation of complex number and variables. But it is also true that this awareness plays no essential role in Cauchy’s paper. Apparently, the use of geometrical language merely had the instrumental role of offering the reader an intuitive “picture” of what is at stake, leaving to analysis the duty of providing a rigorous proof of the relevant “fundamental propositions”.

In his (1826a) Cauchy stated that the theory of residues could be applied to many types of questions, including Lagrange’s interpolation formula, the splitting of rational functions, and the integration of linear differential equations. A series of papers published by Cauchy over the next few years proved that this was not an empty claim.¹² Of particular interest are the applications of the calculus of residues to the evaluation of integrals and infinite series,¹³ in particular Fourier and Lagrange series. As for the convergence of the Fourier expansion of a function $f(x)$, the “proof” he presented to the Académie on February 27, 1826 (Cauchy 1827b) is an interesting mixture of new results and old ideas, for he used both the theory of residues and a comparison “test” for the convergence of series, which amounts to saying that if the series $\sum w_n$ is convergent and $w_n \rightarrow v_n$ for $n \rightarrow \infty$ then the series $\sum v_n$ will also be convergent.¹⁴ At the end of that year, Cauchy returned to this question in a memoir on the application of the calculus of residues to mathematical–physical problems, which he presented to the Académie on December 26, 1826 and had printed as a separate pamphlet in February 1827 (Cauchy 1827c).¹⁵

The start of his *Exercices*, together with many papers he presented to the Académie in 1826, makes that year a very significant one in Cauchy’s scientific activity. So it is interesting to see how educated people reacted to his vigorous

¹²See, e.g., *O.C.* (2) 6, 252–255, 316–319; 7, 40–54; 255–266.

¹³See, e.g., *O.C.* (2) 7, 324–344; 345–362

¹⁴Dirichlet (1829) gave a counter-example that showed that this “criterion” is false.

¹⁵The very same year, on September 17, he presented to the Académie a second memoir on the same subject, see Cauchy (1827d).

mathematical productivity. Unfortunately, the reaction seemed to have been rather poor. In a survey paper, Ferry observed¹⁶ for instance that

We will only announce to geometers a dozen memoirs by M. Cauchy on various questions in mathematical analysis. Without doubt the scholarly and industrious author of these writings does not count on many readers; a general desertion seems to condemn his works to disuse. He might as well content himself with knowing, and write nothing at all. The coldness in the mathematical public, which is not however the subject of a whim, is not a matter of indifference: if it was possible to know the motive, whatever it might be, one would know something more about the methods of the sciences or the profession of the scholar. It is not perhaps the first time that a remarkable and ever-active talent has entirely wasted its forces and its time – a strange phenomenon that one notices with regret.

This supports Abel’s observation in October 1826 that Cauchy “is the only one who at present works in pure mathematics; Poisson, Fourier, Ampère, etc., are exclusively occupied by magnetism and other physical theories”.¹⁷

3.3.2 Lagrange Series

As for the Lagrange series, Cauchy had made a rather cryptic allusion to it in his (1824). He first seriously tackled it in a couple of papers¹⁸ submitted to the Académie on September 3, 1827 but published only 2 years later under a different title (Cauchy 1829a,b). Apparently, this was the first time that Cauchy showed an interest in celestial mechanics.¹⁹ He referred to recent works by Laplace (1827, 1828) as the source of his interest in Lagrange’s series. Indeed, this series had been applied in astronomy in connection with the solution of Kepler’s equation (see below), in order to the expand both the radius vector of the orbit of a planet and the true anomaly in power series in the eccentricity. Although this was its most successful application, the series, contrary to Arnold’s claim (1988, 49), had not been introduced by Lagrange in order to solve Kepler’s equation. Actually, the series had been first introduced by Lagrange in a *Mémoire* on the solution of algebraic equations, read to the Berlin Académie on January 18 and April 5, 1770, where he presented a method for expanding the roots of “literal” (i.e. algebraic) equations of the form $0 = a - bx + cx^2 - dx^3 + \dots$ in series. In spite of his claim, his method is far from being “very simple” and amounts to pages and pages of rather tedious and cumbersome substitutions and calculations culminating in the following theorem: Let p be a root of the equation $\alpha - x + \varphi(x) = 0$, to which the given equation

¹⁶ *Revue encyclopedique* 35 (1827) 311–312.

¹⁷ Abel to Holmboe, in Ore (1957, 147).

¹⁸ They had the explicit titles “Sur la détermination du reste de la série de Lagrange par une intégrale définie” and “Règles de convergence de la série de Lagrange et d’autres séries du même genre”.

¹⁹ One could perhaps recall Buniakowski’s 1825 Dissertation on the application of the calculus of residues to various topics of celestial mechanics, a subject which had likely been suggested by Cauchy.

$0 = a - bx + cx^2 - dx^3 + \dots$ can easily be reduced. Then any function “whatever” $\psi(p)$ can be expressed as

$$\psi(p) = \psi(x) + \varphi(x)\psi'(x) + \frac{1}{2} \frac{d[\varphi^2(x)\psi'(x)]}{dx} + \sum_{n>2} \frac{1}{n!} \frac{d^{n-1}[\varphi^n(x)\psi'(x)]}{dx^{n-1}}$$

provided one puts $x = \alpha$ after differentiation.

In the final sections of the paper he produced a “proof” of the convergence of his series based on the “criterion” that the convergence requires “the series be convergent at its extremity, namely that its last terms be infinitesimals” (1770, 60).

Some months later he applied his theorem to the solution of Kepler’s problem in a paper (Lagrange 1771) read to the Berlin Académie on November 1, 1770. This problem in planetary theory amounts to determining the eccentric anomaly of a planet (or a suitable function of it) in terms of the eccentricity of the orbit. In analytical terms this reduces to solving Kepler’s equation

$$t = x - c \sin x, \quad (3.5)$$

where t and c , the mean anomaly and the eccentricity, respectively, are known quantities. Very unusually for Lagrange, in (1771) he obtained (3.5) by resorting to a geometrical argument based on a drawing of a semi-ellipse inscribed in a semi-circle in order to get the elements of the orbit. In the paper he first expanded x and the radius vector r in Lagrange series, then in trigonometric series with (what we would now call) Bessel coefficients, adding that their convergence requires the eccentricity to be “very small” (1771, 136). Lagrange then summarised his work on the problem in the *Mécanique analytique* (Lagrange *Oeuvres*, 12, 34–41), re-obtaining (3.5) and the related results (see e. g. Arnold 1988, 49–55).

Lagrange presented the same formula in Note XI of his (1798, 281), where he referred to the “direct and elegant proof” of it given by Laplace (1780, 327–330; 334–335) that Lagrange admitted he himself had adopted (but without mentioning Laplace) in his *Théorie*.²⁰ There Laplace (1780, 334–335) had presented (without proof) a theorem which he claimed to be worthy of the attention of analysts for “its generality and simplicity”. Let $\varphi(x, \alpha) = 0$, let $u(x, \alpha)$ be the function to be developed in power series in α , and suppose that $x = a$ is a root with multiplicity j of the equation $\varphi(x, 0) = 0$, then, said Laplace, the coefficient q_n of α^n ($n \geq 1, q_0 = u(a, 0)$) in the expansion will be given by

$$q_n = \frac{1}{n!} \frac{\partial^n u}{\partial \alpha^n} - \frac{1}{(n-1)!j} \frac{\partial^{n-1}}{\partial x^{n-1}} \left\{ \frac{(x-a)^n}{n!} \frac{\partial^n}{\partial \alpha^n} \left[\frac{\partial u}{\partial x} \log \varphi(x, \alpha) \right] \right\},$$

provided that one supposes $\alpha = 0$ after differentiating with respect to α , and $x = a$ after all differentiations. Laplace had then applied this to the particular case

²⁰Lagrange (1797, § 99, 104–105; 1813, § 33, 69–70).

$\varphi(x, \alpha) = x - a - \alpha z(x)$ and u a function of x only. Thus $\frac{\partial u}{\partial \alpha} = 0$, $j = 1$ and the expression above for q_n reduces to

$$q_n = \frac{1}{n!} \left\{ \frac{\partial^{n-1} \left(z^n \frac{\partial u}{\partial x} \right)}{\partial x^{n-1}} \right\}_{x=a},$$

which is the general term of the Lagrange series that Laplace had previously obtained in Sect. VII of his (1780, 327–330).²¹

The problems of the convergence of the expansions of the radius vector and the true anomaly in the Lagrange series were tackled by Laplace in 1825. Assuming that the mean anomaly t is $\frac{\pi}{2}$ he was able to prove that the series expansion for the radius vector R

$$R = 1 + \frac{c^2}{2} - c \cos t - \frac{c^2}{2.1} \cos 2t + \frac{c^3}{2.1.2^2} (3 \cos 3t - 3 \cos t) - \dots$$

is convergent for $c < 0.66195$, c being the eccentricity.²²

Up to this point, complex variables and functions played no role at all. They first entered the picture with the work of Cauchy, who, “struck”, as he said he was, by Laplace’s “remarkable result” (Cauchy 1829a, 29–30), tackled the general question of the convergence of Lagrange series, as well as of the series he himself had obtained, by means of the calculus of residues. In the first section of the paper he studied the “approximate determination”, i.e. the expression for large values of n of the integral

$$S = \int_{x_0}^{x_1} u^n v dx,$$

where u and v are real or imaginary functions of x . Then in Sect. II he turned to the expression

$$S_n = \frac{1}{m!} \frac{d^m [\varphi(t) \varpi^n(t)]}{dt^m},$$

which can “evidently” be written as

$$S_n = \frac{1}{2\pi} \int_{-\pi}^{\pi} r^{-m} e^{-ims} \varphi(t + re^{-is}) [\varpi(t + re^{-is})]^n ds$$

²¹Lagrange series continued to attract the attention of mathematicians for decades, as is shown, for instance, by Heine’s one-page derivation of it (Heine 1857).

²²Later on, this result was reobtained by Puiseux following Cauchy’s methods of complex analysis, in a note added to the 3rd edition of Lagrange’s *Mécanique*, see *Oeuvres* 12, 341–346. Unknown to Cauchy, and possibly to Laplace, in 1817 the Italian astronomer Francesco Carlini had announced a thorough investigation of Kepler’s equation, which included Laplace’s result for the convergence of Lagrange series and a remarkable study of Bessel coefficients for large numbers. The resulting publication (Carlini 1818), however, was flawed by some mistakes that were corrected by Jacobi who published a German translation of it in 1850.

for any r . By putting $m = n\mu$ the latter integral can be transformed in a form to which the results of Sect. I apply. They were summarised by Cauchy in the theorem: the series whose general term is

$$S_n = \frac{1}{m!} \frac{d^m [\varphi(t)\varpi^n(t)]}{dt^m}$$

m, n being large numbers such that $\frac{m}{n} = \mu$ is finite, is convergent or divergent according as the “principal modulus” of the function $\psi(x) = \frac{\varpi(t+x)}{x^\mu}$ is less than or greater than 1. (Cauchy called R the “principal modulus” of the function $\psi(x)$ if $R = |\psi(\rho e^{is})|$ is a maximum as s varies in $(0, 2\pi)$, $x = \rho e^{is}$ being a root of the equation $\psi'(x) = 0$.) Putting $m = n - 1$ for large values of m one obtains approximately $\mu = 1$ and, consequently, the series whose general term is

$$S_n = \frac{1}{(n-1)!} \frac{d^{(n-1)} [\varphi(t)\varpi^n(t)]}{dt^{(n-1)}}$$

is convergent or divergent according as the “principal modulus” of the function $\psi(x) = \frac{\varpi(t+x)}{x}$ is less than or greater than 1. Cauchy remarked that $\frac{S_n}{n}$ is the general term of the Lagrange series which represent the value of $\int \varphi(z)dz$, where z is a root of the equation $z = t + \varpi(z)$, and as his first example he applied this to Kepler’s equation $z = t + c \sin z$, thus re-obtaining Laplace’s result. In the final Section III of the paper Cauchy applied his results to special cases.

On the very same day, September 3, 1827 Cauchy presented to the Académie a second memoir (1829b) devoted to Lagrange’s series. There he considered a root ζ of the equation $z - x - h\varpi(z) = 0$ and stated that

$$f(\zeta) = \mathcal{E} \frac{1 - h\varpi'(z)}{((z - x - h\varpi(z)))} f(z)$$

the symbol $\mathcal{E}$ being related only to the root one considers. After observing that

$$\frac{1 - h\varpi'(z)}{z - x - h\varpi(z)} = \frac{\partial \log(z - x - h\varpi(z))}{\partial z},$$

he expanded $\log(z - x - h\varpi(z))$ in power series of $\frac{h\varpi(z)}{z-x}$ with a remainder term $\varphi(z)$. By differentiating both terms with respect to z and by taking the residues of both of them with respect to $z = x$ and $z = \zeta$ Cauchy eventually obtained

$$\begin{aligned} f(\zeta) &= f(x) + \frac{h}{1} f(x)\varpi(x) + \frac{h^2}{1.2} \frac{d[f'(x)\varpi^2(x)]}{dx} + \dots \\ &+ \frac{h^n}{n!} \frac{d^{(n-1)}[f'(x)\varpi^n(x)]}{dx^{(n-1)}} + \mathcal{E} \frac{h^{(n+1)}\chi(z)}{((z - \zeta)(z - x)^{(n+1)})} \end{aligned}$$

where

$$\frac{\chi(z)}{(z-\zeta)} = \frac{\varpi^n(z)[\varpi(z) - (z-x)\varpi'(z)]}{((z-x-h\varpi(z)))} f(z).$$

This enabled Cauchy to express the remainder of a Lagrange series in the form of a residue, which he transformed into an integral in the next section of the paper before dealing with some particular cases. As we will see, Cauchy resumed the study of Lagrange series a couple of years later, in the context of his new *calcul des limites*.

In 1829 he produced some more papers²³ on the application of the calculus of residues to the evaluation of infinite products, and extended the results of his (1827a) to the case of functions $f(z)$ such that $\frac{1}{f(z)} = 0$ has an infinite number of zeros for $|z| \leq R$ as, for example, $f(z) = \frac{\tanh az}{az} \tan \frac{b}{z}$. In addition, as the *Résumé* was out of print he published the *Leçons sur le calcul différentiel* (1829c), a textbook for his students at the École polytechnique, which, however, contained nothing that was not to be found in his published research papers.

3.4 Working Abroad in Self-exile

The year 1830 was to mark the start of a turbulent period in Cauchy's life as once again his personal and scientific life intertwined with the French political events. However, whereas 15 years before the Restoration had favoured his advance in the academic world, this time politics went the other way for him. The revolution of July 1830, which led to the end of the Bourbon monarchy in France, was a setback to Cauchy's academic and mathematical career.

In 1829 the Bourbon monarch Charles X, who had succeeded Louis XVIII in 1824, set up an ultra-conservative government. This provoked a crisis between the monarchy and the "liberal" movement, which had the majority in the Chamber, that culminated in July 1830 when the liberals gained victory in the elections. As a result, the government suspended the freedom of the press, abolished the newly-elected Chamber and announced changes to the election laws. The people in Paris reacted to this attempt at a coup d'état, and for "the Glorious Three" days the town fell into the hands of the insurgents. This July revolution, headed by the leaders of the wealthy, liberal bourgeoisie, led to the rise to power of Louis Philippe, Duke of Orleans, who was appointed King of the French on August 7. Charles X abdicated in favour of the Duke of Bordeaux, a grandson of his, and fled from Paris to Cherbourg where he embarked for England with the royal family.

Cauchy was deeply shocked by the events. The collapse of his beloved Bourbon monarchy was probably felt by him as an end of the world in whose rightness he deeply believed. The massive participation of his students from the

²³See *O. C.* (2) 2, 84–87; (2) 9, 196–209; 210–253.

École Polytechnique in the revolution, together with the insurgents' violent anti-catholicism and their attacks to his friends the Jesuits, may also have played a role in convincing him to leave the country. Be that as it may, in August 1830 Cauchy took his decision and at the beginning of September he made his way to Fribourg, Switzerland, leaving his family in Paris. There is no definite evidence to explain Cauchy's voluntary exile. However, Belhoste (1991, 145) is right when stating that facts contradict Valson's claim that Cauchy left France after having lost his teaching positions for refusing to swear an oath to the new régime. Actually, the latter was required only after Cauchy had left his country.

According to Belhoste, Cauchy probably had no idea at first that his absence from France would last so long. He was physically and intellectually exhausted by his exceptional scientific activity in the previous years. Belhoste lists an impressive number of publications produced by Cauchy in the 2 years preceding the July revolution, including two books, some 25 issues of his *Exercices de mathématiques*, and no less than 40 papers presented to the *Académie* and to Ferussac's *Bulletin*. He needed a period of rest, and this was the justification his family initially gave to the institutions which required him to meet his teaching duties. After a few months, however, because of his lasting absence from Paris he lost all of his teaching positions.

Having left France under condition of self-exile, Cauchy first settled in Fribourg, where he planned to create an *Académie Helvétique*, financially supported by the most reactionary courts of Europe. To this end he first asked for support from some of the most reactionary Italian rulers, such as Carlo Felice, the King of Sardinia and Ferdinando IV Duke of Modena, who had refused to recognise King Louis Philippe. In the early October 1830 Cauchy, then in Turin, appealed to the Emperor of Austria. By the end of that month he was in Modena, where he gained the promise of support from the Duke, and from there Cauchy addressed the Tsar of Russia. Apparently Cauchy succeeded in obtaining a promise of support from Pope Pius VIII, but there is no evidence of any reaction to his letters from the Courts of Vienna and Petersburg. On his way to Genoa to meet Carlo Felice and gain his support, he spent some days in early November in Milan and Pavia where he got in touch with the catholic, scientific, and intellectual milieu that included the mathematicians Gabrio Piola and Antonio Bordini and the writer Alessandro Manzoni. In December 1830, when the project seemed likely to be realised, the Pope died and, even worse for Cauchy's plans, a radical, political change took place in Fribourg that overthrew the reactionary local government and forced Cauchy to give up his planned *Académie*.

Very little is known about his life in the months that followed this setback. According to [Castella \(1927, 309\)](#) Cauchy was still in Fribourg in early March 1831, while Belhoste (1991, 274) has produced evidence that Cauchy attended a meeting of the Paris *Académie* on March 14. Soon thereafter he left Paris to settle again in Switzerland, and sometime in June he sent from Geneva to the Paris *Académie* a paper on the differential calculus and the calculus of variations which is recorded²⁴ as having been received on July 4. At the same time, Cauchy's main supporters

²⁴*Procès-verbaux des séances de l'Académie* 9 (1921), 657.

in Northern Italy experienced difficulties. In February 1831 the Duke of Modena, perhaps his most convinced supporter, was forced to leave his town and to repair to Mantua under the protection of Austrians because of a rising which soon involved Bologna and the Papal State. In addition, the King of Sardinia, Carlo Felice, died in April. Apparently, in autumn 1831 Cauchy moved to Northern Italy and eventually he settled in Turin where, at the suggestion of the Jesuits who were very influential at the Court, the new King Carlo Alberto offered him a chair of Higher Physics (*Fisica sublime*) at the University. This chair, formerly held by Amedeo Avogadro and suppressed by the King Carlo Felice after the rising of 1821, was re-established for Cauchy on January 5, 1832 (Terracini 1956–1957, 160).

3.4.1 Cauchy and the Italian Mathematical Milieu

Even before this tumultuous period Cauchy was well known to the Italian mathematical milieu, particularly at Modena where he had found in Ruffini a colleague very sympathetic to his religious views. Cauchy had presented him a copy of his *Cours d'analyse*, and he had reciprocated with a copy of his pamphlet (Ruffini 1821) opposing the materialistic philosophy expressed in Laplace (1814). In a letter to Ruffini²⁵ on September 20, 1821 Cauchy replied that he had been close to Laplace for a long time and even had obligations to him, but this notwithstanding, he had never concealed his principles, and indeed he had formally stated them in the introduction to the *Cours d'analyse*.

In the autumn of 1830 Cauchy introduced himself to Italian mathematicians with the paper (1830–1831) (in Italian) that was published in three parts between October 1830 and July 1831 in the journal *Biblioteca italiana*.²⁶ The paper was a reply to the review of his *Exercices de mathématiques* by Giuseppe Cossa whose tone was generally positive except for one remark. Cossa had observed that when dealing with the foundations of the calculus the greatest clarity is needed, and if an author cannot attain it, he must at least admit his failure openly. Cauchy felt challenged by this remark and reacted by claiming that in his works he had put a special emphasis on the need for rigour.

The paper (Cauchy 1830–1831) was a manifesto of his approach to “modern” analysis, as he called it. At the same time it was an open attack on the style of analysis practised by Lagrange and Laplace. As for rigour, after quoting almost verbatim from the introduction to his *Cours* he summarised its main features and those of the *Résumé* as well. In particular, he criticised Lagrange’s use of Taylor expansions as a foundation for the calculus and, in this connection, he referred to his 1822 counter-example $y = e^{-\frac{1}{x^2}}$. The same lack of rigour could be found in the Lagrangian calculus of variations. All in all, Cauchy concluded, Lagrange’s

²⁵See Ruffini, *Op. mat.* 3, 89

²⁶This paper was later reprinted as a separate pamphlet in Rome in 1843.

approach was even less rigorous than Leibniz's and Newton's. There is hardly any doubt that Cauchy was well aware of the impact that his provocative manifesto would have among the Italian mathematicians deeply attached, as they were, to the Lagrangian tradition. And indeed it did provoke. Antonio Bordini, the most authoritative mathematician at the university of Pavia, and perhaps the most deeply rooted in the Lagrangian approach to calculus and mechanics²⁷ limited himself to commenting in his letters to Piola that (Cauchy 1830–1831) had done nothing except “speak badly of Lagrange”. By the autumn of 1830 Bordini was working on the edition of his lectures on calculus (Bordini 1831) which were openly inspired by Lagrange's *Théorie*. He was helped by Piola, a friend of his since their student days,²⁸ who shared his Lagrangian view and complained in his letters of being “submerged by a deluge of infinitesimals” on reading Cauchy's *Exercises*. Although he recognised that he had also been occasionally struck by Cauchy's “flashes of sublime ingenuity”, he refused to call all this material “modern analysis”, as he himself heard it on Cauchy's lips, because in his view the modern theories proclaimed by Cauchy originated ultimately from memoirs by Euler, Lagrange, and Laplace. Like Cauchy, Piola had deep religious convictions and intertwined his mathematical work with apologetic writings in support of the Catholic religion. Nonetheless it comes as no surprise to know that he appreciated the mathematical work of Poisson, whom he called “the new Lagrange of France”, much more than Cauchy's (see Bottazzini 1994, 41–56).

3.4.2 *The Turin Memoirs*

Cauchy experienced an openly hostile reaction to his views on October 11, 1831 when he presented the Turin Accademia delle Scienze with a paper on the existence of power series expansions of functions, where he introduced his new “calculus of limits” (*calcul des limites*). As for the source of this term, one could argue that Cauchy borrowed it from (Laplace 1828, 470), giving it a different and more precise meaning, and indeed Cauchy referred to his “attentive reading” of that paper in the introductory pages of his *Mémoire* (1831a, 160). There Laplace had referred to the methods expounded in his (1812) for finding rapidly converging approximations of

²⁷The official Regulations of the Universities of Padua and Pavia banished the use of infinitesimals from the teaching of calculus in favour of the theory of derived functions. Bordini had graduated in architecture and engineering from Pavia in 1807 and spent all his life as a professor there. He was the author of a number of paper and treatises in applied mathematics and engineering which reveal his great mathematical abilities. Among his students he had some of the most brilliant Italian mathematicians of the Risorgimento generation, including Mainardi, Codazzi, Brioschi, Cremona, Beltrami and Casorati. For his obituary see Casorati, *Opere* (1, 53–59).

²⁸A well-off nobleman Piola did mathematics as an amateur without looking for any academic position. He collaborated with the Milan Observatory and eventually became the President of the Istituto Lombardo di Scienze e Lettere.

integrals involving functions of large numbers. Those methods were also useful in the expansions of functions in series in order to find the *limits* (Laplace's emphasis) of the terms in the expansion and to establish whether the series is convergent. According to Laplace, the limits are the values which the terms of the series tend to attain for increasing index, and which eventually coincide at infinity.

Cauchy claimed that he was chiefly interested in emphasising the advantages which his calculus offered for "calculations which sometimes require several years of work" from astronomers (1831a, 158). This practical issue motivated his *Mémoire*, as he emphasised in the introduction to it that he quickly had lithographed on October 15 and then published in Ferussac's *Bulletin*.²⁹ He began by claiming that "the methods that geometers have used to deduce the movements of the celestial bodies from the principles of gravitation leave something to be desired. Often they lack sufficient rigour" (1831a, 158). As in his (1830–1831), his criticism was once again chiefly addressed to both Laplace's and Lagrange's lack of rigour. After summarising his critical remarks about their use of infinite series, and making a cryptic allusion to his counter-example $y = \exp(-1/x^2)$, Cauchy (1831a, 160) went on to state that "in particular one simply does not find an adequate derivation of Lagrange's formula in Laplace's *Mécanique céleste*, which, however, serves as the basis for almost all the theories expounded in that work", especially in the calculation of the perturbations in the movements of celestial bodies. In addition, these calculations were tremendously time consuming. He had discussed the matter with Plana, the Director of the Turin Observatory, he said (Cauchy 1831a, 159):

M. Plana having again spoken to me recently of the time that is consumed in similar calculations, I said to him that I was convinced that it will be possible to reduce them, and even to determine immediately the numerical coefficient corresponding to a given inequality. In actual fact, some days later I gave him some formulae by means of which one can solve similar questions, and of which I have already given an application to the determination of certain numbers which are useful when considering the theory of Saturn and Jupiter.

Cauchy hoped that the Italian geometers and astronomers would attach some value to his work, but—at least as far as Plana was concerned—his faith in its acceptance was misplaced. In fact, in the report that appeared in the minutes of the meeting one reads that "the reading of the paper was interrupted from time to time by several verbal discussions between the author and Mr. Plana" (quoted in Terracini 1956–1957, 186). A former student of Lagrange at the École Polytechnique, Plana certainly did not agree with Cauchy's critical remarks about Lagrange's and Laplace's lack of rigour. Moreover, in spite of Cauchy's claims, Plana apparently did not believe that Cauchy's "calculus of limits" offered any simplification of the astronomical calculations. As Menabrea recalled in his *Memorie*, Plana opposed "the new hyper-transcendental analysis of which Cauchy was the brilliant promoter, an analysis that provided a slew of inapplicable formulae" (in Peiffer 1978, 14).

²⁹Ten years later, he reprinted it once more in his reborn *Exercices*, see Cauchy (1841d).

However, unlike Plana, Piola soon changed his mind about Cauchy's analytical work. Together with his friend Paolo Frisiani he translated Cauchy's Turin *Mémoire* into Italian and published it with some 82 pages of editorial notes in the *Opuscoli matematici e fisici* that he edited (Cauchy 1834), adding to it a paper of his own on Cauchy's theory of residues (Piola 1834). He did this in agreement with Cauchy, with whom he exchanged letters about his editorial work and other matters to do with physics. At the same time, he also edited the Italian translation (1834) of the pamphlet (Cauchy 1833c) that had appeared in Prague in September 1833, prefaced by an apologetic "Preliminary discourse of an Italian mathematician" of his own.

3.4.3 The Calcul des limites

In his (1831a, 159) Cauchy only sketched the main content of his great *Mémoire* (1831b) that appeared in two parts in lithographic form sometime in 1832, to which he added an *Addition* on March 6, 1833. The first paragraph of Part I dealt with the variation of arbitrary constants in differential equations and its applications to problems of celestial mechanics. This was later published by Cauchy separately in Liouville's newly founded *Journal* (Cauchy 1837d). Here we are interested in § 2 where Cauchy tackled the problem of expanding (explicit and implicit) functions in power series and established methods for evaluating their remainders after a finite number of terms.³⁰ (Part II, amounting to some 90 pages entirely devoted to applications to celestial mechanics, substantiated his claim about the utility of his new calculus.)

He began by observing that $\int_{-\pi}^{\pi} e^{npi} dp = \int_{-\pi}^{\pi} e^{-npi} dp = 0$ if n is a positive integer, while $\int_{-\pi}^{\pi} dp = 2\pi$ for $n = 0$. Applied to a polynomial $f(x) = a_0 + a_1x + a_2x^2 + \dots + a_nx^n$ via the substitution $x = \tilde{x}$ with $\tilde{x} = Xe^{pi}$, the previous formulae yield $\int_{-\pi}^{\pi} f(\tilde{x}) dp = \int_{-\pi}^{\pi} f(\frac{1}{\tilde{x}}) dp = 2\pi a_0 = 2\pi f(0)$. This, Cauchy stated, can "easily" be extended to finite and continuous functions other than polynomials. As one has "generally"

$$\frac{\partial f(\tilde{x})}{\partial X} = \frac{1}{iX} \frac{\partial f(\tilde{x})}{\partial p}$$

(thus tacitly assuming that Cauchy–Riemann equations hold) integration of both sides of this equation with respect to X and p yields at once $\int_{-\pi}^{\pi} f(\tilde{x}) dp = 2\pi f(0)$.

Under the further assumption that $f'(\tilde{x}), f''(\tilde{x}), \dots, f^{(n)}(\tilde{x})$ are finite and continuous for $-\pi \leq p \leq \pi$ and $|\tilde{x}| \leq X$, by repeated integration by parts he obtained

³⁰This was later published by Cauchy (with variations that we will point out) in the second volume of his new *Exercices*. See Cauchy (1841e).

$$\int_{-\pi}^{\pi} \frac{f(\tilde{x})}{\tilde{x}^n} dp = \frac{1}{n} \int_{-\pi}^{\pi} \frac{f'(\tilde{x})}{\tilde{x}^{n-1}} dp = \cdots = \frac{1}{n!} \int_{-\pi}^{\pi} f^{(n)}(\tilde{x}) dp$$

and hence

$$\frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{f(\tilde{x})}{\tilde{x}^n} dp = \frac{1}{n!} f^{(n)}(0).$$

In particular, if $f(0) = 0$ it follows that $\int_{-\pi}^{\pi} f(\tilde{x}) dp = 0$. From the formulae above, Cauchy commented, one can “easily” obtain the sought-for power series expansions. By substituting $\tilde{x} \frac{f(\tilde{x}) - f(x)}{\tilde{x} - x}$ ($|x| < X, x \neq \tilde{x}$) for $f(\tilde{x})$ in the equation $\int_{-\pi}^{\pi} f(\tilde{x}) dp = 0$, and expanding the kernel $\frac{\tilde{x}}{\tilde{x} - x}$ in a convergent geometric series he obtained

$$\int_{-\pi}^{\pi} \frac{\tilde{x} f(\tilde{x})}{\tilde{x} - x} dp = \int_{-\pi}^{\pi} \frac{\tilde{x} f(x)}{\tilde{x} - x} dp = f(x) \int_{-\pi}^{\pi} \left(1 + \frac{x}{\tilde{x}} + \frac{x^2}{\tilde{x}^2} + \cdots \right) dp = 2\pi f(x)$$

and, eventually the “known formula”³¹

$$f(x) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{\tilde{x} f(\tilde{x})}{\tilde{x} - x} dp \quad (\tilde{x} = X e^{ip}).$$

This is the form in which Cauchy first presented his celebrated “integral formula”, which, he insisted, holds under the hypothesis that $f(x)$ is continuous and finite for $|x| \leq X$. (The nowadays usual form $f(z) = \frac{1}{2\pi i} \int_C \frac{f(\zeta)}{\zeta - z} d\zeta$, where $\zeta = r e^{i\varphi}$ describes the circumference C , follows at once.)

As the geometric series above is convergent for $|x| < X$, it follows from the “integral formula” that $f(x)$ can be expanded in a convergent power series of x provided that the modulus of the “real or imaginary” variable x is less than the least value at which $f(x)$ fails to be finite and continuous. Before re-stating this as the first part of his integral representation theorem, he mentioned $\cos x, \sin x$, and e^x as examples of functions which can always be expanded in convergent power series whereas functions like $e^{1/x}, e^{1/x^2}, \cos(1/x)$ become discontinuous at $x = 0$ and consequently have no expansion in power series.

Then he went on to remark that when $f(x)$ can be expanded in series one has for its general term

³¹Cauchy tacitly referred here to (2.35) the formula for the unit disk he found in (1822b), and re-obtained in (1826a).

$$\frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{x^n f(\tilde{x})}{\tilde{x}^n} dp = \frac{x^n}{n!} f^{(n)}(0)$$

and, therefore, the MacLaurin expansion

$$f(x) = f(0) + xf'(0) + \frac{x^2}{2} f''(0) + \dots$$

Cauchy's next step was to provide estimates for the general term and the remainder of this series. He denoted $|x| = \xi$ and by $\Lambda f(\tilde{x})$ the “limit”³² of $|f(\tilde{x})|$ under the condition $|\tilde{x}| = X$ as p varies ($-\pi \leq p \leq \pi$). Then he stated the following inequality today named after him:

$$\left| \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{x^n f(\tilde{x})}{\tilde{x}^n} dp \right| < \left(\frac{\xi}{X} \right)^n \Lambda f(\tilde{x}).$$

As the term on the right side is the general term of the expansion of $\frac{X}{X-\xi} \Lambda f(\tilde{x})$ in a geometric series, Cauchy argued that its remainder will dominate the remainder of the MacLaurin expansion of $f(x)$, that is

$$\left| \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{x^n}{\tilde{x}^{n-1}(\tilde{x}-x)} f(\tilde{x}) dp \right| < \frac{\xi^n}{X^{n-1}(X-\xi)} \Lambda f(\tilde{x}),$$

and he provided a direct proof of this. Next he summarised all this as the second part of Theorem I and extended his results to functions of several variables. In the remaining part of this section Cauchy turned to the application of his results to implicit functions and showed with examples how to determine the “limits” of the errors committed when disregarding the terms of the series so obtained after the first n .

He began³³ (1841e, 74) by considering an implicit function $y(x)$ defined by the equation $f(x, y) = 0$. He supposed that $y = b$ for a particular value of x , so that when $y = b + z$ the given equation becomes $f(x, b + z) = 0$. He further supposed that

1. For a given value of x the equation $f(x, b + z) = 0$ has a unique root z such that $|z| < Z$ for a suitable Z ;
2. For any such z the function $f(x, b + z)$ is finite and single-valued, and a given function $F(y) = F(b + z)$ remains “continuous and finite”.

Then, writing $\chi(x, y) = \frac{\partial f(x, y)}{\partial y}$, the assumption that $f(x, b + z) = 0$ implies that one has

³²This explains the name given by Cauchy to his calculus, which is known today as “method of majorants”. (We would denote $\Lambda f(\tilde{x}) = \sup |f(\tilde{x})|$, and $|\tilde{x}| = X$).

³³The following account of this part of Cauchy's 1831 *Mémoire* is based on the printed version (Cauchy 1841e) of it.

$$F(y) = \mathcal{O} \left(\frac{\chi(x, b+z)}{f(x, b+z)} F(b+z) \right)$$

on taking the integral residue with respect to z in the disk centred at the origin with radius Z . Next, by putting $\tilde{z} = Ze^{iq}$, he observed that $F(y)$ can be rewritten as

$$F(y) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \tilde{z} \frac{\chi(x, b+\tilde{z})}{f(x, b+\tilde{z})} F(b+\tilde{z}) dq.$$

After proving the last formula he observed that by putting first $F(y) = 1$, then $F(y) = y$ one obtains

$$y = \frac{1}{2\pi} \int_{-\pi}^{\pi} \tilde{z}(b+\tilde{z}) \frac{\chi(x, b+\tilde{z})}{f(x, b+\tilde{z})} dq$$

and eventually

$$y = b + \frac{1}{2\pi} \int_{-\pi}^{\pi} \tilde{z}^2 \frac{\chi(x, b+\tilde{z})}{f(x, b+\tilde{z})} dq,$$

which provides an explicit expression for the root $y = b + z$.

He went on to consider the case in which the equation $f(x, b+z) = 0$ has m (distinct or equal) roots $z_0, z_1, \dots, z_{m-1}$ such that $|z_k| < Z$. Denoting by $y, y_1, \dots, y_{m-1}$ the corresponding roots of $f(x, y) = 0$, and reasoning as in the previous case he obtained a formula for expressing the sum $F(y) + F(y_1) + \dots + F(y_{m-1})$. On putting $F(y) = 1$, then $F(y) = y$ he obtained in particular

$$m = \frac{1}{2\pi} \int_{-\pi}^{\pi} \tilde{z} \frac{\chi(x, b+\tilde{z})}{f(x, b+\tilde{z})} dq$$

which, as [Smithies \(1997, 165\)](#) has remarked, “is essentially the well-known formula for the number of zeros of an analytic function inside a circle”.

Next, Cauchy assumed that x varied. Accordingly, the roots of $f(x, y) = 0$ are functions of x , and he looked for expansions of the sum $F(y) + F(y_1) + \dots + F(y_{m-1})$ as a power series of x . To this end he first stated and proved the following theorem: under the assumptions that

1. Equation $f(0, b+z) = 0$ has m roots whose moduli are less than Z ;
2. The function $f(x, b+z)$ is finite and single-valued for $|z| < Z$ and $|x| < X$ for a suitable X ;
3. $\log \frac{f(x, b+\tilde{z})}{f(0, b+\tilde{z})}$ ($\tilde{z} = Ze^{iq}$) can be expanded in ascending power series of x ,

then $f(x, b+z) = 0$ has m roots z_k such that $|z_k| < Z$. To prove this he assumed the expansion

$$\log \frac{f(x, b + \tilde{z})}{f(0, b + \tilde{z})} = xu_1(\tilde{z}) + x^2u_2(\tilde{z}) + x^3u_3(\tilde{z}) + \dots$$

and differentiated it term-by-term with respect to $\tilde{z}$. After multiplying both members of the resulting equation by $\tilde{z}dq$, he integrated the result term-by-term with respect to q thus obtaining

$$\frac{1}{2\pi} \int_{-\pi}^{\pi} \tilde{z} \frac{\chi(x, b + \tilde{z})}{f(x, b + \tilde{z})} dq = \frac{1}{2\pi} \int_{-\pi}^{\pi} \tilde{z} \frac{\chi(0, b + \tilde{z})}{f(0, b + \tilde{z})} dq,$$

which is equal to m , as desired. On multiplying by $\tilde{z}F(b + \tilde{z})dq$ instead of by $\tilde{z}dq$ —where as usual $F(b + z)$ is finite and single-valued for $|z| < Z$ —and integrating the result term-by-term he was able to expand the sum $F(y) + F(y_1) + \dots + F(y_{m-1})$ as a power series in x . Then he generalised his results by replacing $F(y)$ by $F(x, y) = F(x, b + z)$ under the assumption that $F(x, b + z)$ is finite and continuous for $|z| < Z$ and $|x| < X$ (Cauchy 1841e, 82). The above formula for $F(y)$ becomes

$$F(x, y) + F(x, y_1) + \dots + F(x, y_{m-1}) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \tilde{z} \frac{\chi(x, b + \tilde{z})}{f(x, b + \tilde{z})} F(x, b + \tilde{z}) dq$$

which, because of the “known formula”

$$f(x) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{\tilde{x}f(\tilde{x})}{\tilde{x} - x} dp \quad (\tilde{x} = Xe^{ip})$$

can be rewritten as

$$F(x, y) + F(x, y_1) + \dots + F(x, y_{m-1}) = \frac{1}{4\pi^2} \int_{-\pi}^{\pi} \int_{-\pi}^{\pi} \frac{\tilde{x}\tilde{z}}{\tilde{x} - x} \frac{\chi(\tilde{x}, b + \tilde{z})}{f(\tilde{x}, b + \tilde{z})} F(\tilde{x}, b + \tilde{z}) dp dq.$$

By resorting to his calculus of limits Cauchy (1841e, 84) remarked that this can be expanded as a power series in x such that the coefficient U_n of x^n given by

$$U_n = \frac{1}{n!} \frac{1}{2\pi} \int_{-\pi}^{\pi} \tilde{z} \left[\frac{\partial^n}{\partial x^n} \left(\frac{\chi(x, b + \tilde{z})}{f(x, b + \tilde{z})} \right) F(x, b + \tilde{z}) \right]_{x=0} dq \quad (n > 0)$$

is such that $|U_n|$ is majorised by the coefficient of x^n in the expansion of

$$\frac{XZ}{X - x} \Lambda \left[\frac{\chi(\tilde{x}, b + \tilde{z})}{f(\tilde{x}, b + \tilde{z})} F(\tilde{x}, b + \tilde{z}) \right].$$

Then [Cauchy \(1841e, 85–90\)](#) discussed in detail the particular cases when either one or m roots of the equation $f(0, b + z) = 0$ lie at $z = 0$ before considering the case when $f(0, b + z) = 0$ has m roots lying within the annulus³⁴ $z_0 < z < Z$. He restricted himself to stating that even in this case the sum $F(x, y) + F(x, y_1) + \dots + F(x, y_{m-1})$ of the roots could be expanded in convergent power series for $|x| < X$ by resorting to “one of the fundamental formulae of the calculus of residues” ([Cauchy 1841e, 74](#))

$$\int_{-\pi}^{\pi} [\varphi(\tilde{x}) - \varphi(\tilde{x}')] dp = 2\pi \mathcal{E} \left(\frac{\varphi(x)}{x} \right),$$

where $\tilde{x} = Xe^{ip}$, $\tilde{x}' = x_0e^{ip}$, $\varphi(x)$ is a finite, single-valued function for $x_0 < x < X$, and the integral residue has to be taken between x_0 and X , $-\pi, \pi$. Commenting on this, [Smithies \(1997, 170\)](#) remarked that “although Cauchy at this point had in his hands all the tools that would have been needed to prove Laurent’s theorem, he did nothing in this direction”. As we will see in the next section, even later on Cauchy would come very close to Laurent’s theorem without stating it explicitly.

Instead of doing so, he turned to the application of his general results to equations of the form $f(x, y) = \Pi(y) - x\varpi(y)$ which he considered in some special cases. Of particular interest here is his treatment of Kepler equation $y = b + x \sin y$, where he showed that the root y of the equation can be expanded in convergent Lagrange series under the condition that $|x| < 0.662742\dots$ which slightly differs from the value 0.66195 given by Laplace (see Sect. 3.3.2). Eventually, in the concluding paragraphs Cauchy extended some of his results of functions of several variables.

As for the main Theorem I of this paper, we will see in the next sections that Cauchy had been concerned with its correct statement for some 20 years. In his *Cours* ([Cauchy 1821a, 135–136, 235](#)) he had shown how to determine the radius of convergence of a given (real or imaginary) power series. Now he was able to answer the question: how was the radius of convergence of a power series related to the function represented by the series? However, as [Peiffer \(1978, 11–12\)](#) has rightly remarked, there were a number of flaws in Cauchy’s statement of his theorem. He considered the (local) expansion of the function in the vicinity of the origin alone, and, as he had done in his (1825a), he did not require any condition on the function apart from its being “finite and continuous”.

In addition, in the typically ambiguous language that reveals his poor understanding at that time of the nature of complex differentiability, he stated his theorem for functions of both real and imaginary variables although while explaining it he referred to “imaginary” variables alone.³⁵

³⁴However, Cauchy avoided any geometric language.

³⁵In the real case one has to assume that the function is of class C^∞ and, moreover, the following condition is satisfied: $\frac{|f^{(n)}(x)|}{n!} k^n < K$, where K and k are positive real numbers.

3.4.4 Calcul des limites and Residues

On November 27, 1831 Cauchy presented a second *Mémoire* to the Turin Academy, which he had lithographed on December 17, 1831 and published again in Ferussac's *Bulletin* (Cauchy 1831c), where it was followed by a paper (Cauchy 1831d) which included some formulae extracted from the *Mémoire*. They were a summary of the longer *Mémoire* (Cauchy 1831e) he presented on the same day at the Academy and published in lithograph³⁶ on August 8, 1832.

The paper (Cauchy 1831e) is an interesting one in which new ideas and results are intertwined with old methods. He began by considering a (real or imaginary) function $f(z)$ of $z = x + iy$, where $x = x(r, p)$ and $y = y(r, p)$ are rectangular coordinates, and (r, p) are polar coordinates. After summarising the topic of his 1814 *Mémoire* (!) he wrote down the equation

$$\frac{\partial}{\partial p} \left[f(z) \frac{\partial z}{\partial r} \right] = \frac{\partial}{\partial r} \left[f(z) \frac{\partial z}{\partial p} \right] = v(p, r)$$

then considered a closed contour $OO'O'' \dots$ in the (x, y) -plane and the double integrals $\iint v dp dr$, $\iint v dr dp$ in the region bounded by it. In the case when $v(p, r)$ is not finite and continuous for all values of p and r , the difference $\Delta = \iint v dp dr - \iint v dr dp$ can be determined by using either the theory of singular integrals (as he had done in 1814) or the theory of residues. Supposing that $x = x(r, p)$ and $y = y(r, p)$ are continuous and finite within the domain, where $z_1, \dots, z_m$ are the roots of $\frac{1}{f(z)}$, and eventually that $\frac{\partial x}{\partial r} \frac{\partial y}{\partial p} - \frac{\partial x}{\partial p} \frac{\partial y}{\partial r} > 0$, then $\Delta = 2\pi i \mathcal{E}(f(z))$, where this denotes the sum of the residues of f within the domain.

Having stated the theorem, Cauchy turned to its applications. He first considered the case of a rectangular contour. By taking $x = r$ and $y = p$ he re-obtained Δ in terms of simple integrals as he had done in 1814. Then he turned to the more general case of a closed contour of the length c . In this case, denoting the arc by s one has $\Delta = \int_0^c f(z) \frac{dz}{ds} ds = 2\pi i \mathcal{E}(f(z))$. As $|\frac{dz}{ds}| = 1$ it follows at once that

$$|\mathcal{E}(f(z))| \leq \frac{1}{2\pi} \int_0^c |f(z)| ds \quad \text{i.e.} \quad |\mathcal{E}(f(z))| \leq \frac{c}{2\pi} \Lambda f(z),$$

which shows the sought-for link between the two calculi. After stating this as a theorem, Cauchy developed a new “calculus of indices”³⁷ that allowed him to obtain

³⁶An Italian translation of this paper was later published at Modena in *Memorie della Società Italiana di Scienze* 22 (1839), 91–183 followed (on pp. 228–246) by the translation of the paper *Calcul des indices des fonctions* that had first appeared as a lithograph on June 15, 1833 and later been expanded by Cauchy in a paper published in 1837 in the *Journal* of the École polytechnique, see Cauchy (1837e).

³⁷Cauchy later abandoned this “calculus” in favour of other methods. This perhaps explains why, contrary to his usual habit, Cauchy never re-published this paper.

some theorems on the roots of algebraic equations, and he devoted the remainder of the paper to its applications. More precisely, he began by considering the functions $f(z)$, $f'(z)$, and $F(z)$ which are “finite and continuous” inside (and on) the contour $OO'O'' \dots$, and supposed $z_1, z_2, \dots, z_n$ to be the roots (Cauchy made no remark about their possible multiplicity) of $f(z) = 0$ lying inside the contour. Then he stated that

$$F(z_1) + F(z_2) + \dots + F(z_n) = \mathcal{C} \left(\frac{f'(z)}{f(z)} F(z) \right),$$

which is also equal to $\frac{1}{2\pi i} \int_0^c \frac{f'(z)}{f(z)} F(z) \frac{dz}{ds} ds$.

By putting $F(z) = z$ in the above formulae Cauchy obtained (1831e, 193–194)

$$z_1 + z_2 + \dots + z_n = \frac{1}{2\pi i} \int_0^c \frac{f'(z)}{f(z)} z \frac{dz}{ds} ds$$

and also

$$m = \frac{1}{2\pi i} \int_0^c \frac{f'(z)}{f(z)} \frac{dz}{ds} ds,$$

which provides the number of zeros of the function $f(z)$ (counted according to multiplicity). Then he reformulated this in terms of “integral index”, the “index” of a function $\varphi(s)$ with respect to a root σ of the equation $1/\varphi(s) = 0$ being defined as

$$\frac{1}{2} \left(\frac{\varphi(\sigma + \varepsilon)}{\sqrt{(\varphi(\sigma + \varepsilon))^2}} - \frac{\varphi(\sigma - \varepsilon)}{\sqrt{(\varphi(\sigma - \varepsilon))^2}} \right)$$

(ε arbitrarily small) and the integral index being the sum of the indices with respect to all the roots inside the given contour. Thus, Cauchy had obtained a result that foreshadows what will later be called the “principle of the argument” (see Sect. 3.6.4).

As a further application, he considered $f(z)$ as the sum of $\Pi(z) + \varpi(z)$, where the functions $f(z)$, $\Pi(z)$, $\varpi(z)$ are finite and continuous inside and on the contour $OO'O'' \dots$, and $\Lambda \frac{\varpi(z)}{\Pi(z)} < 1$ on the contour. Then, by tacitly assuming the derivatives of the functions are finite and continuous, he proved that $f(z) = 0$ and $\Pi(z) = 0$ have the same number of roots inside the contour (1831e, 205–206). As Smithies (1997, 181) has remarked, “this result is usually known as Rouché’s theorem, although Rouché only gave it for the case where the contour is a circle” (see Sect. 3.6.4).

Cauchy taught in Turin from October 1832 to July 1833, when he was called to Prague to serve as a tutor to the son of the exiled king Charles X. In accordance with the title of his chair, he delivered introductory lectures of a philosophical-theological character (Cauchy 1833a). The essential content of his Turin lectures was published in the *Résumés analytiques* (Cauchy 1833b) which appeared in print when he was already living in Bohemia, and contain nothing new by comparison to his previous

treatises. A vivid account of Cauchy's teaching was provided by Luigi Federico Menabrea.³⁸ Not surprisingly, Cauchy's lectures

were very confused, skipping suddenly from one idea to another, from one formula to the next, with no attempt to give a connection between them. His presentations were obscure clouds, illuminated from time to time by flashes of pure genius (quoted in Belhoste 1991, 156).

so much so that Menabrea was the only one to “see it through” out of some 30 students who initially enrolled in the course.

3.4.5 *Letters and Papers from Bohemia*

In 1834 Cauchy was eventually joined by his wife and his family. When the king left Prague in 1836, Cauchy followed the court in its peregrinations first to Toeplitz, then to Kirchberg and eventually to Göritz. His duties as a tutor of the Duke of Bordeaux left him little time for original research. During these years he lived in scientific isolation, exchanging letters with his friends in Paris and abroad. In spite of this, he was able to submit to the Paris Académie some 18 notes on various subjects, including mathematical optics and the theory of light (see *O.C.* (1), 4, 5–99). In particular, in a letter he sent to his former *répétiteur* Coriolis on January 29, 1837 (Cauchy 1837a) he summarised some of his work, beginning by re-stating his integral representation theorem from (1831b) (see Sect. 3.4.3) for (real or imaginary) functions of a (real or imaginary) variable.

As for the continuity of the (single-valued) functions involved he referred to the definitions he gave in his *Cours d'analyse* thus tacitly assuming that they also hold for functions of a complex variable, pointing out that “a function that does not become infinite in general only ceases to be continuous by becoming multiple” (Cauchy 1837a, 39). Given this, a root of an (algebraic) equation depending on a parameter will be a continuous function of the parameter in any domain in which it is not branched, i.e. the equation will not have multiple roots. Calling the values of the parameter for which the given equation and its derivative have common roots the “principal values”, he stated a theorem on the expansion of the roots in (ascending or descending) power series that he proved in a subsequent letter on May 5, 1837 (Cauchy 1837b).³⁹ There he began by recalling his integral

³⁸A former student of Plana, he became later professor of mechanics at the Military Academy in Turin. After meeting Babbage at the Congress of Italian scientists held in Turin (1840) he published a detailed account of the latter's analytical engine in the *Bibliothèque Universelle de Genève* (1842) which was translated into English by Ada Lovelace. A member of the Parliament after 1848, he took part to the Risorgimento wars as a General and, after the unification of Italy, he played a major political role, first as a minister (1861–1864) then as a Prime Minister (1867–1869) of the Kingdom of Italy.

³⁹The letter may have been sent to Libri; no recipient is stated in the *Oeuvres*.

representation theorem, and as a corollary of it he re-stated the theorem he had announced in his letter to Coriolis as follows: Every root of an equation $F(x, t) = 0$ can be generally expanded in convergent power series of the parameter t for as long as $|t|$ is less than the moduli of all its principal values. As a consequence, each of the distinct roots of $F(x, t) = 0$ can be separately expanded in convergent power series. The roots of $F(x, t) = 0$ can be collected in groups in such a way that at the origin ($t = 0$) the number of the groups equals the number of distinct roots, and as the value of $|t|$ increases two groups amalgamate when two roots belonging to each group become equal for a value of $|t|$ corresponding to a suitable principal value of t . The number of groups of roots is completely determined for any particular value of $|t|$, and $F(x, t) = 0$ can be split into equations such that each of them will separately give the roots belonging to one single group. This was formally stated by Cauchy as his Theorem III. Thus, as Brill and Noether (1894, 187) have rightly remarked, this paper by Cauchy “opened the way to Puiseux’s theory in the most effective manner”. This became more evident in the remaining part of the letter, where Cauchy suddenly resorted to geometric language. After remarking that Theorem III could be obtained from formula (29) of his (1831d, 182) with suitable substitutions, he considered $F(x, t) = 0$ as written in the form $\Pi(z) + t\varpi(z) = 0$ where Π , ϖ are polynomials and $z = x + iy$. By interpreting x, y as rectangular coordinates, he considered the system of plane curves as described by

$$T = |t| = \left| \frac{\Pi(x + iy)}{\varpi(x + iy)} \right|.$$

Actually, as z varies T describes a surface over the x, y -plane, and not a system of curves. In order to understand Cauchy’s statement properly, following the suggestion in Brill and Noether’s (1894, 187), one has to think of the horizontal sections of the surface T corresponding to constant values of T . By projecting them onto the plane one gets the system of curves that Cauchy was talking about.

For $T = 0$ the curves reduce to as many distinct points as $\Pi(x + iy) = 0$ has distinct roots. With increasing values of T these points expand in independent, isolated curves each of them enclosing one of those points, until two or several curves unite in a multiple point corresponding to a principal value of t (i.e. to a value of z corresponding to a cross-point or a branch-point), eventually forming one single curve. For a given value of T Cauchy distinguished two species of curves: curves of the first kind that expand and curves of the second kind that shrink with increasing values of T . As T approaches 0, only the curves of the first kind around the points representing the roots of $\Pi(x + iy) = 0$ will subsist (under the hypothesis that $\deg \Pi(z) > \deg \varpi(z)$), while for $T \rightarrow \infty$ curves of the second kind around the points representing the roots of $\varpi(x + iy) = 0$ will subsist, together with a single very large curve of the first kind that encloses the origin. For any given value of T the number of curves of the first kind is exactly the number of groups of roots mentioned in Theorem III. The formulae given in his (1831b) and in (1831d) allowed him to expand in convergent series the sum of (similar powers of) the roots included in the same curve (Cauchy 1837b, 54). Thus, as Brill and Noether (1894, 188) have

summarised the matter, the system of curves of the first kind corresponding to a given value of T gives rise to a grouping of the roots which can be expanded in ascending powers series of t , either separately or in symmetric grouping with others belonging to the same curve. In the case when $\deg \Pi(z) < \deg \varpi(z)$, one has curves of both kinds, and the infinitely great roots corresponding to $T = 0$ can still be expanded in ascending power series of t which include a finite number of powers of $1/t$. In addition, [Cauchy \(1837b, 55\)](#) observed that if $\Pi(z) = 0$ (resp. $\varpi(z) = 0$) have equal roots, each of the corresponding roots of $\Pi(z) + t\varpi(z) = 0$ can be separately expanded in convergent power series of t (resp. $1/t$) provided T is less (resp. greater) than the moduli of all the principal values of t .

Eventually, he remarked that two or more branches corresponding to a given value $T = \text{const.}$ cross each other at equal angles. In particular, when $t = \exp(S + iP)$ he proved that the curves $S = \text{const.}$ and $P = \text{const.}$ cross each other at right angles.

It is worth remarking that Cauchy limited himself to considering expansions in power series of t (or $1/t$)—in other words to MacLaurin instead of Taylor series. As [Brill and Noether \(1894, 188\)](#) have pointed out, however important this contribution by Cauchy was, by limiting himself to the behaviour of functions in the neighbourhood of the origin he prevented himself from having a “deeper insight” in the theory.

One week later a second letter by [Cauchy \(1837c\)](#) on this subject was published in the *Comptes rendus* of the French Académie. There he considered the polynomial equation $f(x) = \sum_0^n a_j x^{n-j} = 0$ of degree n with (real or complex) coefficients a_j , and $a_0 = 1$. In order to expand its roots in convergent series, he considered the equation $f(x) = k$, where k a (real or complex) constant such that $|k|$ is greater than the moduli of all the principal values of $f(x)$. Under this assumption, the roots of the equation $f(x) = k$ can be expanded in convergent power series in x/k . By putting $x = 1/z$ he obtained

$$\sum_0^n a_j z^{n-j} = [\varpi(z)]^n, \quad \varpi(0) = 1$$

and also $z^n = \frac{1}{k} [\varpi(z)]^n$ whose solution is $z = \lambda \varpi(z)$ provided that λ is a root of $\lambda^n = 1/k$. For $|\lambda|$ small enough (i.e. for $|k|$ great enough) z can be expanded in a Lagrange series

$$z = \lambda \varpi(0) + \left[\frac{\lambda^2}{1.2} \frac{d[\varpi(\varepsilon)]^2}{d\varepsilon} + \frac{\lambda^3}{1.2.3} \frac{d^2[\varpi(\varepsilon)]^3}{d\varepsilon^2} + \dots \right]_{\varepsilon=0}.$$

provided that $|k|$ is greater than the principal modulus of $f(x)$ which could be determined by looking at the roots of $f'(x) = 0$ ([Cauchy 1837c, 63](#)). In the remaining part of the letter Cauchy applied this result to the case in which $f(x)$ is real and has real, distinct roots as well as to the case when it has both real and complex roots. His concluding theorem stated that if $f(x)$ is a polynomial with real coefficients, and the equation $f(x) = 0$ has no repeated roots, then by means of convergent series an auxiliary equation can be determined whose roots are precisely the real roots of $f(x) = 0$ taken with positive or negative signs according as they correspond to

positive or negative values of $f'(x) = 0$. Cauchy promised to resume the matter in a subsequent Note and to clarify his method with numerical examples. But, as Brill and Noether (1894, 185) have remarked—and as we will see in the next section—“only Laurent’s theorem brought all these efforts into focus”.

3.4.6 *The Prague Paper on Differential Equations*

In his letter to Coriolis, Cauchy had also mentioned in passing his recent work on the integration of differential equations, to which we now turn. At the end of (Cauchy 1831a) he had stated that his “new calculus” could be applied to functions defined by (ordinary and partial) differential equation, but he did not develop this idea any further there. This was instead the subject of the paper (Cauchy 1835) which he published at Prague in lithographic form and re-published in the third volume of his new *Exercices* in 1842. In the introduction to it, after mentioning once more his 1822 counter-example $y = \exp(-1/x^2)$ Cauchy (1835, 400) stated that the integration of differential equations by series was “illusory, so long as one did not provide any means of assuring that the series so obtained were convergent”. The methods he was going to explain shared, he said, the advantages of the method he had presented in his (unpublished) second-year lectures at the École Polytechnique (Cauchy 1981), where he had established an existence theorem for the (local) solution of the “Cauchy problem” for a general, differential equation of the first order in the real case. In his (1835) he established a second existence theorem for the (local) solution of a system of first order ordinary differential equations satisfying given initial conditions. He acknowledged (1835, 404) he did this following a route that was suggested to him by the reading of Hamilton’s “wonderful paper” (1834) on the differential equations of dynamics. Accordingly, he showed how one could associate the given system to a single first order linear partial differential equation (which he called the characteristic equation), then he found the general integrals of the system by setting suitable particular integrals of the characteristic equation equal to arbitrary constants. The final step, which he performed by resorting to his “calculus of limits”, was to show that these particular integrals could be expanded in convergent series.

Cauchy illustrated his method for the case of a single differential equation $dx = \mathcal{F}(x, t)dt$ where $x = x(t)$ and $x(\tau) = \xi$. There Cauchy (1835, 432) recalled the Italian translation (Cauchy 1834) of his Turin *Mémoire* of 1831, where he had considered a function $f(x)$ continuous inside a circle with radius r centred at x so that⁴⁰

⁴⁰This mean value equality had also been found by Poisson (1823b, 498) who, however, did not recognise its full scope. As Stäckel remarked, it was “covered by limitless sand dune-like magic formulae” (quoted in Remmert 1991, 205).

$$f(x) = \frac{1}{2\pi} \int_{\pi}^{\pi} f(x + re^{ip}) dp.$$

By applying the “Cauchy inequalities” for $|f^{(n)}| \leq n!r^{(-n)}\Lambda f(x+\tilde{x})$, where $\tilde{x} = re^{ip}$ and $\Lambda f(x+\tilde{x})$ denoted the “limit” of $|f(x+\tilde{x})|$ on the circumference, he was able to conclude that $x(\tau)$, for some value $t = \tau$, could be expanded in convergent power series of $(\tau - t)$, and therefore there existed a (local) solution of the given differential equation, by showing that the modulus of the general term of the expansion is less than $(|\tau - t|k)^n$, i.e. less than the general term of a convergent geometric series provided that $|\tau - t| < \frac{1}{k}$ (1835, 446).

Curiously enough, Cauchy went on to illustrate his results with the example $dx = (x + t)^k dt$ where all the variables involved are real. Apparently, even as late as this Cauchy did not seem to recognise the deep difference between the real and the complex case of his existence theorems, and the ambiguity between the real and complex ran through his entire paper, so much so that in his concluding remark he proudly stated that his new theorems could “easily” be extended to the solution of differential equations in which the variables and functions involved become imaginary. Precisely for this reason he preferred his second existence theorem based on the calculus of limits, which had transformed the integration of differential equations into a rigorous theory. After his return to Paris he took up this topic on various occasions. Thus, for instance, in a number of notes (see *O.O.* (1) 6, 461–470; 7, 5–83) that he submitted weekly to the Académie between June and July 1842 he reformulated his second existence proof for a system of ordinary differential equations and then extended it to first-order partial differential equations of the form

$$\frac{\partial u_i}{\partial t} = F_i \left(t, x_1, x_2, \dots, x_n; u_1, \dots, u_m, \frac{\partial u_1}{\partial x_1}, \dots, \frac{\partial u_m}{\partial x_n} \right)$$

with the initial conditions

$$u_i(0, x_1, x_2, \dots, x_n) = w_i(x_1, x_2, \dots, x_n) \quad (i = 1, \dots, n).$$

Stated in modern terms, what Cauchy did was prove that under the conditions that the functions F_i are linear with respect to the $\frac{\partial u_k}{\partial x_h}$ and analytic in the neighbourhood of a point (the origin, say) where the functions w_i are also analytic, then there exists a unique solution of the Cauchy problem expandable in a locally convergent power series.⁴¹ In Cauchy’s hands, the theory of differential equations in the complex domain became a part, important in its own right, of the general theory of imaginary functions, to which he devoted a large amount of his research work in the next decades. Working in isolation, during this period Cauchy had been

⁴¹As we will see in Sect. 7.10.4.1, apparently unaware of Cauchy’s work, Sonya Kovalevskaya rediscovered and generalised this theorem in her thesis (Kovalevskaya 1875).

able to take essential steps in the exploration of the complex domain, even though the main results he had obtained were almost unknown to the Paris mathematical community.

3.5 Theorems and Results in Complex Analysis

When the Duke of Bordeaux reached 18 years of age his formal education came to an end and Cauchy's duties were over. When he returned to Paris in October 1838 he was close to 50. Things in Paris had changed considerably since 1830. Cauchy's colleagues Fourier, Ampère, Legendre and Navier had died. After his return from exile they were followed by Poisson in 1840, and Lacroix and Coriolis in 1843. Mainly for political reasons, Cauchy was rather isolated in the Paris mathematical community. There is no doubt that his absence from Paris diminished the impact of his work. Most of the work he had done abroad was unknown in Paris, including his "theorems on the convergence of series, perhaps his most important achievement" in the opinion of Brill and Noether (1894, 179). Evidence of this was provided, for instance, by Liouville and Sturm. When they pointed out an "inexactness" contained in a letter that Cauchy sent to the Académie in 1837, where he referred to his (1833d) on the indices of functions, they lamented that "the memoir of 1833 has not reached us; it does not even seem that any of principal geometers of Paris has received that memoir" (in *CR* 4, 720–724). This was perhaps the reason why Cauchy published an expanded version of (1833d) as his (1837e) that very year in the *Journal* of the École Polytechnique, which doubled the size the original paper. There he responded to Liouville's and Sturm's criticism by showing that their theorem could be derived from one of his formulae (1837e, 457).

As Belhoste (1991, 189) has remarked, "an entire generation of French youths were deprived of instruction by Cauchy" as a consequence of his exile. Cauchy still preserved his seat at the Académie, but he had lost all his teaching positions when he left, and he was unable to obtain a position at the *Bureau de Longitudes* because of his refusal to swear an oath to the new regime. Apparently, he used to speak of himself as an "old professor to whom youth has listened to for such a long time and with such a goodwill and who still works for the young in the quiet of his study, still seeking to be of use" (quoted in Belhoste 1991, 198). His feeling of isolation increased in 1843, when he unsuccessfully applied for a chair at the Collège de France, only to find himself (and Liouville) passed over in favour of the second-rate mathematician Guglielmo Libri.⁴² It is worth remembering here that Libri had been one of Cauchy's preferred correspondents during his stay in Prague, and someone

⁴²Cauchy was active for numerous Catholic causes at a time when the Jesuits were believed by many to be interfering with academic freedom, which is the most likely reason why he was passed over in elections at the Collège de France in favour of Libri—a scandal in itself. See Belhoste (1991, 182–189).

to whom he addressed a number of letters on various topics to be published in the *Comptes rendus* of the Académie.⁴³

So, to make known the results that he had obtained in Turin and Prague, as well as his more recent results, Cauchy resumed his *Exercises* in 1839 under the new title *Exercices d'analyse et de physique mathématique*. He also used the *Comptes rendus des séances* of the Paris Académie, recently established in 1836, to publish a note or a memoir almost weekly. Along with various subjects of mathematical physics, he submitted a number of papers dealing with topics in complex analysis to which we now turn.

3.5.1 Calcul des limites and Expansions in Series

After a series of papers dealing with optics, wave theory, and the theory of light, Cauchy turned to analysis beginning with a great *Mémoire* on the integration of linear (ordinary and partial) differential equations with constant coefficients. The *Mémoire* was distributed in a sequence of notes submitted weekly to the Académie between May 27 and June 17, 1839. By means of the theory of residues he developed the theory that he had presented in his second-year lectures at the École Polytechnique in the 1820s and summarised the main results of his Prague memoir (1835).⁴⁴ Some months later, on September 8, he re-published them in the *Exercises* prefaced with a short introduction.⁴⁵

In the meantime, on August 5, 1839, Cauchy had presented a paper to the Académie, whose introduction largely coincided with the first part of his (1831b). However, he showed that he had changed his mind at an important point concerning the hypotheses required by his integral representation theorem. Now he stated it as follows: the function $f(x)$ can be expanded as a convergent power series of x “provided $|x|$ remains less than the smallest value for which the function $f(x)$ or its derivative ceases to be finite and continuous. [our emphasis]” (Cauchy 1839a, 486).⁴⁶ Later in the paper he also explicitly stated that the (Cauchy–Riemann)-equations imply $\int_{-\pi}^{\pi} f(z) dp = 2\pi f(0)$ (1839a, 490).

The path Cauchy followed in proving this was essentially the same as in his Turin *Mémoire*, § 2. He began by giving the variable x the imaginary value $z = Ze^{ip}$ and, as usual, he wrote down the identity

⁴³See, e.g., *O.C.* (1), 4, 11–21; 30–38.

⁴⁴See *O.C.* (1) 4, 369–426. As for the Prague (1835) memoir, it was re-published as such in his new *Exercises* on December 1840. See *O.C.* (2) 11, 399–465.

⁴⁵See *O.C.* (2) 11, 75–133. He added a detailed study of the case in which the characteristic equation is homogeneous, see *O.C.* (2) 11, 227–264.

⁴⁶According to Kline (1972, 639), Cauchy changed his mind after a correspondence he exchanged with Liouville and Sturm, but we have been unable to find any evidence for this.

$$\frac{\partial f(z)}{\partial Z} = \frac{1}{iZ} \frac{\partial f(z)}{\partial p}.$$

He now assumed that both $f(z)$ and $f'(z)$ are finite and continuous for $|z| \leq Z_0$ for some real number Z_0 and any p , and by a double integration with respect to Z and p he obtained $\int_{-\pi}^{\pi} f(z) dp = 2\pi f(0)$. Then, under the further hypothesis that $f(0) = 0$, he deduced that $\int_{-\pi}^{\pi} f(z) dp = 0$. The integral representation theorem was now easily proved by first substituting for $f(z)$ the expression

$$z \frac{f(z) - f(x)}{z - x} \quad (x \neq z, |x| < Z_0),$$

then by developing $\frac{z}{z-x}$ in series and finally by integrating the result term by term. Cauchy invoked the continuity of $f'(x)$ to ensure that the result of the double integration was independent of the order of integration with respect to Z and p .

As he had done in his Turin *Mémoire*, he provided examples of functions such as $e^{1/z}$, e^{1/z^2} , and $\cos(1/z)$, which were discontinuous at $z = 0$ together with their derivatives and consequently could not be expanded in power series about the origin. He remarked that in these cases both the given function and its derivative become discontinuous at the same point, and in the concluding lines of his (1839a, 490) Cauchy commented that one could avoid mentioning the derivative if this were always the case. However, for lack of sufficient evidence, it was “more rigorous” to add the condition on the derivative to the statement of the theorem.

As part of his drive to acquaint his Parisian colleagues with what they might have missed, Cauchy then published an essentially identical version of this paper on September 9, 1839 in the first issue of his new *Exercices* (Cauchy 1840a), where he also managed to mention not only this paper but also his Turin *Mémoire* and its Italian translation (1834). Not content with this he dropped a mention of his old (1829b) on Lagrange series into the short announcement (Cauchy 1839b) that he made to the Académie on November 11, 1839 of a new (apparently unpublished) memoir on the application of the integral representation theorem to the expansion of implicit functions. Some months later, on April 20, 1840, he resumed the matter in a paper (Cauchy 1840c) submitted to the Académie. Essentially the same paper was re-published on August 17, 1840 as an issue of the *Exercices* (Cauchy 1840d).

Here once more Cauchy began by recalling his “memoir on celestial mechanics”—as he called his Turin *Mémoire*—and the (new formulation of) the integral representation theorem stated there. He did not even hesitate to claim that this theorem “immediately gives the rules for the convergence of series provided by the expansion of explicit functions, and *reduces the law of convergence simply to the law of continuity* [our emphasis]” (Cauchy 1840c, 180 or Cauchy 1840d, 331). He made it clear that the “law of continuity” he was referring to was of course given by the relevant definition that he had stated in his *Cours d’analyse*. He repeated this claim later on in the paper when dealing with Lagrange series for the expansions of implicit functions: “so the law of convergence again reduces to the law of continuity” (Cauchy 1840c, 192 or Cauchy 1840d, 343). According to Brill

and Noether (1894, 181), Cauchy meant by this that the expansion of a function implicitly defined by the equation $f(x, t) = 0$ as a power series in t represented a root of the equation as long as $f(x, t)$ was a single-valued, continuous function in the domain under consideration.

Following a remark by Moigno, in his (1840c and 1840d) Cauchy aimed at avoiding integration in the proof of the integral representation theorem by providing instead an “elementary” proof.⁴⁷ To this end he replaced the integral $\frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{zf(z)}{z-x} dp$, which he called the isotropic mean (*moyenne isotropique*) of $\frac{zf(z)}{z-x}$, by “the mean value of $f(re^{ip})$ for a modulus r ”. This was the limit for increasing n of the arithmetic mean of the n values assumed by the function $f(re^{ip})$ on a circle with centre O and radius r , corresponding to the n primitive roots of unity, so $\Pi(r) = \frac{1}{n} \{f(r) + f(\theta r) + \dots + f(\theta^{n-1}r)\}$, where $\theta = e^{2i\pi/n}$. More precisely, he considered a function $\varpi(x)$ of an imaginary variable $x = re^{ip}$ that is “continuous and finite” together with its derivative $\varpi'(x)$ within the annulus $r_0 \leq r \leq R$. When $\theta = e^{2i\pi/n}$ is a primitive root of unity, for any such r the arithmetic mean of the products $\theta^m \varpi'(\theta^m r)$ ($m = 0, 1, 2, \dots, n-1$)

$$\frac{1}{n} (\varpi'(r) + \theta \varpi'(\theta r) + \theta^2 \varpi'(\theta^2 r) + \dots + \theta^{n-1} \varpi'(\theta^{n-1} r))$$

will approach 0 as n increases, as Cauchy proved easily using the techniques he had introduced in his *Résumé* (1823a). Under the same hypotheses on $\varpi(x)$ he also proved that

$$\Pi(r) = \frac{1}{n} \{\varpi(r) + \theta \varpi(\theta r) + \theta^2 \varpi(\theta^2 r) + \dots + \theta^{n-1} \varpi(\theta^{n-1} r)\}$$

will remain “sensibly invariant” for “very large values” of n and any r such that $r_0 \leq r \leq R$. The limit $\lim_{n \rightarrow \infty} \Pi(r)$ was called by Cauchy the “mean value” of $\varpi(x)$ for $|x| = r$. Accordingly, he reformulated his result as follows: if $\varpi(x)$ is “continuous and finite” together with $\varpi'(x)$ within the annulus $r_0 \leq r \leq R$, then the mean value of $\varpi(x)$ corresponding to a modulus r such that $r_0 \leq r \leq R$ is independent of r (Cauchy 1840c, 186–187 and 1840d, 337). In particular, it followed from this that $\Pi(R) = \Pi(r_0)$, and, under the additional hypotheses that both $\varpi(x)$ and $\varpi'(x)$ are continuous and finite for $0 \leq r \leq R$, it is also the case that $\Pi(r) = \Pi(0)$.

Given this, the substitution $\varpi(z) = z \frac{f(z) - f(x)}{z-x}$, where $f(z)$ and $f'(z)$ are continuous and finite functions of z for $0 \leq |z| \leq R$, allowed him to re-state his integral representation theorem as follows: If $|x|$ is “less than the smallest of those moduli for which one of the functions $f(x)$, $f'(x)$ ceases to be finite and continuous”, then $f(x)$ can be represented by the mean value of $\frac{z}{z-x} f(z)$ corresponding to a modulus

⁴⁷The integral representation theorem and its “elementary” proof were included by Moigno in his *Leçons* inspired by Cauchy’s work, see Moigno (1840–1844, 1, 152–157).

r of z such that $|x| < |z|$ and, consequently, it can be expanded as convergent power series in x . It was now a simple calculation for Cauchy to verify that this power series coincides with the MacLaurin expansion of $f(x)$ and to evaluate the remainder of the series by following the same procedure as in his (1831b).

In the second part of the paper (1840c, 192–198 and 1840d, 343–349) Cauchy applied this to the expansion of an implicit function in a Lagrange series, where he re-obtained some of the relevant results he had obtained in (1831b), including the expansion of a root of Kepler’s equation in a convergent power series valid for $|x| < 0.662742\dots$. He then limited himself to re-stating his integral representation theorem, and to recalling the “elementary proof” he had given of it in short note (Cauchy 1840e) where he alluded to his (still unpublished) (1835) Prague lithographed memoir. He summarised the matter in detail some months later in a sequence of three notes (Cauchy 1840f,g,h) submitted weekly to the Académie between October 26 and November 9, 1840. Although there is nothing new in them, (Cauchy 1840g) is worth mentioning because he there referred in passing to his (1825a) for the first time since its publication (Cauchy 1840g, 390).

Of greater interest is the report that Cauchy presented to the Académie on May 10, 1841 (Cauchy 1841a). This was the review of a memoir submitted to the French Académie by the young Norwegian mathematician O.J. Broch.⁴⁸ Inspired by Abel (1828c), Broch had recently published a paper in Crelle’s *Journal* where he dealt with functions of the form $\int \frac{F(x)dx}{\sqrt[n]{R(x)}}$ for any positive whole number n and not only the case $n = 2$ considered by Abel (Broch 1840). In the memoir presented to the Académie he extended this study to functions $F(x^p)$, p being a positive integer. This gave Cauchy the occasion to refer for the first time in his writings to elliptic functions, and to Abel’s still unpublished 1826 memoir. Cauchy did not mention that he had had Abel’s memoir in his hands and done nothing with it, but simply mentioned the positive report by Legendre and the posthumous Prize delivered to Abel by the Académie in 1830. After observing that Broch’s results were included in Abel’s memoir, but nonetheless worth publishing,⁴⁹ he referred to Holmböe’s Preface to Abel’s works (1839) to vindicate “the flattering encouragements, the witness of esteem and admiration” (!) given by scholars to Abel when he was alive.

One week later he took up the matter again and showed that “the lovely theorems” of Abel on elliptic functions, and Abel’s theorem as well, were particular cases of the general theorems on residues he had obtained (Cauchy 1841b). In this connection he recalled his own old paper (1827c), and he was also pleased to mention the responses to his theory of residues provided by Ostrogradskii and Buniakowskii (see Sect. 3.2) and, more recently, by Tortolini (1834–1835; see also

⁴⁸Not good enough as a mathematician to be a “new Abel”, as Holmböe too precipitously called him, Broch made a career as a mathematics professor at the University of Christiania.

⁴⁹The paper was published in Crelle’s *Journal* as Broch (1842).

his later (1842)) and [Richelot \(1840\)](#).⁵⁰ By applying his residue theory Cauchy obtained an expression for the sum, s , of integrals of the form

$$s = \int_{\xi_1}^{x_1} f(x, t) dx + \int_{\xi_2}^{x_2} f(x, t) dx + \cdots$$

We shall not enter into the details of this paper or the one he presented to the Académie the very next week ([Cauchy 1841c](#)) where he repeated his claim that Euler's and Abel's results on elliptic and Abelian integrals were particular cases of his formulae and re-obtained the same expression for s by following a shorter, more direct method that also used his residue theorem. He eventually obtained (1841c, 164–165)

$$\sum \int_{\xi}^x f(x) dx = \mathcal{E}(f(x)) \int_{\tau}^t \frac{\frac{\partial F(x, t)}{\partial t}}{F(x, t)} dt - \int_{\tau}^t \mathcal{E} \left(f(x) \frac{\frac{\partial F(x, t)}{\partial t}}{F(x, t)} \right) dt, \quad (3.6)$$

which corresponds to the formula he had published one week earlier in [Cauchy \(1841b, 155\)](#). From this Euler's addition theorem for elliptic integrals, Broch's formulae and Abel's theorem could be obtained by means of suitable substitutions for $f(x)$ and $F(x, t)$. These papers by Cauchy do not seem to have contributed the development of the subject in any substantial way, perhaps because he limited himself to re-obtaining some already known results in the context of residue theory. They show, however, that he was well aware of Euler's and Abel's work, although he was still unable to include it in his ideas about complex integration. As we will see in Chap. 4, two years later Cauchy was to take up the subject of elliptic functions in more detail.

On August 3, 1841 he reported on a paper on residue theory that Oltramare⁵¹ had submitted to the Académie on May 24, and which Cauchy had been charged to review jointly with Sturm (see *O.O.* 1 (6), 277–280). There Oltramare admitted that he had been “invited” by Cauchy to study the theory of residues and stated the theorem that if $w = w(z)$ is a single-valued function defined by an algebraic equation

⁵⁰Interestingly enough, Cauchy omitted to mention the paper by his friend [Piola \(1834\)](#). Apparently Cauchy was also unaware of [Gregory \(1837\)](#) which seems to have provided the first exposition of the residue theory in England. The first account of the calculus of residues in America was to be given by Benjamin Peirce in the second volume of his (1846). Curiously, Peirce avoided mentioning Cauchy by name, but used Cauchy's peculiar $\mathcal{E}$ notation. The first mention of Cauchy by name was given in [Kummell \(1879\)](#). For a detailed historical survey of the development of the residue theory, see Burkhardt's *Excurs betr. die Entwicklungsgeschichte von Cauchy's Residuentheorie* in [Burkhardt \(1914–1915, 1001–1032\)](#).

⁵¹Gabriel Oltramare was born in Geneva and studied in Paris. After completing his studies he returned to Geneva where he spent all his academic career teaching at the University there. At the start of his review of Oltramare's (1899) in the *Bulletin of the AMS* E. O. Lovett hailed him “the venerable dean of the faculty of sciences of Geneva, who is probably the oldest living pupil of Cauchy” ([Lovett 1899, 109](#)).

which assumes a finite value for infinite values of z , then the integral residue of $w(z)$ equals the integral residue of its inverse function $z = z(w)$. Cauchy recommended the paper for publication in the *Recueil des savants étrangers* but, as usual, it was not printed. Instead, it eventually appeared as (Oltramare 1855).

After the publication of the first part of his Turin *Mémoire* in the *Exercices* in October 1841 as Cauchy (1841d,e), Cauchy presented a short note to the Académie (Cauchy 1841f) on November 28 where he raised the important question of the uniqueness of the expansion of a function in power series. This question was easy to answer in the case of positive whole powers, Cauchy remarked, and in fact it had been answered by the uniqueness theorem he had stated in his *Cours d'analyse* (Cauchy 1821a, 144–145). But the proof he had given there ceases to hold when the series includes both negative and positive whole powers of the variable. Then he went on to state and prove that

1. If the sum of a convergent series of positive whole powers of the variable is 0, then all of its terms are equal to 0.
2. If the sum of a convergent series of positive and negative whole powers of the variable is 0 for a given modulus of the variable, then all of its terms are equal to 0.

From (1) and (2) the following uniqueness theorems could be easily derived⁵²:

3. A continuous function can be expanded in a convergent series of positive, whole powers in a unique manner.
4. A continuous function can be expanded a unique manner in a convergent series of positive, null and negative whole powers of the variable for a given value of its modulus and any value of its argument.

The proofs of theorems (1) and (3) followed the same route as in (1821a). As for the proof of theorem (2) Cauchy observed that it could be easily obtained by multiplying both sides of the equation

$$a_0 + a_1x + a_2x^2 + \cdots + a_{-1}x^{-1} + a_{-2}x^{-2} + \cdots = 0$$

by $e^{nip}dp$ and integrating with respect to p from 0 to 2π , thus obtaining a_n for any positive, null or negative n . From this the proof of theorem (4) followed immediately.

Contrary to what he had promised, he did not take up the subject again in the *Exercices*, perhaps because his attention turned to other matters, including the theory of light and the integration of differential equations in particular (see, e.g., Cauchy 1842a,b,c).⁵³ He returned to it in a paper he presented to the Académie on July 31,

⁵²Recall our earlier remark in the Introduction that Cauchy often used “continuous” to mean something more like “complex analytic”.

⁵³For Cauchy’s 1842 contributions to the theory of differential equations, see Cooke (1984, 24–27).

1843 (Cauchy 1843a) in which he provided an “elementary” proof of his uniqueness theorem (4) (one, that is, that did not resort to integration).

3.5.2 *Laurent’s Theorem*

Cauchy’s paper (1843a) led Pierre Laurent to submit to the Académie a “fragment” of a more extended work.⁵⁴ In the letter of presentation to the Académie Laurent (1843) pointed out that his results could be applied to the separation of the roots of an algebraic equation without resorting to Sturm’s theorem, and this mention was enough to allow Cauchy to see easily how it could be done.

Indeed, at the next meeting of the Académie Cauchy (1843b) stood up to claim priority with respect to Laurent’s statement by pointing in particular to his papers (1837b, c). Then, on October 30, he presented to the Académie his (and Liouville’s) report on Laurent’s memoir, Cauchy (1843g).⁵⁵ Cauchy began by observing that Laurent had provided an extension of his own integral representation theorem from (1831b) as it had been restated in (1840c,d). By analysing Cauchy’s method, Laurent had apparently recognised that one could deduce the following more general statement: If x is a real or imaginary variable, a real or imaginary function $f(x)$ can be represented by the sum of two convergent series, one in ascending and the other in descending whole powers of x whenever $|x|$ is included between two limits within which the function $f(x)$ and its derivative are continuous and finite. Cauchy went on to claim that the equation from which Laurent had deduced his theorem was included as a particular case in some formulae he himself had already obtained and published in (1826d). The theorem itself could be deduced from the theorem he had stated in Cauchy (1840c, 186–187) or Cauchy (1840d, 337). As for Laurent’s statement about the separation of the roots of an algebraic equation, this was “in

⁵⁴A former pupil of the École Polytechnique, which he entered in 1830, Pierre A. Laurent graduated two years later as one of the best students of his class. After a two years’ training at the *École d’Application* at Metz he was sent to Algeria as a lieutenant in the army. He returned to France around 1840 and spent six years working as a military engineer on the enlargement of the port of Le Havre. In this period he became interested in mathematics and submitted a paper to the Académie that Cauchy and Liouville were charged to review. They gave a positive report and recommended the paper for publication, but the recommendation was not followed by the Académie. Later on, the same fate befell a paper on the calculus of variations that Laurent had submitted for the Grand Prize in mathematics for 1842. The paper had arrived late and had therefore not been considered for the Prize. Once more Laurent’s paper was positively reviewed by Cauchy and Liouville and unsuccessfully recommended for publication. Disappointed, Laurent turned to applied mathematics and published papers on the theory of light and other subjects. He kept working as a military engineer and doing research in applied mathematics until his untimely death in 1854. See his obituary in Bertrand (1890a).

⁵⁵Laurent’s original memoir was never published. Its main content was included in the (posthumous) article (Laurent 1863).

agreement” with the conclusions he had stated in [Cauchy \(1823b\)](#) and even earlier, in his old 1819 *Mémoire* ([Cauchy 1824](#)).

In order to support his claims Cauchy added a note of his own ([Cauchy 1843h](#)) to the report to show that “the easiest way” to obtain Laurent’s theorem was by reformulating the results in Cauchy’s ([1840c,d](#)). On supposing that a function $\varpi(x)$ is continuous and finite together with $\varpi'(x)$ for $r_0 \leq r \leq R$, the mean value of $\varpi(x)$ is given by

$$\Pi(r) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \varpi(x) dp.$$

As he had done in ([1840c,d](#)) he observed that $\Pi(R) = \Pi(r_0)$, and $\Pi(r) = 0$ when $r_0 = 0$. Then he made the substitutions

$$y = r_0 e^{ip}, \quad z = R e^{ip}, \quad \varpi(z) = z \frac{f(z) - f(x)}{z - x},$$

where $f(x)$ satisfies the same conditions of continuity and finiteness as $\varpi(x)$ does. After remarking that

$$\Pi(r_0) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{y f(y)}{y - x} dp \quad \text{and} \quad \Pi(R) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{z f(z)}{z - x} dp - f(x),$$

from the equation $\Pi(R) = 0$ he obtained $f(x) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{z f(z)}{z - x} dp$, while the condition $\Pi(R) = \Pi(r_0)$ gives

$$f(x) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{z f(z)}{z - x} dp - \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{y f(y)}{y - x} dp.$$

At this point it was a matter of straightforward transformations for Cauchy to reduce this to the equation

$$f(x) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{z f(z)}{z - x} dp + \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{x f(y)}{x - y} dp - \frac{1}{2\pi(R - r_0)} \int_{r_0}^R \int_{-\pi}^{\pi} f(re^{ip}) dr dp,$$

“from which M. Laurent has deduced his theorem” ([Cauchy 1843h](#), 119). This was followed by a short note ([Cauchy 1843i](#)) presented to the Académie on November 27, where he observed that the theorem on the convergence of series $\sum_{n=0}^{\infty} u_n$ that he had proved in the *Cours d’analyse* (1821a) could be extended to series $\sum_{n=-\infty}^{\infty} u_n$. Calling $\rho = \limsup_{n \rightarrow \infty} (|u_n|)^{1/n}$ and $\rho' = \limsup_{n \rightarrow \infty} (|u_{-n}|)^{1/n}$ the

“moduli” of the series $\sum_{n=-\infty}^{n=\infty} u_n$, he stated that the series will be convergent if both ρ and ρ' are less than 1 and divergent otherwise.

3.5.3 The Continuity of Functions

In the meantime Cauchy had published a great memoir (Cauchy 1843n) on the applications of singular integrals in the *Exercises* on November 13. The introductory remarks to that memoir and some of its “general formulae” reproduced the content of a paper with the very same title (Cauchy 1843m) that he had presented to the Académie earlier that year, on February 20, as a reaction to a paper on the very same subject that Binet (1843) had presented to the Académie the previous week. Curiously enough, there Cauchy fell back on the notation of his old (1814) which he combined with the theory of residues to state some “general formulae” which he applied to the study of Γ -function and to Stirling’s series as well.

By this time Cauchy felt it necessary to publish a paper entirely devoted to continuity (Cauchy 1844a), a concept which played a major role both in his statement of the integral representation theorem and in Laurent’s theorem. Whenever Cauchy referred to continuity, he took the opportunity to mention his *Cours* and the definition [Def 2] given there. He did so even in Cauchy (1844a, 146), the memoir he presented to the Académie on January 22, 1844, where he began by exhibiting an example to show the insufficiency of Euler’s and Lagrange’s definition of a continuous function as one given by a single analytical expression. Indeed, the same function $f(x)$ could be considered either continuous or discontinuous depending on whether it was represented as $\frac{2}{\pi} \int_0^\infty \frac{x^2 dt}{t^2 + x^2}$ or as

$$f(x) = -x \text{ if } x < 0 \quad \text{and} \quad f(x) = x \text{ if } x > 0.$$

One could remove all vagueness and imprecision, Cauchy continued, by adopting the “new” definition [Def 2] that he had given in his *Cours d’analyse*.⁵⁶ Continuity defined that way was, in the wording in Cauchy’s (1844a, 147), “the distinctive character” of functions that can be expanded either in series of positive whole powers of the variables or in Laurent series.

In connection with this statement, he felt it necessary to clarify explicitly what he meant by a continuous function of an imaginary variable $x = re^{i\theta}$: “If, while the argument varies between the limits $[-\pi$ and $\pi]$ and the modulus between two given limits, a real or imaginary function of x remains continuous with respect to the argument and the modulus in such a way as to take the same value when the argument goes from the value $-\pi$ to the value π , this function will be, between

⁵⁶It is worth remarking that more than 20 years after the publication of this volume, Cauchy still referred to the definition of continuity given there as “new”.

the limits assigned to the modulus, what we call a continuous function of the variable x ".⁵⁷ This curious attempt at a definition runs three ideas together. First, there is the obvious geometric interpretation—which, however, Cauchy did not mention—that suggests that this is a definition of continuity of a function defined within an annulus centred at the origin. Then it is clear that Cauchy thought of the function $f(re^{ip})$ as a function of two real variables, r and p , and applied to it the relevant definition of continuity in each variable separately that he had given in the *Cours* (1821a, 45–46). Finally, and in keeping with his earlier difficulties, Cauchy added the condition that the function takes the same value for given r at $p = -\pi$ and $p = +\pi$, thus requiring that the function is single-valued and not branched at the origin.

Then Cauchy stated the “general theorem” that $f(x)$ can be expanded in Laurent series under the condition that both $f(x)$ and $f'(x)$ are continuous between the given limits on $|x|$. In particular, $f(x)$ could be expanded in a Laurent series in an annulus around a value of $|x|$ for which the continuity condition holds for both $f(x)$ and $f'(x)$. Both theorems, Cauchy added, could be “immediately” extended to functions of several variables and to implicit functions as well (under some further conditions). In order to prove this claim he began by establishing some “general formulae”. Supposing that $f(x) = f(re^{i\varphi})$ is continuous for $r \leq R$ together with its derivative, then, by the integral formula, $f(x) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{zf(z)}{z-x} dp$, where $z = Re^{ip}$, and $f(x)$ can be expanded in series as

$$f(x) = a_0 + a_1x + a_2x^2 + \cdots,$$

where $a_n = \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{f(z)}{z^n} dp$. If $\rho = \limsup(|a_n|)^{1/n}$ is the “modulus” of the series $\sum a_n$, then Cauchy added in passing that ρr is the “modulus” of $\sum a_n x^n$ as well as of the series obtained from it by term-by-term differentiation—thus assuming the truth of a theorem that is usually credited to Weierstrass. As a series is convergent when its “modulus” is less than 1, if $f(x)$ or its derivatives become infinite for $r = R$ then $\rho R = 1$. Consequently, $\rho = 1/R$ and r/R is the “modulus” of $\sum a_n x^n$, and for $r < R$ one could estimate “easily” an upper limit of the modulus of the remainder.

Cauchy continued to wrestle with many-valued functions. He next supposed that a function $u = u(x)$ can be implicitly defined as a simple root of $F(u, x) = 0$, depending on a parameter α , say, in such a way that for $\alpha = 0$ the simple root $u(x)$ will be a continuous function whenever $|x|$ is less than a given limit. If, by varying the parameter α , $F(u, x)$ is a continuous function of x , u , and α , then $u(x)$ will in general be a continuous function for $|x| = r < R$, where $|x| = R$ is a value for which $F(u, x)$ has multiple roots. Therefore for $|x| = R$ one has $\frac{\partial F(u, x)}{\partial u} = 0$ and, generally

$$\frac{\partial u}{\partial x} = -\frac{\partial F(u, x)}{\partial x} / \frac{\partial F(u, x)}{\partial u} = \infty$$

⁵⁷See Cauchy (1844a, 147). Cauchy had implicitly referred to the continuity of an imaginary function of an imaginary variable in the same terms as stated here as early as 1823. See, e.g., Cauchy (1823b, 330).

the successive derivatives becoming infinite as well. Thus, r/R is the “modulus” of the series representing the expansion of u in a power series.

Next Cauchy considered a function $f(x)$ that is continuous together with its derivative for $r_0 \leq |x| \leq R$. By putting $y = r_0 e^{ip}$ and $z = R e^{ip}$, Cauchy showed that $f(x)$ could be written as

$$f(x) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{zf(z)}{z-x} dp - \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{yf(y)}{y-x} dp,$$

from which he obtained the Laurent expansion

$$f(x) = \cdots + a_{-2}x^{-2} + a_{-1}x^{-1} + a_0 + a_1x + a_2x^2 + \cdots$$

after the substitutions $a_n = \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{zf(z)}{z^n} dp$ and $a_{-n} = \frac{1}{2\pi} \int_{-\pi}^{\pi} y^n f(y) dp$.

Cauchy illustrated his results with the example $\alpha u^2 - 2u + x = 0$. The root $u(x)$ reduces to $u = x/2$ for $\alpha = 0$ and, for $\alpha \neq 0$ is continuous provided the equation does not have double roots (these are given by the values of u that satisfy both the given equation and its derivative $\alpha u - 1 = 0$). This implies $\alpha x = 1$ and consequently $|x| = 1/|\alpha| = R$. Thus, the root $u = 1 - \sqrt{1 - \alpha x}$ could be expanded in convergent power series for $|x| < R$. In the concluding paragraph on “discontinuous functions” Cauchy discussed the example $e^u = x$ whose root $u = \log r + i\varphi$ is “discontinuous” in Cauchy’s sense when φ crosses from $-\pi$ to π . Then he made a step forward in dealing with multi-valued functions by remarking that in general, if $i\mathcal{R}$ is the increment that a discontinuous functions $f(x)$ assumes by passing from $-\pi$ to π (in geometrical terms, by crossing the x -axis), then $f(x)$ can be represented as

$$f(x) = \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{zf(z)}{z-x} dp - \frac{1}{2\pi} \int_{-\pi}^{\pi} \frac{yf(y)}{y-x} dp - \Delta,$$

where $\Delta = \frac{1}{2\pi} \int_{r_0}^R \frac{\mathcal{R}}{\tau+x} d\tau$.

The following week Cauchy reported to the Académie on a note that the young Swiss mathematician Charles Cellérier⁵⁸ had submitted, and Cauchy and Liouville had been charged to review (Cauchy 1844b). Cellérier’s main theorem was essentially based on the same argument that Cauchy had presented in (1823b) to extend a function $f(x)$ of a real variable to a function $f(x+iy)$ of a complex variable (see Sect. 3.4). Cellérier’s theorem was re-stated by Cauchy as follows: if a real or imaginary function $f(x)$ is such that $f(x) = 0$ for all real values of x , then $f(x+iy) = 0$ when $|y|$ keeps a value less than the smallest of those for which $f(x+iy)$ or its derivative ceases to be finite and continuous. Cauchy went on to

⁵⁸Later a professor at Genf, Cellérier devoted himself essentially to mechanics. He later gave examples of a continuous, nowhere differentiable function and of an infinitely differentiable function that cannot be expanded in Taylor series, which were published posthumously in Cellérier (1890). See JFM 22.0386.01 for Hurwitz’s review.

observe that this theorem allows formulae involving real parameters to be extended to imaginary values of the parameters, and in this connection he referred to his (1825a, 66 and 69) and his (1826b, 381 and 387).

3.5.4 Chebyshev's Criticism

By 1844 Cauchy's papers on complex analysis had found an attentive reader in P.-L. Chebyshev, a young mathematician from St. Petersburg who began his scientific career by publishing a short note in Liouville's *Journal* (Chebyshev 1843). Butzer and Jongmans (1989) have conjectured, admittedly without any documentary evidence, that Chebyshev may have visited Paris in 1842. In the autobiographical report that Chebyshev wrote in the autumn of 1852, when he was appointed to St. Petersburg university, he described his visit in Paris during that summer and said that he had collaborated with Liouville since 1842 (in *Oeuvres* 2, vii–xviii). Whether he visited Paris in 1842 or not, his 1843 paper was little more than an exercise. There he stated (without proof⁵⁹) a very general theorem on multiple integrals that he verified in two particular cases. In the first case he re-obtained Liouville's result

$$\int_0^\infty \int_0^\infty \dots \int_0^\infty x^{a-1} y^{b-1} z^{c-1} \dots f(x+y+z+\dots) dx dy dz \dots =$$

$$\frac{\Gamma(a)\Gamma(b)\Gamma(c)\dots}{\Gamma(a+b+c+\dots)} \int_0^\infty u^{a+b+c+\dots-1} f(u) du.$$

In the second case he reduced the integrals

$$\int_0^\infty \int_0^\infty \dots \int_0^\infty \cos ax \cos by \cos cz \dots f(x^2 + y^2 + z^2 + \dots) dx dy dz \dots$$

to a suitable expression involving only one integral, which he recognised agreed with formula (61) of Cauchy (1823b, 290).

More interesting was (Chebyshev 1844), a short note published in Crelle's *Journal* dealing with the conditions that Cauchy had required for his integral representation theorem. Chebyshev began by remarking that according to "Cauchy's rule", the Taylor expansion

$$f(a) + \frac{z}{1} f'(a) + \frac{z^2}{2!} f''(a) + \dots + \frac{z^n}{n!} f^{(n)}(a) + \dots$$

⁵⁹The proof was provided by Catalan (1843) in a short note which followed Chebyshev's paper.

is convergent or divergent according as $\lim_{n \rightarrow \infty} \left(\left| \frac{z^n f^{(n)}(a)}{n!} \right| \right)^{1/n} \leq 1$. Then he referred to [Cauchy \(1835, 432–433\)](#) to state that

$$f^{(k)}(a) = \frac{1}{2\pi} \int_{-\pi}^{\pi} f^{(k)}(a + Re^{ip}) dp,$$

under the condition that $f^{(k)}(a + Re^{ip})$, $(k = 0, 1, \dots, n)$ are continuous and finite for $r \leq R$ and any p . As integration by parts yields

$$\int_{-\pi}^{\pi} f^{(k)}(a + Re^{ip}) dp = \frac{k!}{R^k} \int_{-\pi}^{\pi} e^{-kip} f(a + Re^{ip}) dp, \quad (k = 1, 2, \dots, n),$$

Chebyshev re-wrote the derivatives $f^{(k)}(a)$ as

$$f^{(k)}(a) = \frac{1}{2\pi} \frac{k!}{R^k} \int_{-\pi}^{\pi} e^{-kip} f(a + Re^{ip}) dp, \quad (k = 1, 2, \dots, n),$$

and observed that

$$\left| \frac{f^{(k)}(a)}{k!} \right| < \frac{1}{2\pi} \frac{\lambda}{R^k} \int_{-\pi}^{\pi} dp < \frac{\lambda}{R^k}, \quad (k = 1, 2, \dots, n),$$

where λ is the maximum value of $|f(a + Re^{ip})|$ in the interval $-\pi < p < \pi$.

Thus, the convergence condition for the Taylor series above reduces to

$$\lim_{n \rightarrow \infty} \left[\frac{|z|^n \lambda}{R^n} \right]^{1/n} < 1$$

or simply to $|z| < R$. Accordingly, the Taylor series is convergent if $|z| < |x|$, where x is such that at least one of $f^{(k)}(a + x)$ ($k = 0, 1, \dots, n$) becomes infinite or discontinuous. Now, [Chebyshev \(1844, 12\)](#) asked, “are these conditions all necessary?” In other words, will the Taylor series be divergent for all z for which $|z| > |x|$? Supposing that $f^{(m)}(a + x)$ becomes infinite or discontinuous for $x = X$, he proved easily that $\lim_{n \rightarrow \infty} \left(\left| \frac{z^n f^{(n)}(a)}{n!} \right| \right)^{1/n} > \frac{|z|}{|X|}$ which allowed him to state the affirmative answer as a “general theorem”. As an illustrative example he gave the expansion

$$(1 + z^2)^{3/2} = 1 + \frac{3}{2}z^2 + \frac{3}{8}z^4 - \frac{3}{8} \left(1 - \frac{5}{6} \right) z^6 + \frac{3}{8} \left(1 - \frac{5}{6} \right) \left(1 - \frac{5}{8} \right) z^8 - \dots,$$

which is convergent or divergent according as $|z| \leq 1$, which is the modulus of $x = i$, for which value the second and the subsequent derivatives of $(1 + x^2)^{3/2}$ become infinite. His theorem, Chebyshev concluded, was an easy consequence of Cauchy’s discoveries, but it “partly contradicted” the “rule” stated by theorem I of

this “illustrious geometer” as stated in (1840a, 46). “The insufficiency of this rule”, Chebyshev argued with deep insight, depended on the fact that Cauchy supposed a definite integral $\int_a^b f(z)dz$ to be expandable in convergent series once $f(z)$ is expandable in convergent series between the limits of integration, “but this happens only in particular cases”.⁶⁰

3.5.5 “A Kind of Paradox”

Apparently unaware of Chebyshev’s paper, some months later, on July 15, 1844, Cauchy presented the Académie with a memoir where he pointed out “a kind of paradox” arising in connection with the expansion of a many-valued function $f(x)$ in power series. One can be led to believe, Cauchy (1844c, 265) argued, that the series always ceases to be convergent when the function ceases to be continuous. In fact, this happens when the function itself or one of its derivatives diverges to infinity at a value of $|x|$ that makes the function discontinuous. But this proposition, Cauchy added, cannot be extended to the case where the function ceases to be continuous without one of its derivatives becoming infinite. Thus, however strange it might seem, the power series expansion of a given function remained convergent to a continuous function even though the function has ceased to be continuous (in his sense). This was the paradoxical conclusion he wanted to explain.

He took the example of the function u as defined by

$$u = \left(1 - x^2 + ix(2 - x^2)^{1/2}\right)^{1/3} + \left(1 - x^2 - ix(2 - x^2)^{1/2}\right)^{1/3},$$

and observed that for real values of x , u was continuous when $\text{Re}(1 - x^2) > 0$, and became discontinuous when $x^2 = 1$. Consequently, according to his previous results, the expansion of u in power series should be divergent for $x^2 > 1$. But a direct inspection showed that this was not the case. An easy calculation allowed him to rewrite u in implicit form as

$$F(x, u) = u^3 - 3u - 2(1 - x^2) = 0,$$

which in turn could be reduced to $u = 2 - \frac{2x^2}{(u+1)^2}$. From this one could deduce the Lagrange series expansion of the function u corresponding to the root of $F(x, u) = 0$

⁶⁰Among his subsequent papers (Chebyshev 1857) is worth mentioning. There he showed that a skilful interpretation of the integration by parts provided an easy method for obtaining Lagrange series. He applied this to the solution of Kepler equation and proved that the expansions of the eccentric anomaly and the vector radius in power series in the eccentricity η are convergent provided that $\eta < 0.66274$, thus re-obtaining Cauchy’s estimate. In addition, he proved that an upper bound for the remainder of the series is given by $(\eta/0.66274)^n$.

that becomes equal to 2 when $x = 0$:

$$u = 2 - \frac{1}{3^2}2x^2 + \frac{4}{3^5.1.2}(2x^2)^2 - \frac{6.7}{3^8.1.2.3}(2x^2)^3 + \dots,$$

which, contrary to the expected value $x < 1$, is convergent for $x < \sqrt{2}$, as Cauchy proved by means of the ratio test.

Looking for an explanation, in § 2 Cauchy repeated in more detail what he had already stated in his (1844a) concerning the moduli and the conditions of convergence of power series, including Laurent series. In addition he proved (1844c, 275) that such series and their term-by-term derived series have the same modulus.⁶¹ Eventually Cauchy re-stated his integral representation theorem Sect. 3.4.3 and Laurent's theorem under the condition that the functions $f(x)$ and $f'(x)$, where $x = re^{ip}$, are continuous for values of r between certain limits $k \leq r \leq K$, where k and K are such that there are values of p where the function or one of its derivatives becomes infinite at $x = ke^{ip}$ and $x = Ke^{ip'}$.

The “paradoxical” behaviour of the Lagrange series above depended on the fact that $F(x, u) = 0$ had multiple roots with respect to u . In fact, $\frac{\partial}{\partial u}F(x, u) = 0$ and therefore $\frac{\partial u}{\partial x} = -\frac{\frac{\partial}{\partial x}F(x, u)}{\frac{\partial}{\partial u}F(x, u)}$ “generally” became infinite. So, said Cauchy (1844c, 282), “one must only exclude the particular case where the value of x for which the equation $F(x, u) = 0$ has equal values satisfies not only the equation $\frac{\partial F(x, u)}{\partial u} = 0$ but also the following: $\frac{\partial F(x, u)}{\partial x} = 0$ ”. Thus he obtained:

$$\frac{\partial F(x, u)}{\partial u} = u^2 - 1 = 0 \text{ and } \frac{\partial F(x, u)}{\partial x} = x = 0.$$

From this it followed that either $u = 1$ and $x^2 = 2$ or $u = -1$ and $x = 0$. In the first case $\frac{\partial u}{\partial x}$ diverged to infinity, as one would expect from the direct inspection of Lagrange series, while in the second case $\frac{\partial u}{\partial x}$ became $0/0$. Finally, for $x = 0$ the function u reduces to 2, and not to -1 . Consequently, its expansion in series is convergent whenever $x^2 < 2$, as is confirmed by the direct inspection of the series.

Even though Cauchy succeeded in explaining the “paradoxical” behaviour of the series in his example, delicate questions concerning the multi-valuedness of functions, branch-points and continuity of complex functions remained unanswered. Freudenthal (1971, 141) has aptly remarked that “from 1821 he [Cauchy] treated multi-valued functions with a kill-or-cure remedy: if branched at the origin, they would be admitted in the upper half-plane only”. According to Freudenthal, “fortunately he more often than not forgot this gross prescription, which if followed would lead him into great trouble, as happened in [1844c]—strangely enough, he wrote this confused paper just after he had taken the first step away from his dogmatism” with his (1844a) paper.

⁶¹ Apparently Cauchy set great store by this result for he re-published this section two years later, on October 19, 1846 as a separate paper in the *Exercices* as Cauchy (1846n).

The last sentence corresponds to Freudenthal's opinion (1971, 140) that "Cauchy had no clear overall view on his own work. Proofs are usually unnecessarily involved and old papers, superseded by newer results, are repeatedly used and quoted. Often he seems to be blindfolded, for example, he did not notice such a consequence of his work as that a bounded regular function must be a constant until Liouville discovered this theorem in the special case of doubly periodic functions". The same could be said about his failing to discover Laurent's theorem.

Freudenthal is right when looking at Cauchy's work from a modern viewpoint. In his rush of publications Cauchy often formulated an idea only to reformulate it in the same (or slightly different) terms some time later, he repeatedly published virtually the same paper, he regularly changed his mind about the best formulation of some of his theorems and returned to old papers and results in order to clarify and extend them. This lack of coherence, coupled with his extraordinarily prolific publications aroused criticism and sarcasm in his contemporaries, as Belhoste (1991, 191–194) has documented. Liouville, for example, said that "Mr. Cauchy was not the kind of person who did not publish what he had discovered or even sometimes rediscovered; he would rather publish it ten times than forget to publish it once".⁶²

3.5.6 Liouville's "General Principle"

Following his (1844c) paper, Cauchy presented some refinements of his previous results on the expansion of functions in power series at subsequent meetings of the Académie in (Cauchy 1844d) and also demonstrated their application to astronomical calculations (see *O.C.* (1), 8, 292–310; 315–324; 336–348). He presented a further note on the subject on December 9, 1844 (Cauchy 1844e), at the very same meeting of the Académie in which Chasles presented a memoir on the geometric properties of elliptic functions and conic sections (Chasles 1844). Commenting on Chasles's memoir Liouville first invited Chasles to extend his geometrical considerations to hyperelliptic integrals, then stated a "general principle" that seemed to him to give the theory of elliptic functions "an uncommon character of unity and simplicity":

If a (single-valued) function is doubly periodic, and if one recognises that it never becomes infinite, one can, from this alone, affirm that it reduces to a constant. (*CR* 19 (1844), 1262)

But he did not publish a proof of this remarkable fact, nor was he to. However, Cauchy immediately recognised the analogy between Liouville's "general principle" for elliptic functions and his own results on functions of a complex variable, and at the very next meeting of the Académie he presented two papers: the first one (Cauchy 1844f) deals with "a remarkable extension" of the formulae on the

⁶²Liouville to Fleury, January 16, 1866, quoted in Neuenschwander (1984), Engl. trl. in Lützen (1990, 85).

expansion of functions in series that he had presented in the previous meetings of the Académie, the second one (Cauchy 1844g) is related to Liouville's principle. In the second paper he referred to a formula from his old paper (1826c) on residues corresponding to the following “fundamental and very general” theorem: if $f(z)$ is a single-valued function of the real or imaginary variable z , and if $zf(z)$ reduces to a determinate constant $\mathcal{F}$ for every infinite value of z then the integral residue of $f(z)$ reduces to the same constant $\mathcal{F}$ (and it reduces to zero if $\mathcal{F} = 0$, a result he had already stated in (1826c)). In addition, he also stated that if $f(z)$ is single-valued and reduces to a determinate constant $\mathcal{F}$ for every infinite value of z , then the function $f(z)$ reduces to $\mathcal{F}$ for any finite value of z .

After emphasising the analogy between this theorem and Liouville's “principle”, Cauchy referred to his even older paper (Cauchy 1814) on definite integrals to remind the reader of the fact that the difference between the values of a double integral that depends on the order of integration can be expressed by a singular integral. In Cauchy's view, this “unique” principle was enough to show that in the statement of his integral representation theorem “one could with rigour avoid resorting to the consideration of derived functions” (Cauchy 1844g, 368). Thus, according to a “judicious observation” made to him by Liouville,⁶³ he had changed his mind with respect to the hypothesis of his integral representation theorem to the effect that the statement of the theorem as given in Turin Memoir ought to be preferred to the one he had given in (1839a, 486).

Rather surprisingly, the same principle seems to have convinced Cauchy in 1839 to add the supplementary hypothesis on derivatives to the statement of his theorem of 1831 and in 1844 to abandon it. However, when dealing with the expansion of functions in power series he added (1844g, 368–369) that the condition on the derivatives should not be completely abandoned because of its usefulness in determining the moduli of the series.

In the concluding part of the paper he referred back to some theorems on residues dating back to 1826 in order to derive the theorem—stated in language that testifies to his profound mixture of confusion and insight—that a continuous function $f(z)$ that becomes zero for every infinite value of z will always reduce to zero for any finite z (Cauchy 1844g, 372). Then, by resorting to the substitution $\frac{f(z)-f(a)}{z-a}$ as he had done in (1831b), from the previous theorem one could derive the “seemingly more general” statement:

if a function $f(z)$ of a real or imaginary variable z remains always continuous, and consequently always finite, it will reduce simply to a constant. (1844g, 372)

He also added another proof based on the residue calculus and remarked that this theorem includes the case of doubly periodic functions (and consequently Liouville's “general principle”) as a particular case.

⁶³There is no record of it in the *Comptes rendus* of the Académie. As Lützen (1990, 125) has conjectured, “it may just have been a private communication”, perhaps a chat they had after the meeting in which Liouville communicated his “general principle”.

Cauchy immediately realised the importance of this result, which had lain within his reach for years but had escaped his attention, and he took up the matter again at the next meeting of the Académie on December 23, 1844 in a note (Cauchy 1844h) where he re-stated his “fundamental principle of the calculus of residues” that if for every infinite value of the variable z the product $zf(z)$ vanishes, then the integral residue of $f(z)$ also vanishes. From this he derived Liouville’s “principle” and, significantly, admitted that in his previous paper (1844g) he had limited himself to stress the analogy between his own and Liouville’s principle because he “did not remember of having published any formula which, in either the general or a particular case” was the expression of the latter principle. However, he added, “such a formula exists in one of my memoirs” (Cauchy 1844h, 378–379).

Then he stated the important theorem that an algebraic or transcendental function can be represented in many cases as an infinite sum of rational functions, each of them becoming infinite for a value of the variable for which the given function becomes infinite, or at least by such a sum augmented with a “complementary function” which remains finite for finite values of the variable. This theorem would become a powerful tool in the hands of Riemann and especially Weierstrass, who in 1880 was to extend it to the representation of an analytic function with an essential singularity at infinity. Cauchy first established his theorem by resorting to the theory of residues, thus re-obtaining some results he had obtained in (1826c). Then he proved the theorem that if the function $f(z)$ vanishes for every infinite value of z , then the complementary function also vanishes. As a consequence he re-obtained the formulae he had established in his (1843c,d,e,f) in the case of elliptic functions, from which one could eventually obtain the “special theorem rightly considered by one of our *honorables confrères* [Liouville, of course] as particularly applicable to elliptic functions” (1844h, 380). This paper led Remmert (1991, 246) to state mistakenly that Liouville’s principle “originated with Cauchy who derived it [in (1844g, 378)] via his residue calculus”.⁶⁴ We will come back to this matter in Chap. 4 when discussing Liouville’s work, and his reaction to Cauchy’s claims.

In (1844h) Cauchy also stated his “fundamental principle” for continuous functions. Indeed, continuity as defined in his *Cours d’analyse* appeared to his eyes as the “great law limiting the existence of formulae” (1845a, 32), as he claimed at the end of a somehow poetically inspired passage on the progress of sciences witnessing the zeal with which the human spirit tends to the truth by generalising more and more, and by launching itself towards infinity. This tendency of the spirit, Cauchy added, has for a long time justified the “generality of algebra” as a kind of evident principle of analysis whereas, instead, the formulae subsist only under certain conditions such as the law of continuity. Cauchy’s aim in (1845a) was to

⁶⁴The direct derivation of the “principle” via Cauchy’s inequalities was later to be given by Jordan in his *Cours d’analyse* (2nd ed. vol. II, 308, and on p. 341 for the particular case of elliptic functions).

establish some properties of continuous functions of a complex variable $x = re^{ip}$ by generalising to closed paths the results he had obtained for x varying in an annulus. Resorting to a geometric language, he began by considering r and p as polar coordinates of a moving point P in the plane, and $f(x)$ as a continuous function for P varying within a domain bounded by two continuous (non-intersecting) curves. In addition, he supposed that for a given x , corresponding to definite values of r and p , and for values close to it, the function can be expanded in convergent Laurent (or Taylor) series $f(x) = \sum a_n x^n$. Then this “equation” will subsist for x (and consequently p) varying by “insensible” degrees. By further supposing that $a_n = 0$ for any n , Cauchy obtained as a corollary that if $f(x) = 0$ for a given x , and for values close to it, then the equation will subsist for “insensible” variations of r and p as long as $f(x)$ remains continuous. As an immediate consequence he stated the theorem:

Suppose that two (continuous) functions of x are always equal to each other for values of x close to a given value. Then on varying x by “insensible” degrees the functions remain equal to each other for as long as they remain continuous (1845a, 39).

Cauchy then remarked that this theorem could be extended to functions of several variables, and added that one could substitute the sum of convergent series for the functions involved in his theorem. By “stretching” the meaning of the concepts, Remmert (1991, 231) interprets Cauchy’s “continuous” functions as meaning “holomorphic” functions and, consequently, considers Cauchy’s theorem above as an “identity principle” for holomorphic functions. As we will see in Sect. 3.6, however, not until 1851 did Cauchy begin to realize fully that the crucial concept involved in his theorems on complex functions was not continuity but analyticity related to Cauchy–Riemann equations. In any case, as Remmert himself observes, Cauchy made no application of his theorem, whose significance was actually first recognised by Riemann and Weierstrass.

Very soon Cauchy’s attention turned to questions related to the approximation of functions of very large numbers, and their applications to astronomy (Cauchy *O.C.* (1), 9, 74–75; 81–121; 190–220). In that context the note Cauchy presented to the Académie on March 31 is worth discussing. After observing that when a function is represented by an integral which cannot be evaluated in finite terms one may resort to integration by series, he then stated explicitly that “it is enough to expand the function under the $\int$ sign in a convergent series, then to integrate every term of the series thus obtained” (Cauchy 1845b, 164). As there are infinitely many different ways to do this even in the case of continuous functions, Cauchy was interested in finding the most convenient ones, i.e. the ones which provide rapidly convergent series. We shall not describe Cauchy’s technique in detail. It is more interesting to remark that as late as 1846 Cauchy had no idea about various modes of convergence, and consequently no doubt about the legitimacy of the procedure he described. Apparently, as we will see in Sect. 3.6, it was only in 1853 that he became aware that his theorem on the sum of a series of continuous functions as stated in (1821a) was not correct.

3.5.7 Refining the Integral Theorem

For almost eight months, from September 1845 to April 1846 Cauchy was almost exclusively absorbed by the theory of permutation groups, and his works intertwined with the contemporary publication of Galois's papers edited by Liouville (see [Lützen 1990](#), 559–580). Then he turned to analytic geometry, and he returned to subjects in analysis only in the summer of 1846 when he reacted to a criticism that Ernest Lamarle, a friend of Liouville, had addressed to his Theorem I as stated in [Cauchy \(1839a\)](#) and repeated in [Cauchy \(1840a,c,d\)](#).

Lamarle had published a paper in the April issue of Liouville's *Journal* in which he showed that the condition of continuity in the statement of Cauchy's integral representation theorem “can be omitted as far as the derivative is concerned” and, in addition, “it is anyway insufficient at least if a certain periodicity of the function is not implied” ([Lamarle 1846](#), 129)—by which he meant that the function $f(z)$ with $z = re^{i\vartheta}$, ($0 \leq \vartheta \leq 2\pi$) assumes the same value for $\vartheta = 0$ and $\vartheta = 2\pi$. In a paper that appeared in the August issue of the same *Journal*, [Cauchy \(1846a\)](#) pointed out that the latter condition was included in his own definition of continuity in order to avoid multi-valuedness, as he had recently stated in (1844a, 147), while he had already abandoned the former condition in his (1844g, 368–369).

After stating Cauchy's integral representation theorem by adding to its 1831 version the additional condition that the function must take the same value for $\vartheta = 0$ and $\vartheta = 2\pi$, in other words that the function be single-valued, [Lamarle \(1846](#), 137) had concluded that outside the region where convergence was assured, “the series becomes divergent”. Rejecting this conclusion, Cauchy referred to his (1844c) paper to explain the possible “paradoxical” behaviour in the expansion of functions in series. He explained this with the very same example suggested by Lamarle: $f(x) = (1+x)^m$, where m is a rational number, $|x| < 1$ (and in addition $|\vartheta| < \pi/2$ in order to avoid multi-valuedness). Eventually, after quoting the paragraph of his *Cours* (1821a, 204) concerning the conventions needed when passing from the real to the imaginary, Cauchy claimed that “the nature of conventions has a marked influence on the character of the functions regarded as continuous” to the extent that, by changing the system of conventions, continuous functions can be made to be discontinuous, and vice versa. Accordingly, Cauchy concluded, in order to apply his integral representation theorem above to the expansion of functions it is most convenient to adopt those conventions which preserve continuity as long as possible ([Cauchy 1846a](#), 46). This is a very peculiar argument, which could hardly be listed among Cauchy's clearest and most rigorous statements. Whatever Cauchy meant by it, apparently on this occasion he thought of the continuity of functions in a way that he had reproached Lagrange and Euler with no more than a couple of years before.

When this paper appeared, Cauchy was fully immersed in revising and refining his previous work on integration (including integration in a complex domain). The first result was a short note ([Cauchy 1846b](#)) that he presented to the Académie on August 3, and which is commonly—but incorrectly—credited by historians

with containing the first general statement of the Cauchy integral theorem (see for instance [Kline 1972](#), 639–640).

In fact, in that paper Cauchy limited himself to stating some results related to analytic geometry and the integral calculus that were contained in a couple of memoirs he was going to publish in the *Exercices*.⁶⁵ After stating a couple of theorems in three-dimensional analytic geometry he turned to the integral calculus and considered a point P in space and quantities $x, y, z, \dots$ varying continuously with P . Given “a region S that lies in a given plane or on a given surface and which has a single closed curve bounding all of it” (1846b, 72), he supposed P to move along the boundary in a determinate sense, s to measure length along an arc of the bounding curve from a given origin on the curve, and K to be a function $K(x, y, z, \dots; \frac{\partial x}{\partial s}, \frac{\partial y}{\partial s}, \frac{\partial z}{\partial s}, \dots)$. Denoting by (S) the value of the complete contour integral $\int K ds$ (i.e. when P has traversed the whole boundary curve) and cutting S into parts $A, B, C, \dots$ by means of lines one has not only $S = A + B + C + \dots$ but also

$$(S) = (A) + (B) + (C) + \dots$$

provided that K remains “continuous and finite” at every point of every contour. Under the same hypothesis, by assuming in particular that

$$K = X \frac{\partial x}{\partial s} + Y \frac{\partial y}{\partial s} + Z \frac{\partial z}{\partial s}, \dots,$$

where $X, Y, Z, \dots$ are such that $Xdx + Ydy + Zdz + \dots$ is an exact differential, then the value of the integral (S) remains constant when varying the contour of S by “insensible degrees” as long as K remains “continuous and finite” at every point of the variable contour. Eventually, if K ceases to be continuous and finite but only at points $P, P', P'', \dots$ lying within S , then $(S) = (P) + (P') + (P'') + \dots$ the latter being singular integrals. Otherwise, if K remains finite in S , one has $(S) = 0$. For the proof of these theorems Cauchy simply referred to his lectures given at the École Polytechnique.

In the special case of a planar region, if X and Y are continuous functions of x and y , and $K = X \frac{\partial x}{\partial s} + Y \frac{\partial y}{\partial s}$ then (S) reduces to the double integral

$$\pm \iint \left(\frac{\partial X}{\partial y} - \frac{\partial Y}{\partial x} \right) dx dy,$$

where the ambiguity of sign arises because Cauchy had not established a positive sense of direction on the boundary. Moreover, if $Xdx + Ydy$ is an exact differential, then $\frac{\partial X}{\partial y} = \frac{\partial Y}{\partial x}$ and the integral (S) equals zero.

⁶⁵Contrary to what he promised, he actually did so only in the memoir on analytic geometry, see *O.C.* (2) 13, 307–402.

It is certainly true, as for instance [Remmert \(1991, 198–199\)](#) has observed, that one can immediately derive Cauchy integral theorem from this when X and Y are thought of as the real and imaginary parts, respectively, of a function $f(z)$ whose derivative exists and is continuous. But what matters from a historical point of view is that Cauchy was completely silent about this and limited himself to real variables and functions. Indeed, contrary to the popular version of this story, *there is no reference in (1846b) to either complex functions or complex integration*.

[Freudenthal \(1971, 140\)](#) argued that Green's work in potential theory is likely to have spurred Cauchy to write this paper, adding charitably that Green's formula had been "possibly rediscovered by Cauchy". It is more likely that Cauchy learned of Green's work (1828) from the young William Thomson (later Lord Kelvin) who visited Paris in spring 1845. Thomson had been able to get a copy of Green's *Essay* just one day before leaving for Paris. He brought with him and showed it to Liouville and, possibly, to Cauchy, whom he visited several times.⁶⁶ In any case, there is no doubt that the French mathematical milieu became acquainted with Green's work well before it began to be published by Thompson himself in Crelle's *Journal* in 1850.

According to [Freudenthal \(1971, 140\)](#), "most disappointing of all is, of course, the fact that he still did not grasp the fundamental importance of his *Mémoire* (1825a). He confined himself to rectangular and circular integration paths and to a special case of his integral formula". Freudenthal's disappointment derived from his failure to recognise that as late as 1846 Cauchy was still sticking to the formal conception of complex functions as "symbolical expressions" that he had stated in 1821. In this connection, it is enlightening that one week after he had presented his paper (1846b) Cauchy presented the Académie on August 10 with a note ([Cauchy 1846c](#)) where he referred once again to his *Cours* (1821a, 204). There ([Cauchy 1846c](#), 75) he repeated what he had stated in 1821 about the conventions needed to establish rigorously the passage from real to imaginary. Then he summarised the approach that he had taken in the *Cours* to define elementary complex functions of a complex variable, including the exponential and the logarithmic function (and their combinations) *via* their expansions in power series. In this connection Cauchy repeated once more that in such formulae as $\log x = \log r + ip$ one had to assume that $-\pi < p \leq \pi$. Otherwise $\log x$ would become discontinuous on crossing the real axis. This was also the case for $f(x) = (1+x)^{p/q}$ and $|x| < 1$ as he had recently shown in his reply (1846a) to Lamarle, to which he referred. "Once these principles are admitted" he wrote, "one can make new and more extensive applications of the theorems that depend on the consideration of imaginary variables and continuous functions" ([Cauchy 1846c](#), 78). However, once again, he instead reformulated some of his old results from the calculus of residues as applications.

As promised, an extended version of this paper bearing the very same title ([Cauchy 1846m](#)) was published in the *Exercices* on October 19, 1846. There Cauchy

⁶⁶For Thompson's stay in Paris, and his contacts with Liouville and Cauchy, see [Lützen \(1990, 135–146\)](#).

repeated word for word what he had stated in the *Cours* about the fact that imaginary expressions need new conventions before being used, otherwise they “do not signify anything, and have no meaning”. After introducing the algebraic and trigonometric form of complex numbers and variables, he remarked that nothing prevents one from considering the modulus and the argument of a complex expression as polar coordinates in the plane. But in fact he made no use of this geometric interpretation, and the paper contains nothing not already published in his previous work.⁶⁷

A break in Cauchy’s flow of notes on integration was provided by the report (1846d) he wrote on a memoir on Lagrange series that the Italian mathematician Felice Chiò⁶⁸ had submitted to the Académie. Lagrange had stated in Note XI to his (1798) that the value of x of which his series gave the expansion was the smallest root (in absolute value) of the equation $u - x + f(x) = 0$. Contrary to this claim, Chiò showed with counter-examples that Lagrange’s statement could lead to incorrect results and added some explanations of his own.

In spite of the great “weight” one had to give to “Lagrange’s authority in analysis”, Cauchy was pleased to state that Chiò was right and recommended his paper for publication in the *Recueil des savants étrangers*. Turning to some of the latter’s theorems on the numerical solutions of the equations, Cauchy did not miss the opportunity to state that some more general results than Chiò’s were to be found in his own (1837b, c), and he followed his report with a paper (1846e) where he showed that it was enough to assume $f(x) = x^2 - ax + b$ with $b = a^2/4$ to verify the inexactitude of Lagrange’s statement. Then he considered the variable $z = x + iy$ as representing a point in the (x, y) -plane in such a way that “to any value of z will correspond a determinate system of values of x and y , and consequently a determinate point in the (x, y) -plane” (1846e, 116). This geometric setting clarified the consideration of the families of curves, which he had introduced in his (1837b, c) and whose content he went on to repeat in detail while relating it to some of Chiò’s results.

Cauchy resumed his work on integration on September 1846 when he presented the memoir (Cauchy 1846g) to the Académie (announced in Cauchy 1846f). Here he summarised the content of his (1846b) by referring only to the case of a plane domain S . Then he gave the formula $(S) = 2\pi i \mathcal{E}(f(z))$ that extended to any closed

⁶⁷In the concluding pages Cauchy referred once more to his criticism of Lamarle, and in a *Post scriptum* added to this paper Cauchy was pleased to remark that Björling, in a memoir on infinite series submitted to the Academy of Uppsala in 1846 had seemingly shared his view about the conventions needed when dealing with x^a and $\log x$. According to Gårding (1998, 14) “one of the recurrent themes and the object of a correspondence [of Björling] with Cauchy was the definitions of the functions x^y and $\log_\beta x$ when x and y are complex numbers and β is positive”. In 1846 Björling submitted to the Academy of Uppsala another memoir dealing with the convergence of series which, perhaps at Cauchy’s suggestion, was translated into French and eventually appeared in Liouville’s *Journal* (Björling 1852).

⁶⁸A professor at the Military Academy in Turin, Chiò had previously had a lively argument with the Italian followers of Lagrange, and Menabrea in particular, because of his criticism of Lagrange’s work.

contour s the formula that he had found in (1826c, 133) for rectangular domains. In his concluding remarks Cauchy said that his formulae could be applied to the study of elliptic functions and promised to resume the matter in future papers.

At the very same meeting of the Académie on September 21, 1846 Cauchy presented yet another paper, (1846h), this one dealing with the imaginary integrals of differential equations. He referred to his (second year) lectures at the École Polytechnique to state that a system of differential equations can always be reduced to a system of first-order differential equations. Thus, given such a system of n differential equations of the n variables $x(t), y(t), z(t), \dots$ depending on a parameter t , and the corresponding initial values $\xi, \eta, \zeta, \dots$ for $t = \tau$, the values of $x, y, z, \dots$ will “usually” be well determined, and the differences $x - \xi, y - \eta, z - \zeta, \dots$ will be “true definite integrals” with respect to t , taken from the origin $t = \tau$, of the kind he had considered in his “memoir on definite integrals taken between imaginary limits”—adding an implicit reference to his (1825a) that has escaped most historians. For, he explained, not only the integrands but also the parameter t can well be imaginary (1846h, 146). Indeed, on resuming the geometrical setting established in his previous paper (1846g) he observed that the real and imaginary parts of t can be thought of as the rectangular coordinates of a moving point P in a plane. By calling the point corresponding to $t = \tau$ the origin O , he considered a line joining O to P . This allowed him to introduce the idea of a curvilinear integral along the line OP , observing that “when one reaches the point P , the value of the integral [e.g. $x - \xi$] will generally be independent of the line one has traversed” (1846h, 146–147).

It could also happen that the integrand became infinite at a point on the line OP . In this case, a small change in the line OP would often produce a sudden change in the value of the integral. Cauchy was pleased to remark that such a fact offered “some analogy” with what he had been pointing out since 1814 concerning the dependence of the value of a double integral on the order of integration. He was also pleased to add that such a fact explained such “paradoxical” formulae as the one obtained by Poisson (1820, 324–325) who had given the real integral $\int_0^\infty \frac{dx}{1-x^2}$ the imaginary value $-\frac{1}{2} \log(-1)$.

Cauchy was quick to recognise that in his (1846b) and in all of the related papers he had tacitly supposed that the functions involved were single-valued⁶⁹ in S . But, he said, “nothing prevents” one from supposing that the function involved takes several different values after several complete turns of the (closed) contour. This happens, for instance, with the integral of a function that contains the roots of algebraic or transcendental functions. In this case, it may happen that one root will change into another one after a complete turn of the moving point around the contour. In this connection the moduli of periodicity (“indices de périodicité”, in Cauchy’s phrase) appeared, which in general cannot be represented by residues but only by integrals, as he had explained in his previous papers (1846i, 165). The idea of branching of

⁶⁹As he put it, “the function under the $\int$ sign resumes precisely the same value when returning to the starting point after traversing the whole curve”. (1846i, 154).

algebraic functions can be recognised in Cauchy's words, but he did not develop it in any detail. Instead, as he had promised, he briefly alluded to the application to elliptic functions, claiming that his results provided the "complete solution of the serious difficulties" to which Eisenstein had alluded (see Chap. 4).⁷⁰

He resumed the subject in Cauchy (1846o) by observing that Eisenstein (1845, 447–448) had "very judiciously" pointed to the difficulties that occur when dealing with elliptic functions and Abelian integrals "by considering their theory as a branch of the integral calculus" (Cauchy 1846o, 171). When one considers the doubly periodic function $\sin am\ u$ as the inverse function of an elliptic integral, Cauchy observed, one is led into "the contradiction of attributing infinitely many distinct values to a definite integral, whose value is unique" (Cauchy 1846o, 172). He was pleased to remark that in order to avoid such a difficulty Eisenstein had followed the route he himself had suggested in his 1843 papers, i.e. to establish the theory of elliptic functions on the consideration of infinite products.

The aim of Cauchy's paper (1846o) was to show how a theory of elliptic, and even Abelian integrals, can be built on his theory of "complete" integration of differential equations, in such a way that $\sin am\ u$ should be considered the inverse function of the "complete" integral of the relevant differential equation. With this aim in mind, he established some general theorems beginning with the following: given a system of n variables $x(t), y(t), z(t), \dots$ defined by a system of first-order differential equations

$$\frac{dx}{dt} = X(x, y, z, \dots), \quad \frac{dy}{dt} = Y(x, y, z, \dots), \quad \frac{dz}{dt} = Z(x, y, z, \dots), \quad \dots$$

satisfying the initial conditions $x(\tau) = \xi, y(\tau) = \eta, z(\tau) = \zeta, \dots$, if $X, Y, Z, \dots$ are continuous with respect to $x, y, z, \dots$ in a neighbourhood of the initial values, then for $|t - \tau|$ less than a certain limit one could satisfy the given conditions by giving $x, y, z, \dots$ values "very close" to $\xi, \eta, \zeta, \dots$ (1846o, 176–177)

Then he went on to allow the variable t to assume complex values, which he thought of as the rectangular coordinates of a moving point in the plane with origin at a point O corresponding to $t = \tau$. According to the previous theorem, under the hypothesis that $X, Y, Z, \dots$ are finite and continuous one could move t to a point O' very close to O , and from there to another point O'' , and so forth, the points $O, O', O'', \dots$ lying on a straight line or on a curve. The values of the corresponding (rectilinear or curvilinear) integrals will vary by "insensible degrees" with t (or, equivalently, with the moving point on the curve $O, O', O'', \dots$).

Then he supposed that one (or several) of the functions $X, Y, Z, \dots$ become discontinuous or infinite at a point C on the curve $O, O', O'', \dots$. If the discontinuity consists in a sudden change of the values of a function which otherwise remains

⁷⁰A week later, on October 19, 1846, Cauchy presented the Académie with a second paper on the subject, where he treated in greater detail what he had sketched in his (1846h) on the integration of systems of differential equations (see *O.C.* (1) 10, 169–186).

finite, then the integrals will still have a finite and well-determined value. Otherwise, if one (or several) of the functions becomes infinite at C , then the integrals may become infinite or indeterminate, and one has to resort to their principal values.

In his concluding remarks he limited himself to stating that in order to give the complete integrals the largest possible extension the curve $O, O', O'', \dots$ may assume any form whatsoever provided that it avoid crossing points $C, C', C'', \dots$ at which $X, Y, Z, \dots$ become infinite. Accordingly, the complete integrals will be multi-valued, or possibly infinitely many valued, functions of t .

At the next meeting of the Académie, on October 26, Cauchy presented a continuation of this study (1846p). In the last section (1846p, 194–195) he set himself the problem of finding the integrals of the given differential system along an arbitrary path $O, O', O'', \dots$ in the plane, under the hypothesis that $X, Y, Z, \dots$ become discontinuous only by becoming infinite at a (finite) number of points $C, C', C'', \dots$. The rather confused solution he gave there was nonetheless to provide a spur to Puiseux, who some years later turned it into a fruitful tool for studying algebraic functions and their integrals (see Sect. 3.6.1). In particular, apparently it was only after seeing its utility in Puiseux's hands that Cauchy fully realised the importance of his integral theorem of 1825.

Kline (1972, 638) has rightly observed that “Cauchy must have thought long and hard to realize that some relations between pairs of real functions achieve their simplest form when complex quantities are introduced”. In fact, the crucial year 1846 showed that Cauchy was well acquainted with the basic facts about complex functions, including the idea of representing complex variables in a plane, the integral theorem—now stated in a more general setting than in his (1825a)—the concept of a residue and the integral formula. Yet a comprehensive theory of complex functions was still lacking. In addition, there were unanswered questions about the nature of branch-points and the multi-valuedness of complex functions and integrals that he was unable to deal with in a satisfactory way.

3.5.8 From “Algebraic Equivalences” to “Geometrical Quantities”

In facing these problems Cauchy had been led to consider paths in the (complex) plane, which must have rendered inadequate even in his own eyes the conception of complex quantities that he had given in his *Cours* but which he had apparently not yet abandoned. He referred to it again in June 1847 when he proposed a theory of “algebraic equivalences” in order to gain clarity by banishing imaginaries and by “reducing the letter i to no more than a real quantity”. Referring to the conception of the imaginaries as symbolic expressions which he had presented in (1821a), Cauchy claimed that by adopting it there were no need left to “torture one's mind” by trying to discover the meaning of the symbol i . But the theory of imaginary would be brought “within everybody's intelligence” by reducing the imaginary expressions

to be real quantities. Although such a reduction seemed “most unlikely, and even impossible” at first sight, Cauchy claimed to have been happily successful (Cauchy 1847a, 313).

Inspired by Gauss’s and Kummer’s work on the classes of quadratic forms, Cauchy’s basic idea was to call two real polynomials equivalent if they give the same remainder when divided by $i^2 + 1$. Thus, calculations with complex expressions will no longer involve $\sqrt{-1}$ —“which we repudiate completely without regret” (1847b, 100). Instead, he transformed the former imaginary equations into new algebraic equivalences (mod $i^2 + 1$). In the expanded version of this paper (1847b), published in the *Exercices* Cauchy extended his theory of equivalences to trigonometric and exponential functions *via* their expansions in power series. Considering, for example, the exponential series

$$e^x = \sum_{n \geq 0} x^n / n!$$

and writing ix in place of x , he obtained

$$e^{ix} = \sum_{n \geq 0} (ix)^n / n! = \sum_{n \geq 0} (-1)^n x^{2n} / n! + i \sum_{n \geq 0} (-1)^n x^{2n+1} / n!$$

from which Euler’s relation followed:

$$e^{ix} = \cos x + i \sin x \quad (\text{mod } (i^2 + 1)).$$

In spite of his claims, however, two years later, after “new and mature reflections” Cauchy (1849a, 152) changed his mind again and decided to adhere to the theory of geometric quantities (*quantités géométriques*), i.e. the nowadays usual interpretation of complex numbers (and variables) in the complex plane. On this occasion Cauchy remembered the names of many who had contributed to this theory, including Buée (1806)—Cauchy had certainly known his paper since 1821 at the latest, for he referred to it (without mentioning Buée by name) in Note I appended to his *Cours* (1821a, 333)—and Argand (1806). But he avoided mentioning either Hamilton (1837) or Gauss (1832), claiming instead that he had been led to his “new” conception by profiting from the ideas that Saint Venant had presented in a note on geometric sums and differences published in 1845 in the *Comptes rendus* of the Académie.⁷¹ Cauchy (1849a) is simply a summary of a more extended note that he was going to add as Note I to an old unpublished memoir on integral calculus that he had presented to the Académie on December 27, 1824 (!) and had now decided to publish (1850, 282–317). Note I was also essentially re-published by Cauchy in

⁷¹Later on, in 1853 Cauchy claimed once more to have found in Saint Venant’s (1845) paper the source of inspiration to his theory of “algebraic keys” which essentially reduced to a method for solving linear systems. This raised an argument with Grassmann, who wrote to the French Académie to claim priority; the commission charged by the Académie to settle the matter (of which Cauchy was a member) never came to a conclusion. See Grassmann (1894–1911, 3.2, 174–203).

the *Exercices* as (1849b, c) with the short addition (1849d). Cauchy also profited from the publication of his old memoir by adding to it short summaries of memoirs on the elements of the theory of geometric quantities and the power series of these quantities that he went on to publish in the *Exercices* (see *O.C.* (2) 14, 241–249; 265–282, Notes II–IV, pp. 318–324.)

It was in this geometrical setting that in the 1850s Cauchy began re-establishing the results he had been obtaining since the 1820s.

3.6 Last Papers

The revolution of February 1848 that overthrew the monarchy of Louis-Philippe was greeted with joy by Cauchy. Although his hopes that the Duke of Bordeaux, his former pupil in Prague, could succeed the Orleans on the throne were very soon dashed, Cauchy felt he could be reinstated in a teaching position because the new government of the Republic had eliminated the oath of loyalty. And indeed, in 1849 he was appointed to the chair of mathematical astronomy at the Sorbonne that had been left vacant by the astronomer Leverrier, who had moved to the chair of physical astronomy. Soon thereafter another chair fell vacant. In the wake of the February revolution Libri, who was charged with having stolen rare books and manuscripts from the French public libraries, lost the protection of the former prime minister Guizot and fled from Paris to London, abandoning his chair at the Collège de France. In 1851 Cauchy applied for that chair as well, but it went to Liouville after a controversial election. Both Cauchy's and Liouville's lectures were devoted to the theory of complex functions, and they exerted a decisive influence on a group of young mathematicians, including Hermite, Puiseux, Briot and Bouquet, who attended their lectures. The latter's book *Théorie des fonctions doublement périodiques* (1859) is clearly inspired by these courses.

According to Belhoste (1991, 232), “on the basis of his work as a teacher, Cauchy sensed that it was necessary that his complex variable theory be put on a clear and rigorous footing”. Consequently, in spite of his “reluctance to use a geometric approach”, he abandoned both his algebraic theories of imaginaries—the one presented in his (1821a), and the more recent theory of algebraic equivalences—and came to adopt the theory of imaginary quantities. Teaching duties could well have played a role, but the fact that Cauchy's own research on integration in 1846 had eventually convinced him of the inadequacy of a purely algebraic approach was perhaps more decisive. Be that as it may, in the last years of his life Cauchy devoted himself to presenting complex function theory in a systematic way by rewriting his previous results in terms of geometric quantities. In so doing he introduced new terminology and, more significantly, he was able to obtain some important new results.

After a gap of more than a year since his (1849a), and some thirty notes presented weekly to the Académie on various subjects, including vibrations of the ether, the theory of light and other topics in geometrical optics, on January 20, 1851 Cauchy resumed his favourite analytic work with a memoir on irrational functions

(Cauchy 1851a). By then he had in his hands since November 19, 1849 a memoir on elliptic functions that Hermite had presented to the Académie and that Cauchy (jointly with Sturm) had been charged to report upon. In it, Hermite had politely stated that his theory was essentially based on the results derived from Cauchy's integral theorem.⁷² However, after more than a year his memoir was still waiting for Cauchy's report. Nonetheless, there is hardly any doubt that his academic duty with respect to Hermite's memoir was not the spur prompting Cauchy to resume his work in complex analysis. That was provided by the contemporary work of Puiseux⁷³ who had recently begun to publish a long memoir on algebraic functions in Liouville's *Journal* (Puiseux 1850) and had discussed his new research with Cauchy himself (1851a, 300).

3.6.1 Puiseux's Memoir

Puiseux (1850, 367) began by considering a function $u(z)$ defined by a polynomial equation $f(u, z) = 0$ such that $u(c) = b$, and raised the question of determining the value of u corresponding to $z = k$ supposing that the path joining c to k is known. Puiseux formulated the problem in a geometric setting by referring to Cauchy's papers of 1846; this allowed him to consider paths in the complex plane. He then supposed that $f(u, c) = 0$ has simple roots, let b_1 be one of them, and $u_1(z)$ be a continuous function satisfying $f(u, z) = 0$ such that $u_1(c) = b_1$. He considered a path joining c with k in such a way that at none of its points does u_1 become infinite or equal to another root of the given equation (in modern terms, u_1 is a single-valued branch of an algebraic equation in a (simply connected) domain that contains no singular points). On such a path, u_1 will take a value h at k that is constant for infinitesimal variations of the path (with fixed end-points c and k).

This “fundamental proposition of the theory” (1850, 369) could, he went on to say, be extended to any two paths joining the fixed end-points where one path can be changed into the other by continuous deformations provided that in so doing the paths do not cross any point at which u_1 becomes infinite or equal to another root of $f(u, z) = 0$. Puiseux (1850, 371) prefigured topological (more precisely, homotopic) concepts by stating that if k coincides with the starting point c , then u_1 will take the same initial value b_1 if the closed path thus obtained “shrinks to the point” c without crossing any point where u_1 becomes infinite or equal to another root of

⁷²See CR 29 (1849), 594–595.

⁷³Victor Puiseux had graduated from the École Normale Supérieure in Paris in 1841 with a thesis in astronomy and celestial mechanics. In 1849 he took up a teaching position there as *maître de conférences*. Puiseux attended Cauchy's courses at the Faculté des sciences, and according to Belhoste (1991, 233) “these two men also quickly became good friends”. In 1857 he was to succeed Cauchy as professor at the Faculté des sciences and subsequently he held a position at the Bureau de Longitudes. He was elected to the Académie des sciences in Paris in 1871; Bertrand (1890b) remarked “The election was due to his merit, but its unanimity, to his character”.

the equation. Next he turned to the consideration of the curvilinear integral $\int_c^k u_1 dz$ between imaginary values. Under the same hypotheses about u_1 and the paths he proved, by following the steps of (Cauchy 1825a) and resorting to the calculus of variations, that the integral $\int_c^k u_1 dz$ has a constant value which is independent of the path. Thus, $\int u_1 dz = 0$ in the case of a closed path⁷⁴ that “shrinks to a point” without crossing any critical value. Then he expanded u_1 in power series following a method based on the integral representation theorem as given in Cauchy’s Turin *Mémoire* and related papers.

Puiseux next supposed that $a', a'', a''', \dots$ denoted points corresponding to multiple roots (or infinities) of $f(u, z) = 0$ and joined them to the starting point c by means of straight lines. Denoting by $\rho > 0$ a quantity less than the least of the segments $ca', ca'', ca''', \dots$, Puiseux considered a disk σ centred at c with radius ρ (so it contains none of the points $a', a'', a''', \dots$). Then, by considering any point z within σ (or on the circumference $\partial\sigma$, say) and any (continuous) path joining c with z that does not leave σ , he observed that as a consequence of his previous results u_1 is a well-determined, single-valued function of z , $u_1 = \varphi(z)$, say. Calling γ any point in σ , he considered the expression $\frac{\varphi(z) - \varphi(\gamma)}{z - \gamma}$ which is finite, equals $\varphi'(z)$ for $z = \gamma$, and takes the same value after a complete turn of z on $\partial\sigma$. Then

$$\int_{\partial\sigma} \frac{\varphi(z) - \varphi(\gamma)}{z - \gamma} dz = 0 \quad \text{and} \quad \varphi(\gamma) \int_{\partial\sigma} \frac{dz}{z - \gamma} = \int_{\partial\sigma} \frac{\varphi(z) dz}{z - \gamma}.$$

As $\int_{\partial\sigma} \frac{dz}{z - \gamma} = \int_{\partial\varepsilon} \frac{dz}{z - \gamma} = 2\pi i$, where $\partial\varepsilon$ is a circle centred at γ with (arbitrarily small) radius ε , one has $\varphi(\gamma) = \frac{1}{2\pi i} \int_{\partial\sigma} \frac{\varphi(z) dz}{z - \gamma}$. By expanding $\frac{1}{z - \gamma}$ as a power series in $\frac{\gamma - c}{z - \gamma}$

$$\frac{1}{z - \gamma} = \frac{1}{z - c} + \frac{\gamma - c}{(z - c)^2} + \frac{(\gamma - c)^2}{(z - c)^3} + \dots,$$

Puiseux eventually obtained the expansion of $\varphi(\gamma)$ as a power series in $(\gamma - c)$:

$$\varphi(\gamma) = \frac{1}{2\pi i} \left(\int_{\partial\sigma} \frac{\varphi(z) dz}{z - c} + (\gamma - c) \int_{\partial\sigma} \frac{\varphi(z) dz}{(z - c)^2} + (\gamma - c)^2 \int_{\partial\sigma} \frac{\varphi(z) dz}{(z - c)^3} + \dots \right)$$

for any γ in the disk σ centred at c . So, as Brill and Noether (1894, 198) observed, Puiseux provided a genuine extension of the results of Cauchy, who in (1831b) and related papers always had considered only expansions in power series of z (i.e. in a disk centred at the origin).

Puiseux then noted that the series above provides the expansion for $z = \gamma$ of the root of $f(u, z) = 0$ which takes the value b_1 for $z = c$ and varies continuously with

⁷⁴Puiseux explicitly emphasised that the path was allowed to intersect itself, like a lemniscate, and also to wind about c an arbitrary number of times.

z , always supposing that the path joining c to γ lies within σ .⁷⁵ Then he went on to show that the power series above could be replaced by more general series that are convergent in domains other than disks (e.g. lemniscates).

In the second part of his memoir Puiseux allowed the path to cross critical points $a', a'', a''', \dots$. He first considered the case when the function u_1 becomes equal to a multiple root of $f(u, z) = 0$ and excluded points where the function becomes infinite by assuming that the coefficient of the greatest power of u in $f(u, z) = 0$ is independent of z (and therefore cannot vanish).

Supposing that $f(u, z) = 0$ has p coincident roots equal to b at $z = a$, it follows that for $u = b$

$$f(u, a) = \frac{df(u, a)}{du} = \frac{d^2 f(u, a)}{du^2} = \dots = \frac{d^{p-1} f(u, a)}{du^{p-1}} = 0$$

but $\frac{d^p f(u, a)}{du^p} \neq 0$. He considered an infinitely small contour around a and a point c on it. Then the roots $\beta_1, \beta_2, \dots, \beta_p$ of $f(u, c) = 0$ will differ very little from b . He remarked that after a complete turn of z along the contour, the roots of $f(u, z) = 0$ which at the starting point differ very little from the simple roots of $f(u, a) = 0$ will take again their initial values, and asked about the corresponding behaviour of $u_1, u_2, \dots, u_p$.

Puiseux began by considering the case when $\frac{\partial f(u, z)}{\partial u}|_{z=a, u=b} \neq 0$. After a thorough, detailed analysis he concluded that in this case the functions $u_1, u_2, \dots, u_p$ which equal b at $z = a$ “can be disposed on a circle in such a way that after a complete turn of z on an infinitely small contour around a the final value of each of them will equal the initial value of the following one” (Puiseux 1850, 388). These functions built what Puiseux called a “circular system”. According to Cauchy (1851g, 327), “the novelty and the importance” of Puiseux’s memoir lay above all in this result. Stated in modern terms, Puiseux proved that after a complete turn on the contour the value β_1 of u changes into $\beta_2 = \varepsilon \beta_1$, where ε is a p th root of unity. Thus, after p turns β_1 changes back into β_1 . Then he proved that the functions $u_1, u_2, \dots, u_p$ can be expanded in convergent power series of $(z - a)^{1/p}$ within a disk centred at a with radius given by the least of the distances $aa', aa'', aa''', \dots$. This is where the celebrated “Puiseux series” first occurred.

The case when $\frac{\partial f(u, z)}{\partial u}|_{z=a, u=b} = 0$ required a much more delicate analysis involving the study of various possibilities that Puiseux carefully analysed (1850, 388–404) before concluding that the functions $u_1, u_2, \dots, u_p$ can in general be distributed in a certain number of circular systems, each of them including a varying number of functions (possibly only a single function).⁷⁶ If μ is the number of

⁷⁵According to Markushevich (1996, 192), Puiseux here established “the identity (under the hypothesis of his study) of continuous extension [of a branch of an algebraic equation] with analytic continuation”.

⁷⁶In modern terms, they correspond to the orbits of the subgroups of the Galois group of $f(u, z) = 0$, of which he calculated the order.

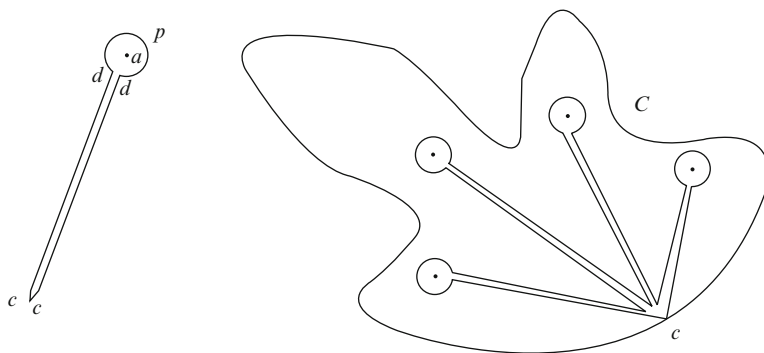


Fig. 3.2 A loop (*left*) and an elementary contour equivalent to the contour C (drawn by Jeremy Gray)

functions belonging to a given circular system, then each of them, u_k say, with $1 \leq k \leq \mu$, can be expanded in a suitable, convergent power series of $(z-a)^{1/\mu}$ within a disk centred at a with radius given by the least of the distances $aa', aa'', aa''', \dots$. Puiseux made it clear that μ equals the number of turns the point z needs to complete on a very small contour about a to let the function u_k take again its initial value.

Having illustrated the results obtained so far with simple examples such as $u^m = (z-a)(z-a')(z-a'')(z-a''') \dots = 0$ (with $a' \neq a'' \neq a''' \neq \dots$) or $u^3 - u - z = 0$, he went on to introduce the concept of an “elementary contour” (or *loop*, according to later wording), which was to play a fundamental role in his theory. Supposing that $a, a', a'', a''', \dots$ are values of z such that $f(u, z) = 0$ has multiple roots, let $f(u, a) = 0$ have p roots $u_1 = u_2 = \dots = u_p = b$. An “elementary contour” corresponding to any point c and the point a was defined by Puiseux as any path (typically, a straight line) joining c to a point d very close to a without crossing any of the points $a, a', a'', a''', \dots$ that proceeds from d onwards in an infinitesimal contour around a ($d p d$, say, typically, a circle) and is then followed by the initial path now traversed from d to c , see Fig. 3.2.

Puiseux proved that the functions $u_1, u_2, \dots, u_p$ behave with respect to the elementary contour $c d p d c$ as they do with respect to any closed path through c that encloses only the point a , i.e. they distribute themselves according to the same circular systems as he had previously presented. Accordingly, one can replace any closed path starting from c and enclosing only the singular point a by a similar elementary contour, denoted (a) . More generally, a closed path starting from c and enclosing all the points $a, a', a'', a''', \dots$ without crossing any of them could be reduced to a suitable sequence of loops $(a), (a'), (a''), \dots$ any two of which have only c in common.

Puiseux pointed out that in doing so one has to distinguish two directions of travel—direct and inverse—for each loop, which he denoted, respectively, by $(+a)$ and $(-a)$. He called the relevant sequence of loops the “characteristic” of a contour traversed in a given direction. A closed path which shrinks to a point without crossing any of the $a, a', a'', a''', \dots$ has characteristic 0 and therefore can

be added any number of times to any path without changing its characteristic. Then he stated (without proof) that two contours having the same characteristic can always be transformed into one another without crossing the points $a, a', a'', a''', \dots$, and conversely, if under the same condition two contours can be deformed into one another they have the same characteristic. Thus, contours with different characteristics cannot be reduced to one another by deformation. We agree with Markushevich's comment that "in this way Puiseux established a purely topological (homotopic) invariant for closed curves in the plane" (1996, 196).

These results allowed Puiseux to reformulate his "fundamental proposition" by stating that when two closed paths passing through c have the same characteristic then the function u_1 will take the same value when z will return to c having traversed either path. By resorting to the concept of characteristic of a path Puiseux was also able to reformulate his results about the values of the functions $u_1, u_2, \dots, u_m$ having initial values $b_1, b_2, \dots, b_m$ at c when z starting from c describes any closed path, or reaches a point k following two distinct paths.

So far, Puiseux had excluded the case when any of the points $a, a', a'', a''', \dots$ are points where the function becomes infinite. He now observed that the study of this case could easily be reduced to the previous one by showing that, e.g. the irreducible equation

$$Nv^m + Pv^{m-1} + Qv^{m-2} + \dots + Sv + T = 0,$$

where $N, P, Q, \dots, T$ are polynomials in z , could be reduced to the monic equation

$$u^m + Pu^{m-1} + NQu^{m-2} + \dots + N^{m-2}Su + N^{m-1}T = 0$$

by the substitution $v = u/N$. The same reasoning as above applies to this equation, for the functions $u_1, u_2, \dots, u_m$ satisfying it distribute themselves with respect to a point a in a certain number of circular systems, and the corresponding functions

$$v_1 = u_1/N, v_2 = u_2/N, \dots, v_m = u_m/N$$

will also distribute themselves in the same number of corresponding circular systems. Suppose that $N = (z - a)^v \tilde{N}$ then $v_n = u_n / (z - a)^v \tilde{N}$. If u_n belongs to a circular system of order μ , it can be expanded in (positive) powers of $(z - a)^{1/\mu}$, and correspondingly v_n can be expanded in a similar power series of $(z - a)^{1/\mu}$ which, however, may include (a finite number of) negative powers (Puiseux 1850, 426).

Next Puiseux (1850, 428) observed in passing that the results obtained so far could be applied to a transcendental equation $f(u, z) = 0$ provided that $f(u, z)$ and its derivatives with respect to u and z are single-valued, continuous functions of u and z , these conditions being "the only thing that our theory supposes".⁷⁷

As an application of the concepts introduced in the first two parts, in the concluding part of his memoir Puiseux took up the study of the integral $\int_c^k u_1 dz$

⁷⁷Perhaps because of this passage Manning (1974–1975, 371) claimed mistakenly that Puiseux possessed the concept of an essential singularity.

of an algebraic function. He began by showing how to evaluate the value of such an integral along a path cmk which avoids the critical points $a, a', a'', a''', \dots$ to an arbitrary degree of approximation. He did this by going back to the process of continuous extension of a path that he had illustrated in Part I. Accordingly, he divided the path cmk into a finite number of parts $cmc', c'm'c'', c''m''c''', \dots$, for each of which he expanded u_1 in power series as he had shown in Part I and eventually integrated it term-by-term. By repeating this for each part of the path, as a result of such a process the integral $\int_c^k u_1 dz$ along cmk was expressed as a finite sum of such series.

Then he used the concept of “elementary contours” $(\pm a), (\pm a'), (\pm a''), \dots$ around the points $a, a', a'', a''', \dots$ to define the concept of an “elementary integral” such as $A_{\pm n}^k = \int u_n dz$ taken along the elementary contour $(\pm a)^k$. In this way, the value of $\int u_1 dz$ along a closed path from c could be expressed as the sum of a certain number of elementary integrals. As a particularly remarkable example Puiseux considered the case of a contour Δ passing through c and including all the points $a, a', a'', a''', \dots$. Then the characteristic (Δ) of Δ taken in a positive direction is such that $(\Delta) = (+a)(+a')(+a''), \dots$. Supposing that some of the functions $u_1, u_2, \dots, u_m$ resume their initial value after a complete turn of z along Δ , he let u_k be one of them. If after traversing the paths with characteristics $(+a), (+a)(+a'), (+a)(+a')(+a''), \dots$ the function u_k takes the initial values of, resp. $u_{k'}, u_{k''}, u_{k'''} \dots$, then

$$\int_{\Delta} u_k dz = A_k + A_{k'}' + A_{k''}'' + \dots$$

but also, more significantly,

$$A_k + A_{k'}' + A_{k''}'' + \dots = 2\pi i \lambda_k,$$

where λ_k is the residue of u_k with respect to the point at infinity, as he proved by considering a circle centred at the origin that included all the points $a, a', a'', a''', \dots$, into which Δ may be suitably deformed, and then by making the transformation $z \rightarrow 1/z'$.

Next Puiseux introduced the concept of a period p of the integral $\int_c^k u_1 dz$ defined as the sum of a suitable number of elementary integrals in such a way that p is independent of c and on adding an integer multiple of p to a value of $\int_c^k u_1 dz$ one reobtains a value of the integral. This done, he tackled the problems

1. Find all the distinct periods belonging to a value of $\int_c^k u_1 dz$;
2. Discover if each period belongs to all the values of the integral above or to some of them only;
3. Determine the distinct values of the integral up to integer multiples of the periods (1850, 439).

He succeeded in solving these problems in particular cases such as: when u is a rational function (already treated by Cauchy in 1846); hyperelliptic integrals, where he rediscovered Jacobi's (1835) results on quadruply periodic functions

of two variables (see Chap. 4); and when u is defined by the binomial equation $\Phi(z)u^m - H^m = 0$, Φ and H being polynomials with no common factor.

In his concluding remarks Puiseux prudently felt it necessary to recognise explicitly what he owed to Cauchy. He openly admitted (1850, 479) that the “true idea” of the integrals between imaginary limits belong to Cauchy who had introduced it “a long time ago” in (1825a). He modestly called his own theorems corollaries of Cauchy’s integral theorem, which followed from the theorems stated in (Cauchy (1846b, 72) and (1846i, 156–157) although—he proudly added—they “acquire a more precise meaning” than Cauchy’s once one knows whether the function takes its starting value or not after the independent variable has gone on a circle around its starting point, according to what he had proved in Part II of his memoir. He also observed that his theorems on the distribution of the functions $u_1, u_2, \dots, u_p$ in circular systems could be obtained from a theorem on the substitutions of the roots of an algebraic equation as established in Cauchy (1815b) even though he added (1850, 479) that his own method actually allowed one to produce the sought-for distribution.

Even the “principle” that he had introduced in Part III, which reduces the integral along a path to an integral along only one path plus some “elementary” integrals, should be attributed to Cauchy, who alluded to it in (1846p, 194–195). However, Puiseux rightly emphasised that he had been able to “go farther” than Cauchy even with respect to Cauchy (1846i, 163) for he had succeeded in finding all the periods of the integral when u is given by a binomial equation. Finally, Puiseux was pleased to remark that his results were in agreement with the ones obtained by Hermite in 1844 (see Sect. 4.2.4).

In many respects, Puiseux’s memoir was a remarkable piece of work. It was the first time that someone other than Cauchy not only related his integral theorem of 1825 to his (1846) papers, but also—and more importantly—it actually showed how powerful a tool it was for investigating the properties of complex functions. In fact, Puiseux had been able to merge Cauchy’s rather confused and dispersed results into a coherent theory of algebraic functions and their integrals that provided a geometric setting which prefigured Riemann’s work (see Chap. 5).

3.6.2 Cauchy’s Geometrical Analysis

As Cauchy (1851a, 300) shows, Puiseux’s great memoir had already been (partly) published when, on January 20, 1851, Cauchy himself presented the first of a series of papers to the Académie that dealt with algebraic, and more generally imaginary, functions.⁷⁸ However, a reader of Puiseux (1850) would have been rather

⁷⁸At the same meeting of the Académie Puiseux presented a new memoir (1851) *Recherches sur les fonctions algébriques* (see *C. R. Acad. Sci. Paris* 32 (1851), 93) which Cauchy, Sturm, and Binet were charged to review, they being the members of the commission already charged to review a memoir on the same subject previously submitted in 1850 by Puiseux.

disappointed by Cauchy (1851a) for he would find nothing new in it. Worse, he would instead find a step backwards there with respect to Puiseux's achievements in dealing with multi-valued functions and their integrals.

Cauchy (1851a, 292) began by considering a function u defined by the (algebraic—Cauchy assumed U was continuous) equation $U(u, z) = 0$. He rephrased (without proof) Puiseux's results on the behaviour of a branch u_g along a path according to whether it includes or not critical points $C', C'', C''', \dots$. Then, in order to avoid multi-valuedness Cauchy considered the straight lines $C'D', C''D'', C'''D''', \dots$ as the indefinite extensions of the “radius vectors” (*rayons vecteurs*) joining a fixed point O to the points $C', C'', C''', \dots$, the *points d'arrêt* in his terminology whereas the straight lines $C'D', C''D'', C'''D''', \dots$ were called by him “*lignes d'arrêt*” that “can be compared to obstacles at which the moving point z stops, without ever crossing them” (Cauchy 1851a, 294). As Brill and Noether (1894, 191) have rightly emphasised, it is worth noting here that neither Cauchy's *lignes d'arrêt* nor Hermite's *coupures* or cuts (see Sect. 6.7.6) can in any way be considered as an “equivalent substitute” for the concept of *Querschnitt* of a Riemann surface which we discuss below (see Sect. 5.2.1).

In the dissected plane thus obtained Cauchy reformulated some of the relevant achievements of Puiseux by considering the functions $u_1, u_2, u_3, \dots$ as continuous functions of z , and by applying the results stated in his own (1846i, o, p) to the evaluation of the values of the (rectilinear) integral $\int u dz$ and of the (curvilinear) integral $S = \int u \frac{\partial z}{\partial s} ds$ taken along a curve PQR which may or may not cross a *ligne d'arrêt*. In the first case $\int_{PQR} u \frac{\partial z}{\partial s} ds$ equals the difference of the rectilinear integrals $\int_{\bar{O}R} u dz - \int_{\bar{O}P} u dz$. In the second case when the curve PQR crosses the *lignes d'arrêt* $C'D', C''D'', C'''D''', \dots, C^{(m)}D^{(m)}$ at points $Q', Q'', Q''', \dots, Q^{(m)}$ the curve PQR will divide into parts $PQ', Q'Q'', Q''Q''', \dots, Q^{(m)}R$.

When z travels along them by moving from P to R along the curve PQR , the function u will coincide with some $u_g, u_h, \dots, u_n$ out of $u_1, u_2, u_3, \dots$, and the integral S will be given by the sum of the values of the relevant rectilinear integrals $\int u_g, \int u_h, \dots, \int u_n$ taken along the chords of $PQ', Q'Q'', Q''Q''', \dots, Q^{(m)}R$. Eventually he considered the case when P coincides with R , and the function u takes the same value after a complete turn of z on the curve. Then the value of the integral S along such a closed curve is increased by the sum of the values of rectilinear integrals taken along the straight lines joining the *points d'arrêt* to each other if $(z - c')u, (z - c'')u, (z - c''')u, \dots$ vanish for $z = c', z = c'', z = c''' \dots$, and by the residues of u at the *points d'arrêt* where the last condition is not satisfied.

On February 3, 1851 Cauchy presented to the Académie the continuation of this paper. Only the statement of the main theorem on the number of the periods of the integral $\int u dz$ is recorded in the *Comptes rendus* (Cauchy 1851b). But, whatever be the meaning of Cauchy's sketchy explanations, that number “cannot be made in any way to coincide with Riemann's” (Brill and Noether 1894, 191). One week later Cauchy presented a further addition (1851c) to his previous papers where he briefly sketched the case when u is determined by a system of n algebraic equations

$$U(u, v, w, \dots, z) = V(u, v, w, \dots, z) = W(u, v, w, \dots, z) = 0.$$

At the very same meeting Cauchy presented another note (1851d) which turned out to be the first paper in which “guided by the analogy” with the real case he was able to introduce the general concept of an imaginary function $w = u + iv$ of an imaginary variable $z = x + iy$ that he had failed to give in [Cauchy \(1821a\)](#). By extending to imaginary variables the definitions given for real variables, “as one has naturally to do”, w “will be thought of as a function of z when the value of z will determine the value of w ”. ([Cauchy 1851d](#), 302). To this end, Cauchy added, “it will be enough that u and v be determined functions of x, y ”. Having introduced the geometrical interpretation of z in the plane, he observed that the derivative dw/dz , w being “any function” of z , will in general depend not only on z but also on dy/dx or, in geometrical terms, on the tangent to the curve described by the moving point z . The derivative will be independent of the direction of the moving point z “in the particular case” when

$$\frac{\partial u}{\partial x} = \frac{\partial v}{\partial y}; \quad \frac{\partial u}{\partial y} = -\frac{\partial v}{\partial x},$$

from which $\Delta u = \Delta v = 0$ follow at once. This will “ordinarily” happen when w and z are related by an equation $f(w, z) = 0$, where $f(w, z)$ is always continuous with respect to w and z (a statement which yet again hints at the decisive role that Cauchy kept attributing to continuity). In his concluding remarks [Cauchy \(1851d](#), 304) was pleased to state that the principles just established confirmed what he had said elsewhere (1844g, 368–369) about the need to mention the derivative in the statement of his (1831b) Theorem I.

Once he had defined the concept of function that way, Cauchy began the very next week to present a series of papers to the Académie where he reformulated his theory of residues and related results in the new context. Thus, in [Cauchy \(1851e\)](#) he referred to his 1827 papers in the *Exercices* as well as to his (1831c,d) to re-state the main formula as

$$(S) = \int f(z)dz = 2\pi i \mathcal{E}(f(z)),$$

where the integral is extended to the closed contour of a plane domain S , $f(z)$ is a function satisfying the conditions stated above, and $\mathcal{E}$ denotes the sum of its residues at the points of infinity of $f(z)$ lying within S . If $f(z)$ is a transcendental function with infinitely many points of infinity at values z_k , some of them lying within S , then the formula above will provide the sum of a certain number of terms of an infinite series. By suitably extending S to domains $S_1, S_2, \dots, S_n$ so as to include an increasing number of points z_k , under the addition hypothesis that the corresponding curvilinear integrals (S_n) converge to a fixed limit for $n \rightarrow \infty$, the formula above will then provide the sum of a convergent series. Cauchy applied this result to particular cases. For instance, if S is a disk with radius R centred at the origin, the corresponding formula for the residues provides an expression for the isotropic mean of $zf(z)$. By considering the half disk in the upper half-plane instead of the disk he obtained formulae which could be applied to the evaluation of such integrals as $\int_{-\infty}^{\infty} f(x)dx$ as Euler and Laplace had obtained. Finally, he considered an annulus centred at the origin, and by a suitable choice of $f(z)$ he obtained some

formulae (1851e, 312–313) which could be applied to the decomposition of the elliptic functions in simple factors.

This was the subject of the paper (1851f) that he presented to the Académie the next week. There he considered an annulus centred at the origin with radii r_0 and R , and a function $\varphi(z)$ continuous in the annulus except at points of infinity z_k . He re-established the formulae obtained in (1851e) for a suitable choice of $\varphi(z)$, and eventually applied them to $\varphi(z) = \sinam z$. He referred to (1846o) to make it clear that $\sinam z$ had to be thought of as the “complete” solution of the differential equation $ds = t dz$ subject to the conditions that s and t

1. Are continuous with respect to z ;
2. Satisfy the equation $t^2 = (1 - s^2)(1 - k^2 s^2)$ with $0 \leq k \leq 1$;
3. $s(0) = 0, t(0) = 1$.

Calling $K = 2 \int_0^1 \frac{dx}{\sqrt{(1-x^2)(1-k^2x^2)}}$ and $K' = 2 \int_0^{1/k} \frac{dx}{\sqrt{(x^2-1)(1-k^2x^2)}}$ he obtained

$$\sinam z = z \frac{\prod(1 - \frac{z}{nK + in'K'})}{\prod(1 - \frac{z}{nK + i(n'+1/2)K'})}$$

n, n' being integers. There was nothing new in this paper, and Cauchy drew no new conclusions from it for any further development of the theory of elliptic functions, but apparently he was happy to give it as an indication of the power of his approach to elliptic functions via his residue calculus. He limited himself to stating that similar expressions could be obtained for the other elliptic functions, and concluded his note by promising to resume the subject in future memoirs.⁷⁹

At the very same meeting of the Académie, on February 25, 1851, he presented his report of Puiseux’s 1850 memoir (Cauchy 1851g). It soon became clear that Puiseux’s prudent modesty had not, after all, protected him from Cauchy’s self-promotion. Cauchy missed no opportunity to summarise his own related results in some detail, beginning with an explicit reference to his 1825 integral theorem. In this connection he pointed out (1851g, 329) that the condition that the contour of integration lay within a rectangle was not necessary, as he had shown in (1846b, 72). He went on by quoting a long passage from his (1846g, 142–143) to claim that Puiseux’s “elementary” integrals were nothing other than singular integrals of the first order, as he had explained in the continuation of (1846g) not included in the *Comptes rendus* of the Académie (and never published). He also quoted a long passage⁸⁰ from his own (1846i, 162–163) to claim that the theorem stated there “actually allowed one to reduce the determination of the indices of periodicity to the evaluation of certain singular integrals” (Cauchy 1851g, 338). Having reminded his readers that Puiseux had shown how the integral $\int u dz$ can be reduced to the sum of

⁷⁹Apparently he did so in a series of notes, but for most of them only a short summary is recorded in the *Comptes rendus* of the Académie (see *O.C.* 1 (11) 350–354; 384–385).

⁸⁰In fact, Cauchy slightly modified to fit it to Puiseux’s context, even adopting the latter’s notation.

“elementary” integrals and had set himself the task of determining the values of the integral and the indices of periodicity in particular cases, Cauchy went on to claim that he had already treated the case of a rational function himself in his 1846 papers, where he had also shown “the route one had to follow”. Thus, Cauchy concluded, “Puisseux has taken up the question at the point where the publications already made [by Cauchy] had left it” (1851g, 333). He went on to summarise Puisseux’s results in a couple of pages (this in a 10-page report!) and concluded that the memoir was worth publishing in the *Recueil des savants étrangers* because of Puisseux’s “new developments and improvements of the theory of curvilinear integrals” and his “sagacity” in showing how functions of a circular system are permuted among themselves. Publication in the same *Recueil* was the recommendation with which Cauchy one month later concluded his two-page report (1851n) on Puisseux’s new researches recently submitted to the Académie.⁸¹

Actually, (Puisseux 1851) was a continuation and a completion of his (1850), following Cauchy’s steps. Puisseux began by proving that a single-valued algebraic function is necessarily rational (1851, 229–233), then he proved that in the case of an irreducible equation $f(u, z) = 0$ which defines functions $u_1, u_2, \dots, u_m$ of z , given any two of them, u_1 and u_n say, one can always, starting from an arbitrary point c , find a closed path such that after a complete turn of z on the path u_1 will assume the initial value of u_n (1851, 233–235). Given this, he was able to prove the main theorem of the note: if $f(u, z) = 0$ is irreducible, and v_1 is the value of the integral $\int_c^k u_1 dz$ along any path cmk and p is the value of $\int_c^k u_n dz$ along a closed path such that u_n takes the same value after a complete turn of z on it, then p is a period and by adding to v_1 integer multiples of all the periods one will still obtain a value of the integral $\int_c^k u_1 dz$. This theorem answered the second of the problems he had set himself in his (1850, 439). At the same time it provided him with a useful criterion for deciding whether an algebraic equation is irreducible or not. This completed his study of the branches of an algebraic function. To sum up, we can say with Markushevich (1996, 198) that Puisseux’s papers “gave a model for the study of multi-valued functions using the methods created by Cauchy”.

In the meantime, on March 31, 1851 Cauchy had finally reported on Hermite’s 1849 memoir.⁸² As was customary with Cauchy, his report (1851h) emphasised the role of his own results, including in particular his theory of residues, and concluded by recommending Hermite’s memoir for publication in the same *Recueil des savants étrangers* as he had for Puisseux’s (needless to say, it was never published there). Cauchy’s report gave rise to an argument with Liouville who remarked that “he himself had found a general theory of doubly periodic functions a long time ago” (*C. R. Acad. Sci. Paris* 32, 450–452) as witnessed by his communication to the

⁸¹As was usual with this kind of recommendation, the memoirs were never published in that *Recueil*. However, Cauchy’s reports provide evidence that the memoirs submitted by Puisseux to the Académie reproduced essentially the same content as his (1850) and (1851) memoirs, if they were not indeed simply the very same papers.

⁸²We discuss this memoir below, see Sect. 4.2.4.

Académie on December 9, 1844. In his turn, Cauchy followed Liouville's remark with a note (1851i) where he claimed priority over Liouville by referring to his papers (1843d,e,f), and even to the older (1826a,b) and (1831c,d), and concluded by giving an elementary proof that the number of zeros of doubly periodic function within the parallelogram of the periods equals the number of infinities there, a proof that he claimed to have shown to Hermite who in turn said he had already found it (1851i, 376).⁸³

After this incident Cauchy limited himself to presenting to the Académie a short note (1851m) where he introduced the term “monogenic” to denote functions satisfying the Cauchy–Riemann equations, as stated in his previous (1851d). On the same occasion he also introduced the term “monotypic”, which in early 1852 he changed into “monodromic”, to denote continuous, single-valued functions. As he explained e.g. in [Cauchy \(1852, 407\)](#) a function is monogenic in the neighbourhood of a point z when its derivative is monodromic there. [Cauchy \(1852\)](#) is interesting in itself, for there he tackled anew the problem of expanding an implicit function in power series in the neighbourhood of a point z_0 . More precisely, he considered n functions

$$\mathcal{Z}(z, u, v, w, \dots), \mathcal{U}(z, u, v, w, \dots), \mathcal{V}(z, u, v, w, \dots), \mathcal{W}(z, u, v, w, \dots), \dots,$$

which are finite, monodromic, and monogenic in a suitable neighbourhood of the point with coordinates $(z_0, u_0, v_0, w_0, \dots)$. He further required that $u, v, w, \dots$ satisfy the differential system

$$\frac{dz}{\mathcal{Z}} = \frac{du}{\mathcal{U}} = \frac{dv}{\mathcal{V}} = \frac{dw}{\mathcal{W}} = \dots$$

with initial conditions $u(z_0) = u_0, v(z_0) = v_0, w(z_0) = w_0, \dots$

According to the theorems stated in his (1835), if $\mathcal{Z}(z_0, u_0, v_0, w_0, \dots) \neq 0$ then the general integrals $u, v, w, \dots$ of the system can be expanded in convergent power series of $z - z_0$ in a suitable neighbourhood of z_0 . As the terms of these series are finite, monodromic, and monogenic (or, shortly, synectic as he proposed to denote all those properties with a new term) functions of z , the same holds true for the functions $u, v, w, \dots$. Thus, given a synectic function $\Omega(u, v, w, \dots)$, after substituting for the variables $(u, v, w, \dots)$ the integrals $u, v, w, \dots$ above, the function Ω thus obtained will still be a synectic function of z in the neighbourhood $|z - z_0| < \lambda$, where λ is the least of the moduli for which the corresponding value of z satisfies either the characteristic equation $\mathcal{Z} = 0$ or $\frac{1}{\Omega} = 0$, and therefore it can be expanded in convergent power series of $z - z_0$ there. This enabled him to solve a system of n synectic equations $U = 0, V = 0, W = 0, \dots$, where $U, V, W, \dots$ are synectic functions of z satisfying given initial conditions as above, by reducing it to the system of differential equations

$$dU = 0, dV = 0, dW = 0, \dots,$$

⁸³Liouville's theory of elliptic functions and Cauchy's claims are discussed in detail in Sect. 4.2.4.

satisfying the same initial conditions. In particular, if $\Omega(z)$ is defined by a system of synectic equations, and satisfies the initial condition $\Omega(z_0) = \Omega_0$, then $\Omega(z)$ is a synectic function and can be expanded in a power series of $z - z_0$ which is convergent for $|z - z_0| < \lambda$, where λ is the least of the moduli for which the corresponding value of z satisfies either $\Omega = 1/0$ or $\frac{d\Omega}{dz} = 1/0$. As a consequence, Cauchy concluded, “one can always consider a synectic function as an entire function of z built up with a finite or infinite number of terms” (1852, 415).

3.6.3 Cauchy, Briot, and Bouquet

In this period Cauchy’s interests had mainly turned to subjects related to the application of series to astronomy, but occasionally he returned to complex analysis by reacting to publications by others. He did so in March 1852 when he reported on a new memoir by Chiò on Lagrange series, and he followed his report⁸⁴ with three notes on the same subject presented to the Académie within a week (see *O.C.* (1) 11, 415–437). He did so again after the publication of Björling (1852) to whose work Cauchy referred in a series of memoirs on the logarithm and the exponential of geometric quantities published in the *Exercices* (see *O.C.* 2 (14), 283–305), which were in turn followed by some memoirs on the trigonometric functions of geometric quantities (see *O.C.* 2 (14), 307–357) that he wrote during 1853. And he did so again in March 1853 when he reacted to a couple of notes submitted to the Académie by Briot and Bouquet that he, Liouville, and Binet, had been charged with reviewing.⁸⁵

The first of these (Briot and Bouquet 1853a) was devoted to the study of power series of imaginary variables. Following Cauchy’s steps, they proved that such a series is convergent within what they named its “convergence circle” and defined a continuous, monogenic function there. In their second note Briot and Bouquet (1853b) began by claiming that the “lovely” integral representation theorem of Cauchy’s Turin *Mémoire* on the expansion of functions in power series was still somehow “wrapped in clouds”. After restating it so as to require the continuity of the derivative, they mentioned Lamarle’s (1846) criticism as well as Wantzel’s (1847) claim that when the function is continuous the same holds true for its

⁸⁴As he had done in 1846, Cauchy recommended Chiò’s memoir for publication in the *Recueil des savants étrangers*. Eventually it appeared as Chiò (1854).

⁸⁵Charles Auguste Briot graduated from the École Normale Supérieure at the head of his class in 1838 and held a succession of teaching positions before becoming a professor at the Sorbonne and the École Normale Supérieure in 1864. There he concentrated mostly on thermodynamics and rational mechanics, but he also teamed up with Jean-Claude Bouquet, an old friend from his school days, who by 1874 was also a professor at the Sorbonne and they wrote a series of successful textbooks on several elementary topics. They were highly regarded in their day as teachers: Briot was awarded the Prix Poncelet of the Académie des sciences in 1882 and Bouquet was particularly praised by Jules Tannery and by Hermite, with whom he shared political and religious views.

derivative. Then Briot and Bouquet stated that a function $w = f(z)$ can be expanded in convergent power series of z in a disk centred at the origin with radius R provided that $f(z)$ is finite and monodromic there and “has a unique derivative at every point whatever be the direction of the displacement”. They added that these conditions are necessary and also pointed out that the last condition could be “very easily” interpreted geometrically as the function $f(z)$ providing a conformal representation of the disc in the z -plane on the w -plane.

Thus, Briot and Bouquet eventually formulated the correct hypothesis one has to require for Cauchy’s theorem, i.e. the existence of a unique derivative $f'(z)$ (independent of the path). When Goursat (1900) proved Cauchy’s integral theorem under this hypothesis he stated that “by assuming Cauchy’s point of view it is sufficient to build the theory of analytic functions to suppose the continuity of $f(z)$ and the existence of the derivative” (1900, 16)—this topic is discussed in more detail below, see Sect. 8.5.4.

Cauchy took the opportunity provided by Briot and Bouquet’s first note (1853a) to return to his old “theorem” that the sum of a convergent series of continuous functions is a continuous function as stated in his *Cours d’analyse* (1821a, 120). As Briot and Bouquet (1853a) had observed, he said in Cauchy (1853a, 31), such a theorem is true for power series, “but for other series it could not be admitted without restrictions”.⁸⁶ Indeed, commenting on it as early as 1826b Abel (1826b, 225) had already observed that Cauchy’s theorem seemed to admit “exceptions”, and as an example he gave the series

$$\sin x - \frac{1}{2} \sin 2x + \frac{1}{3} \sin 3x - \dots$$

whose sum is discontinuous at values $(2m+1)\pi$, m being an integer. Cauchy did not reply at that time, and even in Cauchy (1853a) he avoided mentioning Abel although he gave the similar example

$$\sin x + \frac{1}{2} \sin 2x + \frac{1}{3} \sin 3x + \dots$$

to show that the statement of his theorem needed “restrictions”. However, he added, the required condition could be “easily” stated “in a few words” (Cauchy 1853a, 31–32).

To this end he referred to his old definition [Def 2] of continuity of a function as given in (1821a, 43), but (tacitly) reformulated it in a more precise manner by making it clear that $f(x)$ is continuous within given limits if for every x within those limits

⁸⁶Cauchy’s theorem has raised many controversial interpretations among the historians of mathematics. We do not need to enter such a discussion here and limit ourself to remarking that there is hardly any doubt that by 1853 Cauchy had changed his mind about his theorem since 1821. Why else would he have admitted the need of “restrictions” to make the theorem correct?

1. $f(x)$ is finite and single-valued, and
2. An infinitely small increment of x *always* produces an infinitely small increment of $f(x)$. [Our emphasis]

Accordingly, he considered a series $\sum u_n(x)$ of continuous functions within given limits. If for a suitably large n , for any $n' > n$ and all x in the interval it is the case that $|u_n(x) + u_{n+1}(x) + \cdots + u_{n'}(x)| < \varepsilon$ for an arbitrarily small ε , then the series is convergent and its sum is a continuous function of x within the given limits. This was the required condition, which is not satisfied by the series given by his example, for its sum vanishes at 0 but it equals $\pi/2$ for $x > 0$ “very close” to 0.

Next he turned to the complex case by taking x to be an imaginary variable z that he interpreted in geometrical terms. According to the definition given in (1851d, 302), he considered a function of z , and extended his definition of continuity for functions of real variables to it by stating that $f(z)$ is continuous within a given domain if $f(z)$ is finite and single-valued for every point there, and in the neighbourhood of each point an infinitesimal increment of z always produces an infinitesimal increment of $f(z)$. Then he restated his theorem for series $\sum u_n(z)$ of continuous functions, adding that when $u_n(z)$ has a unique derivative $u'_n(z)$ for any n then $\sum u'_n(z)$ is in turn a convergent series whose sum $s'(z)$ is the derivative of $s(z) = \sum u_n(z)$. He concluded the note by explaining once more the meaning of the terms monodromic, monogenic and synectic that he had already introduced in recent papers.

Cauchy took up the matter again in a series of papers that appeared in the *Exercices* on November 7, 1853 (see *O.C.* (2) 14, 359–416). As he had claimed in (1851d, 301), he repeated in (1853b, 359) that one could extend the definitions given for functions of algebraic (real) variables to functions of geometric quantities “by letting oneself be guided by analogy”. Consequently, in order that $Z = X + iY$ be a function of $z = x + iy$ “it will be enough that X and Y are determined functions of x and y ” (1853b, 360). But in spite of Cauchy’s claim the truth is rather as [Casorati \(1868a, 71\)](#) later remarked: a generalisation of this type was not suggested by a truly profound “analogy”. Analogy could merely lead to a theory of functions of a complex variable that was a theory of functions of two real variables. “It is thus that Cauchy himself, while conserving the given definition of a function of a complex variable $x + iy$, found it necessary to introduce an epithet [monogenic] to designate, among all the functions comprised in the definition, only those that, like all the functions resulting from ordinary systems of operations performed on the combination $x + iy$, enjoy the property of having for every value of $x + iy$ a derivative independent of the value $\frac{dy}{dx}$ ” ([Casorati 1868a, 71](#)).

The paper ([Cauchy 1853b](#)) was followed by a long memoir (1853c) devoted to the concept of continuity of functions of geometric quantities. He began by recognising that the relevant definitions given in [Cauchy \(1821a\)](#) were “there applied particularly to real or imaginary functions of *real variables*” (1853c, 367) [our emphasis]. But, he added, nothing stops the same definitions from being extended to the case where the variables are geometric quantities. After quoting long passages from his *Cours* (1821a), he made the sought-for extension by merely

translating into geometrical terms both the definitions [Def 1] and [Def 2] that he had given there (1853c, 376). In order to preserve single-valuedness in the case of multi-valued functions like $z^{1/2}$ or $\log z$ he made it clear that “it was easier” to consider each branch (type, in Cauchy’s terminology) of the function as a separate, well-determined function of the variable by dissecting the plane with suitable *lignes d’arrêt* (the negative, real semi-axis in the examples above). Interestingly enough, in this connection Cauchy quoted with emphasis the claim he had made in (1846a, 46) when answering Lamarle about the “marked influence” that the nature of conventions has on the continuity of functions. Then he went on to repeat in greater detail what he had said in (1851a), illustrated with the examples above. (The subsequent memoirs of the *Exercices* devoted to the differentiation of geometric quantities and their functions are merely (expanded) repetitions of what he had already done in various notes of the *Comptes rendus*.)

3.6.4 Last Period of Creative Work

Cauchy experienced the last period of intense work in his life in early 1855, when starting on February 12 he presented the Académie with a weekly series of notes culminating with his last important achievement, the concept of the logarithmic indicator (*compteur logarithmique*). Cauchy’s notes (*O.C.* (1) 12, 204–243) dealt mainly with the integration of a system of differential equations, and there was nothing essentially new there with respect to his previous papers, from his (1835) to (1852), which he repeatedly quoted.

Among them, however, [Cauchy \(1855a\)](#) is worth mentioning for in it he praised Weierstrass’s work on Abelian function as presented in [Weierstrass \(1854\)](#), the French translation of which had just appeared in Liouville’s *Journal*. In his paper Cauchy dealt with the integral y/x of the equation $\frac{dy}{dx} = \frac{dy}{dt} / \frac{dx}{dt}$, x and y being functions of t satisfying the initial conditions $x(\tau) = \xi$, $y(\tau) = \eta$ for $t = \tau$. He first observed that when $\xi = \eta = 0$, the value of $\frac{dy}{dx}$ will be the limit of y/x for $t \rightarrow \tau$. Then he considered z as a monodromic and monogenic function $z(t)$. Therefore, so are $\frac{dz}{dt} = \frac{d \log z}{dt}$ and also $\frac{d^n \log z}{dt^n}$ (n being any integer). If the latter decomposes into the sum of two monodromic and monogenic functions u and v such that $\frac{d^n \log z}{dt^n} = u + v$, then this equation could be satisfied by putting $z = y/x$, and $\frac{d^n \log y}{dt^n} = u$, $\frac{d^n \log x}{dt^n} = v$, where x and y are synectic functions of t when u and v have a finite value (the first one when $z = 1/0$ and the second one when $z = 0$). As a particular application he considered the equation $\frac{dz}{dt} = Z$ where $Z = \sqrt{(1 - z^2)(1 - k^2 z^2)}$. Accordingly, $\frac{d^2 \log z}{dt^2} = -\frac{1}{z^2} + k^2 z^2$ and $\frac{d^2 \log y}{dt^2} = -\frac{1}{z^2}$, $\frac{d^2 \log x}{dt^2} = -k^2 z^2$ are such that the integral $z(t)$ of the equation above will be given by the ratio y/x . If $z(0) = 0$ one gets the elliptic function $z = \sin am t$, one of whose “nicest properties” is expressed by the last two differential equations ([Cauchy 1855a](#), 235). At this point Cauchy was pleased to affirm that his conclusion was “precisely the same” as those of Weierstrass who, in his “lovely” paper (1854) had explained the “principle” of the decomposition of y/x

in two parts, and according to whose method $z = \sin at$ reduced to the form y/x where y and x satisfy the equations $\frac{d^2 \log y}{dt^2} = -\frac{x^2}{y^2}$; $\frac{d^2 \log x}{dt^2} = -k^2 \frac{y^2}{x^2}$.

“So far as I can judge”, [Cauchy \(1855a, 235\)](#) concluded, the “main difference” between his and Weierstrass’s methods was that they followed different routes to establish that the integrals y and x are synectic functions of t .

The trigger for these notes is likely to have been a memoir that Briot and Bouquet had submitted to the Académie and that Cauchy was charged to report on. He did so on March 12, 1855 ([Cauchy 1855b](#)). As Briot and Bouquet explicitly referred to his own work, Cauchy “felt obliged” to do the same. Then, to let the importance of their work be appreciated he reported in detail their example $t \frac{dx}{dt} = X$, where $x(t)$ vanishes at $t = 0$ and $X(x, t)$ is finite, monodromic and monogenic, and therefore expandable in power series around the origin $(0, 0)$ as $X = ax + bt + \varphi(x, t)$ where $a = \frac{\partial X}{\partial x}|_{x=0, t=0}$, $b = \frac{\partial X}{\partial t}|_{x=0, t=0}$ and $\varphi(x, t)$ is built up by terms of degree greater than 1 in x and t . In Cauchy’s view, as stated in (1855b, 252), this was the more original and important part of their work. Eventually he reported on their study of the general case $\frac{dx}{dt} = f(x, t)$ by pointing to the analogy with Puiseux’s work when $f(x, t)$ is the ratio of two finite, monodromic, and monogenic functions both vanishing at $(0, 0)$. Eventually, Cauchy was pleased to remark that Briot and Bouquet had recently added a note to their memoir dealing with the theorem he had stated in his (1852).

This report by Cauchy, although not very important in itself, is worth mentioning for it shows how pleased Cauchy was to find Briot and Bouquet, whose political and religious views he shared, responding to his research. They in turn acknowledged the Académie’s (and Cauchy’s) approval of their work in a subsequent memoir (1855) on the integration of differential equations by means of elliptic functions that they presented to the Académie on December 1855 and that Cauchy (jointly with Liouville and Binet) was charged to review—as he actually did in (1856b). Cauchy also supported the young Méray by presenting to the Académie on April 2, 1855 a paper in which Méray, inspired by Briot and Bouquet’s contemporary memoirs, tackled the problem of determining the differential equation that is necessarily satisfied by any doubly periodic, monodromic and monogenic function ([Méray 1855](#)).⁸⁷

Cauchy took the occasion of his report on a couple of (posthumous) memoirs that Laurent’s widow submitted to the Académie⁸⁸ to (partly) revise his unfair judgement of 1843. Indeed, after praising Laurent’s “deep science and great sagacity”, he eventually recognised that Laurent’s theorem was not a simple “extension” of a theorem of his own—as Laurent had modestly called it, and [Cauchy \(1843g\)](#) himself

⁸⁷Méray followed Cauchy’s courses at the Sorbonne, and later on in 1863 he re-edited Cauchy’s *Cours d’analyse* on behalf of Saint-Venant and Cauchy’s family (see Belhoste 1991, 293).

⁸⁸They were later to be published as [Laurent \(1863\)](#). This paper is interesting in many ways. Not only does it provide a detailed account of Laurent’s achievements in complex function theory, it also shows how Laurent applied complex functions to the study of the equilibrium distribution of heat.

had claimed—but instead “a new theorem worth noting” (Cauchy 1855c, 256). Better posthumously than never, one could say.

At the very next meeting of the Académie, on March 26, Cauchy presented a note (1855d) where under the new heading of the “integral variation” of a function Z , defined as the difference $Z_P - Z_Q$ of the values that $Z(z)$ takes at points P and Q along a path joining them without crossing any singular point, he re-stated some of his old results, including the integral theorem and the residue theorem. The interest of this note lies in its showing the route Cauchy that followed to introduce the concept of the logarithmic indicator. He considered $\log Z$ and supposed $c, c', c'', c''', \dots$ to be the values of z for which $\log Z$ becomes infinite (i.e. for which $Z = 0$ or $1/Z = 0$). If a (simply connected) domain S contains none of those points, then the “integral variation” (S) of $\log Z$ along the boundary of S is 0, otherwise it equals $2\pi ki$, k being a whole number. To make this more precise, Cauchy observed that $2\pi i$ is the “integral variation” of $\log z$ along a circle centred at the origin. Accordingly, he stated (Theorem 1) that when Z is finite, monodromic, and monogenic at every point of S (included the contour, where $Z \neq 0$), then the number of zeros of Z inside S is given by $k = (S)/2\pi i$. If (Theorem 2) Z is monodromic and monogenic inside S , and it becomes neither infinite nor zero on the contour, then k gives the difference between the number of zeros and the number of infinities of Z inside S . As an application, he obtained a new proof of the fundamental theorem of algebra.

Cauchy reformulated Theorems 1 and 2 some weeks later, on April 30, 1855. He did this in a note (1855e) presented to Académie in which he introduced the idea of logarithmic indicator wrapped up in his very clumsy notation of “indices” that he tacitly took from his (1831e) (curiously enough, without mentioning it, although he made amends in his next paper (1855f)). Cauchy’s result can be made clearer by considering the integral

$$(S) = \frac{1}{2\pi i} \int_{\partial S} \frac{Z'}{Z} dz$$

taken over the boundary of a (simply connected) domain S . Accordingly, he re-stated his Theorems 1 and 2. As an immediate consequence (which however Cauchy did not mention explicitly) one has the “principle of the argument” according to which when z describes the contour of S the argument of $Z(z)$ increases by $2\pi(N - P)$, where N is the number of zeros and P the number of infinities of Z inside S . Instead, he re-obtained once again a proof of the fundamental theorem of algebra as well as a proof of Liouville’s theorem that an elliptic function has as many zeros as points of infinity within the parallelogram of the periods. Then he applied his formula to the evaluation of the number of roots of an algebraic or transcendental equation satisfying given conditions, thus rederiving Sturm’s results (Cauchy 1855f). He took up the subject again almost two years later in (1857a, b) in works which were echoed in contemporary researches by Rouché (1859) and, later on, in (1861) and (1862).

In 1861 Rouché wrote a long memoir on Lagrange series that he submitted to the Académie before publishing it in the *Journal* of the École Polytechnique. There Rouché began by claiming that in spite of the works by Laplace, Jacobi, Cauchy, Chebyshev and others, giving a “complete proof of [Lagrange’s] formula is a rather complicated problem” (1861, 296; 1862, 193). According to the abstract of (1861)

he published in the *Comptes rendus*, after expounding some principles on imaginary functions “mainly due to Cauchy” he proved Lagrange’s formula using a couple of very simple theorems which he then applied to particular cases such as the solution of Kepler’s equation. He proudly stated that his results were more precise than the ones one could find in Cauchy’s papers, which were “presented in a more or less explicit manner in the middle of a great apparatus of formulae and complicated notation” (1861, 296; 1862, 194). What Rouché failed to emphasise in his abstract was perhaps the gem of his paper, a theorem that he stated in the concluding remarks and today is named after him :

“If the constant α is such that $|\alpha \frac{\varphi(z)}{f(z)}| < 1$, for $|r|$ less than the one for which either $\varphi(z)$ or $f(z)$ ceases to be finite, continuous, monodromic and monogenic, the equations

$$f(z) - \alpha\varphi(z) = 0 \quad \text{and} \quad f(z) = 0$$

have the same number of roots inside a circle centred at the origin with radius r ” (1862, 217–218). Rouché was right in saying that his results were to be found in some of Cauchy’s papers, hidden by complicated formulae and notation for, as we have seen above, this theorem was essentially given (and for more general domains than a circle) in the latter’s (1831e).

As for Cauchy, he had been able to apply the logarithmic indicator to subjects other than counting the zeros of an equation, as he showed in [Cauchy \(1856a\)](#) by providing a new proof of Puiseux’s (1850) “fundamental proposition”. This had turned out to be of increasing importance in analysis, as was shown by the memoirs of Briot and Bouquet. On July 14 Cauchy published a note ([Cauchy 1856c](#)) that provided a short, comprehensive summary of his results in complex function theory. Finally, Cauchy summarised the theory of residues in a paper (1857c) that he presented to the Académie only a few months before his death.⁸⁹

There he began by recalling the interesting “analogy” that had guided him in 1826 when he introduced the concept of a residue for the first time. As [Lagrange \(1797\)](#) had defined the derivative of a function $f(x)$ as the coefficient of ε in the expansion of $f(x + \varepsilon)$ in increasing power series of ε , so he had defined the residue of $f(x)$ as the coefficient of $1/\varepsilon$ in the expansion of $f(x + \varepsilon)$ in decreasing powers of ε . But, Cauchy observed, in both cases it was safer to avoid the recourse to series. As he had done in the *Résumé* (1823) for the derivative, now after “mature”

⁸⁹We note here that not only can we not confirm the story that the *Comptes rendus* adopted a policy of requiring articles to be no longer than four pages in order to keep itself from being swamped by Cauchy’s contributions, but that this article of eleven pages would seem to refute it. The story likely has its origins in [Bell \(1937, 287\)](#). The procès-verbaux of the French Académie on July 13, 1835 do record, however, that a limit of six to eight pages was to be imposed, which evidently Cauchy flouted. This may have been one of the reasons why Biot publicly criticised Cauchy in 1842 in the *Journal des savants* for flooding the *Comptes rendus* with badly written, verbose papers; see [Biot \(1842\)](#) and [Belhoste \(1991, 192\)](#). The situation today is that members of the Académie are limited to six pages, and non-members to four, but we cannot determine when this rule was introduced, although it does seem that it is being obeyed.

reflection he was eventually able to do the same for the residue by resorting to his memoirs (1825a) and (1831c,d), as well as his more recent papers on monogenic and monodromic functions.

With respect to the best way of introducing the residues he had already changed his mind in (1855g) where he had considered them “from a new point of view” by defining the residue of a function $f(z)$ that is monodromic and monogenic in the vicinity of a point of infinity c as the isotropic mean of the values $\zeta f(c + \zeta)$ for $|\zeta|$ very small. Now, in (1857c) he considered a function $Z(z)$ defined in a domain S , and stated that if Z is monodromic and monogenic everywhere on S , then $\int_{\partial S} Z dz = 0$, while if Z has isolated singularities (as he now called the points where Z was infinite) then $\int_{\partial S} Z dz = 2\pi i \mathcal{E}(Z(z))$. “We recognize here”, [Cauchy \(1857b, 437\)](#) was pleased to remark, “how useful it is to define clearly the functions of geometric quantities, or in other words, the functions of imaginary variables, by distinguishing not only monodromic and non-monodromic functions, but also monogenic and non-monogenic functions”. This achievement was the final culmination of efforts that had occupied him for more than 40 years.

3.6.5 Cauchy’s Final Months

Cauchy’s last year saw him active in creating the *Écoles d’Orient*. These were established in 1856 with a view to exploiting the opportunity to spread Christianity more openly in the Ottoman domains that had been produced by the peace treaty that ended the Crimean war. Or rather, and more precisely, the aim was to try to convert adherents of Orthodox Christianity to the Roman Church. Cauchy became a vice-President of the organisation, and the mathematicians Binet and Hermite also joined it. Hermite had followed Cauchy’s courses at the Sorbonne in 1849 and always considered him his “venerable master”. When Hermite nearly died of smallpox in 1856 he was converted to Catholicism by Cauchy. He thereafter remained a man of the political right. In the same year he was elected to the Académie, succeeding Binet. As noted in Belhoste (1991, 238) “Of all the charitable works that Cauchy created, the work of the *Écoles d’Orient*, which still exist today, was by far the most successful”. But the same year saw him on the unpopular and losing side in a dispute with Duhamel, and in May 1856 his old friend Binet died. Less than a year later Cauchy’s younger brother Alexandre died, and this death too profoundly saddened Cauchy. Then on 21 May 1857 an illness that had seemed to be weakening its hold on him flared up and Cauchy died early in the morning of 23 May 1857. Cauchy had already requested that his unpublished works be edited and published, and this task was undertaken first by the young Jesuit student Michel Jullien, then Méray, and finally by his biographer Valson, who prepared his *Oeuvres Complètes* for publication by the Académie des sciences in 1876. Gauss had died in 1855, and his collected works were also in process of being edited and published. A new generation had now to advance mathematics, but for the first time they would be accompanied by, and involved in producing, the works of their fathers.

3.7 Cauchy's Legacy

By dint of the instability and disorder that events stamped on his personality and thinking, the influence that he exerted on them [that is, on the mathematical sciences] will not be completely felt until sufficient time has passed to evaluate all the consequences [of his scientific work]. Young geometers who have the courage to read his works in detail and with care will find them to be a mine of ideas, with rich veins of discoveries and insights to follow through on and to bring up to date. (Briot, quoted in Belhoste 1991, 212).

Briot was right in his obituary of the great mathematician recently deceased. Perhaps circumstances prevented him from saying that the “mine of ideas” provided by Cauchy’s works was dispersed in a sea of papers, often hidden under clumsy notation, formulae and terminology, as Rouché later openly pointed out. But courage *was* needed to read Cauchy’s works in detail. Even his questionable style of publication, which had led him to publish and re-publish virtually the same paper (or a slight modification of it) many times could hardly encourage young geometers to take up the task.

As for our subject, from 1814 to the very end of his life Cauchy had produced an enormous amount of memoirs and notes, working hard and tirelessly and almost alone for a long time. Beginning with his (1825a) on integrals between imaginary limits Cauchy ventured into a new, unexplored domain, and it is his undeniable merit to have eventually created an entirely new field of mathematics. Certainly, his (1825a) tentatively proposed a geometric setting by referring to curves in the (real) (x,y) -plane to state his integral theorem, but in our opinion this does not mean that Cauchy had a clear idea of the complex plane as well as of complex paths in it, contrary to what Kline (1972, 637) claimed. In spite of his profound insights, at that time Cauchy had no clear idea of a theory of functions of a complex variable distinct from a theory of maps from $\mathbb{R}^2$ to $\mathbb{R}^2$. By missing this point most historians, unlike Cauchy, have overestimated the content of this memoir. Indeed, it is quite remarkable that Cauchy himself apparently failed to appreciate its novelty and its implications, which is one that he, contrary to his habit, failed to mention for decades.

Instead, in a way that reflects the original impetus for this work in 1814, Cauchy seems to have been more interested in providing extensive applications of his new calculus of residues to the evaluations of (improper) real integrals. He used geometric language merely instrumentally in later papers on residues, and his failure to think systematically in a geometric way may have prevented him from obtaining a deep understanding of the implications of his analytic formulae. Even in 1846, when for the first time he referred to his 1825 memoir and reformulated his integral theorem for more general curves than rectangles or circles he still adhered to his old, formal 1821 definition of imaginary variables and functions as “symbolic expressions”, and in 1847 he even tried to banish them from analysis with his theory of “algebraic equivalences” modulo $i^2 + 1$.

After 1846 Cauchy showed himself to be well acquainted with the basic facts about complex functions, even though a comprehensive theory of complex functions was still lacking. There is hardly any doubt that he had known of Argand’s work

since 1813, but it was not until 1849 that he adopted a geometric theory of complex functions. Even then he was unable to deal in a satisfactory way with branch-points and multi-valued complex functions and integrals. The idea of branching of algebraic functions can be recognised in Cauchy's papers, but his idea of dissecting the plane with suitable *lignes d'arrêt* in order to avoid the multi-valuedness of functions turned out to be a very poor device compared to the contemporary work of Puiseux. His lack of geometric insight may also explain his failure to contribute in any serious way to the theory of elliptic functions and Abelian integrals, which he treated only occasionally to test the power of his analytic results.

Briot was right to remind his readers of the "instability and disorder" of political events, for they exerted a marked influence of Cauchy's life and work. His decision to leave Paris following the 1830 revolution and to spend eight years abroad in self-exile prevented him from influencing the French mathematical milieu in the way that his works deserved. Working in complete isolation in Turin and Prague Cauchy produced some of his best achievements, including the calculus of limits (method of majorants) related to the study of Lagrange series and the integration of ordinary differential equations in the complex domain. Back in Paris, he spent some ten years helping the younger generation of French analysts become acquainted with his achievements. Having lost his teaching positions, he did this by profiting extensively from the *Comptes rendus* of the Académie, and the new *Exercices d'analyse et de physique mathématique*, which he used to publish, and repeatedly re-publish, his earlier results and some refinements of them. Even his unfounded claims of priority with respect to Laurent and Liouville can be seen as attempts at vindicating the importance of work that he felt was underestimated. In the same spirit he was pleased to recognise the responses to his work drew from some of the young mathematicians such as Hermite, Puiseux, Briot and Bouquet, Méray and even (although posthumously) Laurent.

The absence of the teaching duties that had forced him to produce his textbooks in the 1820s, when he was teaching at the École Polytechnique, may have prevented him from giving a systematic presentation of his courses on complex analysis at the Sorbonne in the 1850s. For all his profound insights, and the quality of his achievements, it is a matter of fact that his work in complex analysis lacked the clarity and even the rigour that characterised his accounts of real analysis. From this point of view, his equivocations over the right hypothesis required by his theorem on the expansion of functions in power series (the integral representation theorem of his 1831 Turin memoir), as well as his lasting emphasis on the role of continuity of functions instead of differentiability, show how long and hard Cauchy must have worked to discover the characteristic properties of functions of a complex variable that distinguish them from functions of two real variables. Eventually he succeeded in the last years of his life, when he created the theory of synectic (holomorphic) functions of a geometric (complex) variable. By that time he properly understood the role of the Cauchy–Riemann equations, and he was able to reformulate his main results, including the integral theorem and the residue theorem in this context. The tortuous path that Cauchy followed in creating analytic function theory and that we have described in detail in this chapter shows how unsatisfactory it would be from

an historical point of view to attribute to him in the 1820s results and concepts that he was able to elaborate only decades later.

For all the richness of his discoveries, Cauchy's legacy in complex analysis, therefore, was not easy to follow. He left it to younger mathematicians such as Briot and Bouquet to present his achievements in coherent form, and eventually to make his work the standard, and almost exclusive, reference of French analysts for decades to come.

Appendix. Russian Contributions

Cauchy's work attracted the attention of several Russian mathematicians following Ostrogradskii and Buniakovskii, among them Yulian V. Sokhotskii, who graduated from St. Petersburg university in 1866 at the age of 24, where he had Iosif I. Somov among his teachers. According to [Markushevich \(1996, 167\)](#), Somov was the author of original contributions to the theory of elliptic functions and their applications to mechanics. Somov also published the first book on elliptic functions in Russian (*Foundation of the theory of elliptic functions*, St. Petersburg 1850) which was largely based on Jacobi's *Fundamenta*. In their review of the book Ostrogradskii and Buniakovskii acknowledged that it was "the first complete, systematic work in Russian on one of the most remarkable and difficult areas of integral calculus" (quoted in [Markushevich 1996, 167](#)) even though, they added, it was flawed by inaccuracies in the proof of a number of theorems. Perhaps recalling the work he had done during his stay in Paris Ostrogradskii devoted a course of lectures to the theory of residues, Cauchy integral theorem and formula in 1858–1859.

Looking at the contribution of Russian mathematicians to complex function theory, Markushevich rightly emphasised the role of Lobachevskii's non-Euclidean geometry in the late development of the theory, particularly with respect to the works of Poincaré, Klein, and Koebe. Ironically, Ostrogradskii had expressed a very critical opinion of Lobachevskii's works on several occasions. For instance, when Lobachevskii's paper "On the principles of geometry" was submitted to the St. Petersburg Academy for publication in 1832 in the proceedings of the Academy Ostrogradskii, who was charged with reviewing the paper, stated that it was inaccurately written and largely incomprehensible, and consequently "for this reason not worth of the attention of the Academy" (quoted in [Betti 2005, 111](#)).

Like Ostrogradskii, Sokhotskii drew his inspiration for his research from the French mathematical tradition, and Cauchy's work in particular. He completed his masters thesis in 1866, but met with the opposition of Chebyshev, who apparently disliked the theory of residues and the theory of complex functions in general and believed it was not necessary to use them in analysis. Sokhotskii had to wait until in 1868 before his dissertation was eventually accepted by Chebyshev. (Some twenty years later Chebyshev was to change his mind—without mentioning either Sokhotskii's thesis or any of Cauchy's relevant works—and write a couple of papers

(1886 and 1889) using the theory of residues to evaluate approximations to the integral of a real, positive function.⁹⁰)

It seems that Sokhotskii did not consider the theorem named after him, Casorati, and Weierstrass, today to be his major contribution. He did not mention it in the preface to his dissertation and instead emphasised the importance of the theory of residues, a subject which “is hardly being developed at all by present-day scholars” (in Markushevich 1996, 230). His dissertation is divided into two parts.⁹¹ The first one is theoretical in character and includes two chapters: “General properties of uniform functions” (pp. 1–24), and “Theory of integral residues” (pp. 24–43). The second part (pp. 44–135) is devoted to applications, and includes chapters on the “expansion of inverse functions in series” (Lagrange series in particular), “symmetric functions”, Legendre polynomials and the expansion of functions in continued fractions. The results he obtained as applications of his theorems on residues were not new, as he openly recognised in the preface.

Sokhotskii stated his most famous theorem in the following terms in chapter 1:

“If a given function $f(z)$ becomes infinite of infinite order at some point, then the function $f(z)$ must take all possible values at that point” (quoted in (Mitrinović and Kečkić 1993, 94)).

In order to understand this statement correctly, some explanations of Sokhotskii's outdated terminology are needed. When he considered the behaviour of a function $f(z)$ in a neighbourhood of an isolated singularity $z = a$, he looked at the Laurent expansion of $f(z)$ there. If it is of the form

$$f(z) = \sum_{k=-n}^{+\infty} A_k(z-a)^k$$

he said that $f(z)$ becomes infinite of order n at $z = a$, whereas if the expansion is of the form

$$f(z) = \sum_{k=-\infty}^{+\infty} A_k(z-a)^k$$

he said that $f(z)$ becomes infinite of infinite order at $z = a$. His claim that “the function $f(z)$ must take all possible values” at $z = a$ has to be interpreted as meaning that $\lim_{z \rightarrow a} f(z)$ can take any value, as it is clear from his example of the function $\sin \frac{1}{z-b}$ which “takes all possible values at $z = b$ ” as well as from the proof of the theorem which essentially follows the modern route.

Following Cauchy, in Chap. 2 Sokhotskii defined the residue at a point $z = a$, including $z = \infty$, then stated and proved some fifteen theorems on residues that are standard results today. He ended the chapter by introducing the concept of the

⁹⁰For a detailed account, see Mitrinović and Kečkić (1993, 64–67).

⁹¹We follow here the detailed account given in Mitrinović and Kečkić (1993, 93–109).

residue of a function $f(x)$ with respect to a line along which $f(x)$ is discontinuous, defined as $\frac{1}{2\pi i} \int_C f(z) dz$ where C is a closed path enclosing the line.

Sokhotskii graduated in 1873, and according to [Markushevich \(1996, 233\)](#) his thesis “was remarkable in that it laid down the foundations of the theory of singular integral equations containing the Cauchy principal values of integrals”. As Sokhotskii stated in the opening lines of his work,⁹² he tried “to direct the reader’s attention to the solution of the equation $\varphi(x) = \int_a^b \frac{f(t)}{t-x} dt$, namely, to the determination of the function $f(x)$ from a given function $\varphi(x)$ ”. In Sokhotskii’s view, this problem was worth of particular attention “because solving it leads us to the fundamental elements of the theory of functions of a complex variable”. In the first section of his thesis he introduced the concept of integral “of Cauchy type”

$\varphi(x) = \int_a^b \frac{f(t)}{t-x} dt$, where a and b are complex values, the integral is taken along a path joining them, and $f(t)$ “may be discontinuous between the limits of integration”. Sokhotskii remarked that at each point x of the path the integral $\varphi(x)$ assumed two values which he considered as corresponding to points x_1 and x_2 on opposite sides of the path, which he regarded as a line of discontinuity of $\varphi(x)$. Among the theorems he proved, the most important in Markushevich’s view are the following:

Theorem 1. *If $\varphi(x_1)$ and $\varphi(x_2)$ are values corresponding to two points close to the line of discontinuity, then $\frac{1}{2\pi i}(\varphi(x_1) - \varphi(x_2)) = \frac{1}{2}(f(x+0) + f(x-0))$. Consequently, if $f(x)$ is continuous at x , one has $f(x) = \frac{1}{2\pi i}(\varphi(x_1) - \varphi(x_2))$.*

Theorem 2. *If $f(x)$ is continuous at x on the line of discontinuity so that $f(x+h) - f(x-h) = \theta h^\alpha$, where h is infinitely small, $\alpha > 0$ and $\theta > 0$ for $h = 0$, then $\varphi(x_1) = \int_a^b \frac{f(t)}{t-x} dt + \pi i f(x)$, where the integral denotes the limit $\lim_{h=0} [\int_a^{x-h} \frac{f(t)}{t-x} dt + \int_{x+h}^b \frac{f(t)}{t-x} dt]$.*

As [Markushevich \(1996, 234\)](#) remarks, the Lipschitz condition on $f(x)$ in Theorem 2 guarantees the existence of the principal value of the integral under the (implicit) assumption that the line of discontinuity is smooth. He also adds that this and other results by Sokhotskii on the integrals of Cauchy type were later re-discovered by [Plemelj \(1908a\)](#) and eventually applied to the theory of elasticity by N. I. Muskhelishvili and his school in the 1950s.

⁹²We quote from [Markushevich \(1996, 233\)](#), whose account of Sokhotskii’s thesis we follow here.

Chapter 4

Complex Functions and Elliptic Integrals

The definition of these transcendents by an infinite series has the principal advantage that their values are represented in a definite way, while there is no precise way in which they are represented by integrals as soon as the limits of integration become imaginary.

Jacobi

4.1 Introduction

In this chapter we consider how elliptic function theory and complex variable theory were finally drawn together in the 1830s and 1840s. As the recognition of the importance of the work of Abel and Jacobi grew, mathematicians came to feel that it was unsatisfactory to base the theory of elliptic functions on the inversion of many-valued integrals. One alternative would have been to adopt and develop Cauchy's theory of complex integrals. By and large this was not done, and it is interesting to examine why. The study of elliptic integrals was felt by many to be fraught with ambiguity because of the square root in the integrand. Moreover, Cauchy's system of definitions, based on his newly defined concepts of limit, continuity, differentiability, and integrability, was incompatible with talk of many-valued functions—Cauchy did not define continuity for a many-valued function, and indeed a many-valued function cannot be continuous according to Cauchy's use of the term. Although a doubly periodic function is a meromorphic function defined on the whole of the complex plane, an elliptic integral makes better sense on something like a Riemann surface (a torus in this case). Thus the many-valued nature of an elliptic integral posed a challenge to mathematicians throughout the 1830s and 1840s. So the perceived problem with the foundations did not meet with a ready answer in the newly emerging theory of complex functions. Matters were to be worse with hyperelliptic integrals, because the corresponding inverse functions could not be treated as multiply-periodic functions in the plane.

Similar questions rose in the study of more general Abelian integrals, and in the 1830s and 1840s mathematicians were not able to see their way to a conceptual understanding of these functions, so the progress that was made was more on the formal side. But in this way a body of knowledge was established that, supplied with new foundations, could become part of complex function theory in the next decades.

Other areas of mathematics saw the new functions, especially the theta functions, get put to use. In mechanics, the motion of a top can be described in terms of elliptic integrals, and pursuing these ideas led Jacobi to reformulate Abel's theorem (see below, Sect. 4.4) as a theorem in differential equations, the form that Riemann was later to use it. In number theory the use of theta functions was found to underlie one of Gauss's proofs of quadratic reciprocity. Jacobi then made some conjectures about the class number formula that Dirichlet was eventually able to prove as part of his work on the infinitude of primes in arithmetic progressions, thus opening the door to the widespread use of complex function theory in number theory.

4.2 Unifying the Theories of Elliptic and Complex Functions

4.2.1 *Jacobi's Theta Functions*

In the winter of 1835/36 Jacobi lectured on elliptic functions (Jacobi 1835–1836). As is well known, the central novelty of these lectures was a new approach based on the theory of theta functions, but these lectures also inform us about Jacobi's opinion of Cauchy's ideas. Koenigsberger, commenting on these lectures, of which Rosenhain's notes were available to him, said that they were of “a depth and elegance of representation, and brought forward such a richness of material, that none of his earlier lectures were necessary” (Koenigsberger 1904a, 420). Rosenhain said that they stretched Jacobi's listeners so much that it was thought fit to set the new year's seminars aside to study them.

Theta functions had already occurred in Jacobi's work on elliptic functions (see Sect. 1.3); what was new was that now they played a foundational role in the theory. As Jacobi said, neither Abel nor Fourier had thought of the connection between theta functions and elliptic functions. Jacobi also made it clear that one of his reasons for according to the theta functions such a fundamental position in the theory was their immense power in the analytic theory of numbers, which he was currently exploiting.

A theta function is a remarkably rich object, with a history that goes back into the eighteenth century and an intimate connection with the heat equation, whence the reference to Fourier. There are many definitions of theta functions, of which Jacobi's may serve as typical.¹ It is a function of two complex variables, z and τ ,

¹This is not the same as the object defined by Eq. (1.51).

$$\theta(z, \tau) = \sum_{n \in \mathbb{Z}} (-1)^n e^{n^2 \pi i \tau} e^{2inz} = 1 + 2 \sum_{n \in \mathbb{Z}} (-1)^n e^{n^2 \pi i \tau} \cos(2nz) \quad (4.1)$$

The series converge (absolutely and uniformly, as we would now say) for all values of z wherever τ lies in the upper half plane. It is legitimate to differentiate term by term and deduce that the theta function satisfies the one-dimensional heat equation

$$\frac{\partial \theta(z, \tau)}{\partial \tau} = \frac{\pi}{4i} \frac{\partial^2 \theta(z, \tau)}{\partial z^2}. \quad (4.2)$$

The theta function is periodic in its first variable with period π :

$$\theta(z + \pi, \tau) = \theta(z, \tau), \quad (4.3)$$

and also quasi-periodic with respect to the first variable

$$\theta(z + \pi\tau, \tau) = -e^{-\pi i \tau} e^{-2iz} \theta(z, \tau). \quad (4.4)$$

Jacobi's treatment was apparently akin to the one he developed three years later and which slowly became much better known, being, in Koenigsberger's phrase, more systematic. It was finally published in 1881, in the first volume of Jacobi's *Werke*. There he introduced three new theta functions, all defined in terms of the theta function defined by Eq. (4.1) (see Jacobi *Werke* 1, 501).

The new functions are

$$\theta_1(z, \tau) = -ie^{iz + \pi i \tau/4} \theta\left(z + \frac{\pi\tau}{2}, \tau\right), \quad (4.5)$$

$$\theta_2(z, \tau) = \theta_1\left(z + \frac{\pi}{2}, \tau\right), \quad (4.6)$$

$$\theta_3(z, \tau) = \theta\left(z + \frac{\pi}{2}, \tau\right), \quad (4.7)$$

which he regarded as functions of the single variable z . The function θ_1 is an odd function of the variable z , the other theta functions are even functions of z , as can be seen by noting that the Fourier series for these functions are pure cosine series except for the one for θ_1 , which is a pure sine series.

Jacobi found many simple relationships between these functions by playing with the indices of summation, which not only captivated him but also enabled him very quickly to reach his famous theorem on the sums of products of four theta functions. This theorem asserts that if w, x, y, z and w', x', y', z' are two sets of four variables, related by the equations

$$w' = (w + x + y + z)/2 \quad (4.8)$$

$$x' = (w + x - y - z)/2 \quad (4.9)$$

$$y' = (w - x + y - z)/2 \quad (4.10)$$

$$z' = (w - x - y + z)/2 \quad (4.11)$$

then

$$\begin{aligned} & \theta_3(w) \theta_3(x) \theta_3(y) \theta_3(z) - \theta_2(w) \theta_2(x) \theta_2(y) \theta_2(z) = \\ & \theta(w') \theta(x') \theta(y') \theta(z') + \theta_1(w') \theta_1(x') \theta_1(y') \theta_1(z'). \end{aligned} \quad (4.12)$$

This theorem seems to have been decisive in his move to “invert the historical route to the discovery of elliptic functions”, to quote from the lectures of 1835–1836² where it occupies a prominent place near the start. Its importance is that Jacobi could then deduce a remarkable collection of striking identities from his theorem on four theta functions, including these (on setting $x = y = z = -w$):

$$\theta_3(x)^4 + \theta_1(x)^4 = \theta(x)^4 + \theta_2(x)^4 \quad (4.13)$$

and

$$\theta_3(0)^4 = \theta(0)^4 + \theta_2(0)^4. \quad (4.14)$$

The last amounts to the identity

$$\begin{aligned} & \{1 + 2q + 2q^4 + 2q^9 + \dots\}^4 = \\ & \{1 - 2q + 2q^4 - 2q^9 + \dots\}^4 + 16q \{1 + q^{1.2} + q^{2.3} + \dots\}^4, \end{aligned} \quad (4.15)$$

where $q = e^{\pi i \tau}$, from which, as Jacobi had long ago told Legendre, theorems on expressing numbers as the sums of four squares follow directly (see Sect. 1.4). It also allowed Jacobi to define two quantities k and k' as follows (*Werke* 1, 511):

$$\sqrt{k} = \frac{\theta_2(0)}{\theta_3(0)}, \quad \sqrt{k'} = \frac{\theta(0)}{\theta_3(0)} \quad (4.16)$$

while ensuring that $k^2 + k'^2 = 1$. Other equations derived immediately before these enabled Jacobi to define an angle ϕ such that the elliptic functions themselves could be defined via these quotients (p. 512):

$$\sqrt{k} \sin \phi = \frac{\theta_1(z)}{\theta(z)}, \quad \sqrt{\frac{k}{k'}} \cos \phi = \frac{\theta_2(z)}{\theta(z)}, \quad \frac{1}{\sqrt{k'}} = \frac{\theta_3(z)}{\theta(z)}. \quad (4.17)$$

Other formulae already obtained could therefore be interpreted as the addition theorem for elliptic functions. Jacobi then deduced in half a page that

²For the theorem, see Jacobi *Werke* 1, p. 506, and for the quote p. 499. The identities below occur on pp. 510–511.

$$\frac{\theta_3(0)\theta_1'(0)}{\theta(0)\theta_2(0)}z = \int_0^\phi \frac{dt}{\sqrt{1-k^2\sin^2 t}}. \quad (4.18)$$

This showed that z is proportional to an elliptic integral, thus establishing that the new functions are indeed to be obtained by inverting an elliptic integral. The constant of proportionality depends on the value of the complete elliptic integral

$$K = \int_0^{\pi/2} \frac{dt}{\sqrt{1-k^2\sin^2 t}}. \quad (4.19)$$

These formulae permitted Jacobi to define ϕ , k , and k' as functions of z and q . Next he showed how to determine z as a function of ϕ and k without using q , and then how to determine q as a function of k . Finally, after four pages of work, Jacobi deduced (p. 525) that $q = e^{-\pi K'/K}$, where

$$K' = \int_0^{\pi/2} \frac{dt}{\sqrt{1-k'^2\sin^2 t}}. \quad (4.20)$$

Although Jacobi established many more results in this paper we have quoted enough for our purposes. The central importance of the theta functions for Jacobi was that they were defined without reference to integrals and were capable of yielding not only the basic aspects of the *Fundamenta nova* but also a powerful array of techniques for proving results about elliptic functions. However, an important gap remained, as Weierstrass pointed out in his notes to Jacobi's *Werke* (1, 545). Whereas Jacobi's original approach had depended on a real parameter k , it now depended on a complex one, q , related to k by the equation $k = \left(\frac{\theta_2(0,q)}{\theta_3(0,q)}\right)^2$. However, he had only shown how to obtain q from k when k was real and between 0 and 1. Weierstrass commented that it was very important both for the general theory of elliptic functions and for their applications to solve this problem in general, and he proposed shortly to publish his solution, which used a fourth order transformation due to Jacobi. So the new theory, safely removed from the domain of complex integrals, was indeed more rigorous, but it was still limited to real moduli.

As for the importance of the theta functions, [Dirichlet](#) (1852, 15) had this to say in his obituary of his friend Jacobi:

When one thinks that the new function now dominates the entire domain of elliptic transcendents, that Jacobi derived important theorems in higher arithmetic from its properties, and that it plays an essential role in many applications, . . . , one must accord this function by far the paramount position in the science of elementary transcendents. Yet as important a function as this still has no other name than the transcendent Θ , the accidental sign by which Jacobi first denoted it, and mathematicians would pay a debt of gratitude to Jacobi if they joined his name to it, and honour the memory of a man whose finest discovery it was to have been the first to recognise the inner nature and great significance of this transcendent.

Jacobi himself commented that Cauchy's work was inadequate for the purposes he had in mind³:

The definition of these transcendents by an infinite series has the principle advantage that their values are represented in a definite way, while there is no precise way in which they are represented by integrals as soon as the limits of integration become imaginary, because we are less accustomed to them in this form. Their theory has been described by Cauchy, but it lacks the degree of clarity that an infinite series has. Specifically, there is the obscurity that the square root under the integral sign brings, particularly when the variable takes imaginary values

[Koenigsberger](#) (1879, 189) went on to write that Jacobi ran into great difficulties “with the idea of the infinitely many values of elliptic integrals which he sought to explore using Cauchy's intuitive approach”.

4.2.2 *Cauchy's Theory of Elliptic Functions*

Given the difficulties Jacobi was experiencing understanding the complex integrals underlying the theory of elliptic functions, it is interesting to see what Cauchy himself had to say about the foundations of elliptic function theory. This is a topic usually omitted from even the best histories of the subject.⁴ After his first two papers on elliptic functions in 1841 (see Sect. 3.5.1) Cauchy wrote quite extensively on them in a series of papers written in the concluding months of 1843 when, for some reason, his attention was drawn back to the subject of functions expressed by means of infinite products.

In a note of September 25 (1843c) he applied the calculus of residues to transform infinite products of linear factors into power series, thus re-obtaining some of Jacobi's formulae (1843c, 59). Then, the following week, on 2 October he presented to the Académie a paper (1843d) in which he proposed to consider only what he called geometrical factorials, expressions of the form

$$w(x, t) = (1 + x)(1 + tx)(1 + t^2x)(1 + t^3x) \dots \quad (4.21)$$

He remarked ([Cauchy 1843d](#), 66):

As each of the so-called elliptic functions reduces to the ratio of two factorials, one should not be astonished to find that the formulae deduced from the consideration of geometrical factorials provide, as particular cases, the developments of elliptic functions in series.

The details need not detain us except to note that the calculus of residues provided the bridge between the infinite products or factorials and the infinite series. On

³Quoted in [Koenigsberger](#) (1904b, 186–187).

⁴It has been discussed, briefly, in [Houzel](#) (1978) and in more detail in [Gray](#) (1992).

16 October he obtained the three elliptic functions “most commonly used” within the framework of his theory (Cauchy 1843e, 96). In the next memoir, of 23 October, he worked his way back to the elliptic integrals and commented that

M. Jacobi has reduced the evaluation of elliptic functions to the determination of the relationships which exist between the functions we call reciprocal factorials One can with some advantage pursue the opposite route and, having first established directly the remarkable relationships which these reciprocal factorials enjoy and the formulae which express these properties, one can deduce from these formulae those which serve to establish the relationships of relevance to elliptic functions. (Cauchy 1843f, 97).

So news of Jacobi’s lectures had not reached Paris.

Cauchy was particularly pleased with the fact that from his starting point it was easy to study how the formulae involving elliptic functions must be modified when the variable x becomes imaginary. But scarcely had he begun to consider the details when he was blown off that course by the need to respond to Laurent’s discovery of what we today call Laurent’s theorem, as was discussed in Sect. 3.5.2, and he did not return to the topic of elliptic functions until 1846.

These 70-odd pages written by Cauchy in barely one month do not seem to have greatly changed the subject of elliptic function theory. This may be because they do not, themselves, bear on any unsolved problems in the subject. But they do present a reasonable foundation for it in the theory of particular kinds of infinite products which, via the residue calculus, leads to all the principal results. They show that Cauchy, at least, was aware that Jacobi’s method of simply replacing real values by complex ones was not automatically valid. They also show that Cauchy was still unable to include in his ideas about complex integration a theory of the integrals of algebraic functions.

4.2.3 The Theories of Cayley and Eisenstein

4.2.3.1 Cayley

Further light is shed on contemporary attitudes to complex integrals by some early papers of the English mathematician, Arthur Cayley. In 1845 Cayley wrote a paper (1845a) on elliptic functions, designed to clear up the indeterminateness in the value of Abel’s complete elliptic integrals

$$\begin{aligned}\frac{\omega}{2} &= \int_0^{\frac{1}{c}} \frac{dx}{\sqrt{(1-c^2x^2)(1+e^2x^2)}}, \\ \frac{v}{2} &= \int_0^{\frac{1}{e}} \frac{dx}{\sqrt{(1+c^2x^2)(1-e^2x^2)}}.\end{aligned}\tag{4.22}$$

Cayley (1845a, 135) saw that the indeterminateness depended on the path of integration:

Where the limits are real, it is tacitly supposed that the variable passes through a succession of real values, and thus ω, v may be considered as completely determined by these equations, but only in consequence of this tacit supposition. If c and e are imaginary, there is absolutely no system of values to be selected for ω, v in preference to any other system. The only remaining difficulty is to show from the integral itself, independently of the theory of elliptic functions, that such integrals contain an indeterminateness of two arbitrary integers; and this difficulty is equally great in the simplest cases. Why, a priori, do the functions

$$\sin^{-1} x = \int^x \frac{dx}{\sqrt{1-x^2}} \text{ or } \log x = \int^x \frac{dx}{x}$$

contain a single indeterminate integer?

By raising this question but not answering it, Cayley showed very clearly that he understood that a complex integral was a path integral but that he did not have a theoretical understanding of the implications of that view.

Cayley reformulated the theory of elliptic functions in two long papers of 1845, written in one form for the *Cambridge Mathematical Journal* (Cayley 1845b) and in an only slightly more general form (Cayley 1845c) for Liouville's *Journal*. In his view the best way to start was with formulae Abel had put at the end of his great memoir, which gave the expansions of the elliptic functions as quotients of two infinite double products. These he called, following Cauchy but without acknowledging the fact, "Abel's double factorial expressions" because the general factor contained two independent integers. So this double factorial form displayed the reason for the double periodicity. However, Cayley noted (1845b, 136), nothing like them appeared in Jacobi's *Fundamenta nova* and this made it "difficult to trace the connection between Jacobi's formulae; and in particular to account for the appearance of an exponential factor that runs through them".

Cayley's new starting point had its analogue in the theory of the trigonometric functions, specifically the infinite product expansion for the sine function. Accordingly Cayley proposed to study the function defined by the double product

$$u = x \prod \prod \left(1 + \frac{x}{(m\omega + nvi)} \right), \quad (4.23)$$

where ω and v are real positive quantities and the product is taken over all integers m and n (the case $m = n = 0$ being tacitly excluded). After making some remarks about the convergence of such products which are of independent interest Cayley rapidly and simply deduced a variety of interesting results. His approach was successful and he eventually found that all the functions Abel had introduced were at his fingertips. The hardest problem involved passing from the doubly infinite products to doubly infinite sums. Cayley relied on a theorem he took from Cauchy's *Exercices de mathématiques* 2, 289,⁵ which he interpreted for his readers as follows: "the integral

⁵See Cauchy (1827e, 336).

residue in question is the series of fractions that would be obtained by the ordinary process of decomposition”.⁶

So we see that Cayley did not regard the inversion of an elliptic integral as the best starting point for a theory of elliptic functions, because the theory of such functions was not clear to him. In particular, the reason for the periodicity of such functions was not clear, although he knew that elliptic integrals were infinitely many valued since they depended on the path of integration. Instead, not unlike Cauchy, Cayley advocated starting with doubly infinite products, and deriving Abel’s elliptic functions from four such products, cunningly chosen. This is intriguing testimony to the way in which different parts of Cauchy’s theory of complex functions were gaining acceptance.

4.2.3.2 Eisenstein

A similar approach to Cayley’s to the study of elliptic functions was taken at about the same time by the young German mathematician Gotthold Eisenstein. His work poses a delicate problem for the historian, because its reception is so hard to evaluate. On the one hand, in 1847 no less a figure than Gauss wrote the preface to the volume of Eisenstein’s mathematical works ([Eisenstein 1847b](#)) and said that Eisenstein had produced so many good things that he had secured for himself an honourable place among his predecessors, Euler and Lagrange. A remarkable honour for one who was only 24 at the time. On the other hand, as Weil observed, echoing Kronecker, Eisenstein’s work was soon seldom quoted; “Eisenstein’s ideas could indeed seem ‘as good as lost’ ” ([Weil 1976](#), 4).

Eisenstein himself was another prodigy supported by Crelle, whose journal in 1844 opened its pages to a flood of articles by the young man, later collected in his *Mathematische Abhandlungen* (1847b). Crelle commended him to Alexander von Humboldt, who was in charge of education in Prussia, and, mindful of the tragic fate of Abel, successfully set about raising him a salary from the Prussian Ministry of Culture. In 1845 Eisenstein was able to meet his exact contemporary Kronecker almost daily, and in this way was taken up by Kummer. Kummer, supported by Jacobi and von Humboldt, successfully urged that Eisenstein be awarded an honorary doctorate at the university of Breslau in early 1845. Meanwhile von Humboldt had introduced him to Gauss, with whom he maintained a lengthy correspondence. But in 1846 a dreadful priority dispute broke out between Eisenstein and Jacobi, who accused the younger man of stealing some of his results. At this time Kronecker left Berlin, and Eisenstein became isolated mathematically. In 1847 Eisenstein “habilitated” from the university of Berlin and that summer gave a lecture course on elliptic functions with the young Bernhard Riemann in his audience. In the revolutionary year of 1848 he was badly beaten up by Prussian soldiers and earned a reputation for radicalism that apparently threatened his stipend

⁶[Cayley \(1845b](#), 148). This is stated in slightly different terms also in [Cayley \(1845c](#), 175).

from the Prussian Ministry. In 1850 Dirichlet, with the approval of Jacobi, proposed him for membership of the Berlin Akademie, and in 1852 he was indeed elected as Jacobi's successor. But his health, always poor, was by then broken. Like Abel before him, early fame was followed by an early death from tuberculosis; Eisenstein only lived to be 29.

His chief mathematical interest was in the theory of numbers, which he approached via the theory of elliptic functions, and the implications of his work for a theory of complex functions are slight. As Weil said (1976, 35) "One virtue of Eisenstein's approach is that it supplies directly (without recourse to function theory) so many of the formulas in the theory of elliptic functions, in the explicit form which is most appropriate for their use in number-theory". And later on: "Eisenstein had obviously prided himself on the wholly elementary nature of his function-theoretic methods". In a short paper (Eisenstein 1844) Eisenstein began by observing the analogy between the infinite product expansion of the trigonometric functions and the infinite double product for the elliptic transcendents. He proceeded to sketch the details, contenting himself in this short paper with showing how to obtain the infinite product expansions given by Jacobi. These, he said, can be written as the quotient of four similar products of the form $\prod (1 - \frac{x}{(\lambda + \lambda' A)})$ where each of λ and λ' runs through either the even or the odd integers ($\lambda = \lambda' = 0$ excepted) and A is a given imaginary constant. Indeed, if one writes $P(0, 0; x)$, $P(1, 0; x)$, $P(0, 1; x)$, and $P(1, 1; x)$ according as λ and λ' are, respectively, even or odd, then the familiar elliptic functions are, he claimed, $\frac{P(0, 0; x)}{P(0, 1; x)}$, $\frac{P(1, 0; x)}{P(0, 1; x)}$, and $\frac{P(1, 1; x)}{P(0, 1; x)}$.

His reasons for taking this new approach are worth quoting in full (Eisenstein 1844, 188):

The usual definition that one gives of the elliptic functions, quite contrary to the analogy with the exponential function, is that they are the inverse functions of certain well-known integrals But since already the clear meaning of such an integral, whose differential goes suddenly from real to imaginary, is not easy to grasp, it may rather seem almost impossible for the learner to obtain a clear idea a priori of the inversion of such an integral function, while the geometrical intuition which one can at least call on for the circular functions is here broken. There is a particular difficulty with the periods If one wants to define sine as that function y of x which is given by the integral

$$\int^y \frac{dy}{\sqrt{1-y^2}} = x$$

then to agree with the well-known properties of sine one must consider that the integral takes infinitely many different values for each given value of y , which contradicts the usual meaning one gives to such an integral (e.g. for $y < 1$). In the same way for the elliptic integral . . . $\sin am x$ one must say that for each value of y it takes so to speak infinitely many different values infinitely many times.

What this quotation tells us is that Eisenstein's difficulties with complex integrals were even greater than Cayley's, for he made no mention of the way a complex integral depends on the choice of path between its endpoints.

Eisenstein returned to the foundations of elliptic functions in the long paper (1847a). Even its lengthy title is worth quoting: "A precise study of the infinite

double products by means of which the elliptic functions can be represented as quotients, and the double series connected with them (as a new foundation for the theory of elliptic functions with particular respect to their analogy with the circular functions)". Seldom can a title have said so much. Eisenstein introduced elliptic functions by first considering the infinite double product

$$\phi(x) = x \prod \left(1 - \frac{x}{u}\right),$$

where $u = m + ni$. This product and the corresponding infinite sum are taken over all integers m and n except the pair $(m, n) = (0, 0)$. The basic periods of the function are 1 and i , as can be seen by comparing this function with Euler's infinite product for the sine function.

Eisenstein then defined the general elliptic function, $y = (2, x)$ in his notation, by the infinite sum $(2, x) = \sum' (x + w)^{-2}$, where the sum is taken over all $mu + nv$ in which m and n are integers, not both zero, and u and v are two non-zero complex numbers whose ratio u/v is not real. He then defined $(3, x) = \sum' (x + w)^{-3}$ (with the same summation convention) and showed that $(3, x)^2$ satisfies a cubic equation in $(2, x)$ with leading term 1. He took $x = \frac{\alpha}{2}$, $x = \frac{\beta}{2}$, and $x = \frac{\alpha + \beta}{2}$ as the three independent roots of the equation $(3, x) = 0$, and showed, on setting $a = (2, \alpha/2)$, $a' = (2, \beta/2)$, and $a'' = (2, (\alpha + \beta)/2)$, that $y' = 2(y - a)(y - a')(y - a'')^{1/2}$, so "The function $(2, x)$ is therefore in fact an elliptic function of the first kind in x " (Eisenstein 1847a, 226). So Eisenstein, like Cayley before him, had a general grasp of elliptic functions with arbitrary periods, something which the then-current theory of integration made it hard to obtain. Eisenstein was also pleased that his approach showed clearly why the elliptic functions are doubly periodic, whereas Jacobi's approach of representing them as quotients did not; the numerator and denominator are each only singly periodic, so the periods appear unsymmetrically (1847a, 214).

Eisenstein's ideas stimulated Cauchy to have second thoughts about elliptic integrals, and he produced a series of papers that ran through several memoirs in the *Comptes rendus* before coming to a halt (see Sect. 3.5.2). He agreed in his (1846i and 1846o) that Eisenstein had quite rightly pointed out some difficulties, and replied (Cauchy 1846i, 167) that

What one calls the inverse integral of the definite integral

$$t = \int_0^x \frac{dx}{\sqrt{1-x^2}}$$

is not the same as the value of x deduced from the complete integral of the differential equation

$$dx = \sqrt{1-x^2} \cdot dt$$

but the value of x that provides the complete integral of the differential equation

$$dx = y dt$$

if one requires the new variable y

1. to satisfy the finite equation

$$x^2 + y^2 = 1,$$

2. to vary with x by insensible degrees, and if one requires, moreover, that x, y, t simultaneously take the initial values

$$x = 0, \quad y = 1, \quad t = 0.$$

Similar considerations, he went on, pertained to elliptic integrals. His conclusion was that in this way one could profitably hope to avoid irrational functions and those involving radicals in favour of differential equations. While these few remarks are refreshingly clear, and seem to prefigure the theory of Riemann surfaces, that theory is properly named for its German discoverer. Cauchy did little more in this direction. What he did do is confined to his theory of “lignes d’arrêt”, first presented in January 1851, as was discussed at the end of the previous chapter (see Sect. 3.6.2).

So the customary observation that Cauchy left it to others to create a theory of algebraic functions and their integrals is correct, but more can be said. Cauchy by and large avoided those branches of mathematics which, however attractive, involved many-valued functions, and other leading younger mathematicians shared his concern when it came to many-valued integrands. The test case, elliptic function theory, was reformulated without reference to elliptic integrals in order to avoid the problems the square root brought with it, and it was only Riemann who found a way to extend Cauchy’s way of thinking about functions (i.e. single-valued functions) to the integration of algebraic functions. This left open the acute question of hyperelliptic integrals, and we shall turn to them in the next Section. There remains the interesting question of how the theories of elliptic functions and complex functions were first brought together, and we conclude this section with a brief account of that story.⁷

4.2.4 Liouville and Hermite on Elliptic Functions

The prime mover was Joseph Liouville, who in the 1840s was at the height of his powers (Fig 4.1). Liouville had only ranked eighth when he graduated from the École Polytechnique in 1827, but over the next three years at the École des Ponts et Chaussées he came to realise that the career of an engineer was not for him and he withdrew before completing the three years of training to embark on a career as a mathematician. By then he had already submitted a memoir on the theory of heat to the Académie des sciences in Paris, but 1829 was the wrong year because this was the year the *Grand Prix* went to Abel and Jacobi. Liouville persevered, writing

⁷The matter has been investigated by Lützen in his magisterial biography of Liouville (Lützen 1990), upon which our account is based.

Fig. 4.1 Joseph Liouville (in Lützen, *Joseph Liouville 1809–1882: master of pure and applied mathematics*, Springer ©1990, frontispiece)



widely on theoretical applied mathematics while holding various teaching positions, but the delays in publishing articles on mathematics and the few journals that would take them inspired him to create a journal of his own. The first volume of the *Journal des mathématiques pures et appliquées* appeared in 1836. As its name suggests, it was modelled on Crelle's *Journal für die reine und angewandte Mathematik*, founded in ten years before, and it was quickly a success. Liouville's career also began to prosper. He became a professor at the École Polytechnique in 1838 and was elected to the Académie des sciences the next year. Thereafter he did important work on differential equations (Sturm–Liouville theory), stability of rotating masses of fluids, potential theory and integral operators, and constructed the first provably transcendental numbers.

A wide-ranging mathematician, and the editor of a successful journal, Liouville read the first edition of Abel's *Oeuvres*, and discussed the theory of elliptic functions with Charles Hermite, then a student of his (see Lützen 1990, Chap. XIII). Hermite had entered the “mathématiques préparatoires” class of the Collège Louis-Le-Grand in 1840, where, like Galois before him, he had Louis Richard as his mathematics professor, who considered him as “a little Lagrange” (Brezinski 1990, 6). Two years later he entered the École Polytechnique ranked 68, a mediocre rank essentially due to his very poor performance in descriptive geometry, a subject he disliked all his life. After a year there he was threatened with dismissal because of a congenital deformity of his foot (all his life he walked with a cane) and although the dispute was resolved he elected not to graduate from the École, and with Liouville, his professor of analysis, as his protector Hermite began his mathematical career.

Liouville wrote in one of his many notebooks that Hermite had shown him that there cannot be an analytic function with two distinct real periods by an argument

of the following sort, due originally to Jacobi. Suppose that there is such a function, and write down its Fourier expansion with respect to one of the periods. Now look at the coefficients and use the fact that the function has another period. One rapidly obtains a contradiction.

To be precise, if the function is represented in this way with respect to the period α :

$$f(x) = A_0 + \sum_j A_j \cos\left(\frac{2j\pi x}{\alpha}\right) + \sum_j B_j \sin\left(\frac{2j\pi x}{\alpha}\right) \quad (4.24)$$

then the fact that $f(x) = f(x + \beta)$ implies that

$$f(x) = A_0 + \sum_j A_j \cos\left(\frac{2j\pi(x + \beta)}{\alpha}\right) + \sum_j B_j \sin\left(\frac{2j\pi(x + \beta)}{\alpha}\right). \quad (4.25)$$

On equating the j th coefficients and adding the squares of the resulting equations, Liouville found that

$$(A_j^2 + B_j^2) \left(\sin^2 \frac{2j\pi\beta}{\alpha} + \left(1 - \cos \frac{2j\pi\beta}{\alpha} \right)^2 \right) = 0. \quad (4.26)$$

So if α/β is irrational and $j \neq 0$ it must be the case that $A_j = B_j = 0$, and the “function” reduces to a constant.

Liouville soon saw that this proof could be extended to establish rather more, namely that there can be no doubly periodic function analytic which is everywhere finite. For, let such a function $f(x + iy)$ be expanded as a series in x of the form

$$f(x) = \sum_j A_j \cos\left(\frac{2j\pi x}{\alpha} + \varepsilon_j\right), \quad (4.27)$$

where α is a period. For β to be a period, it is necessary that

$$A_j \cos\left(\frac{2j\pi x}{\alpha} + \varepsilon_j\right) = A_j \cos\left(\frac{2j\pi x}{\alpha} + \varepsilon_j + \frac{2j\pi\beta}{\alpha}\right). \quad (4.28)$$

So if α/β is irrational and $j \neq 0$ it must be the case that $A_j = 0$, and again the function is a constant. As Lützen points out in his analysis of Liouville’s work (Lützen 1990, 539) Liouville’s argument extends readily to any doubly periodic complex function (see also Peiffer 1983). As we noted earlier (Sect. 3.5.6) Liouville was very pleased with the force of this discovery, although he was never to publish a proof, and confined himself to giving private lectures on it. One of these courses he gave to the German mathematicians Borchardt and Joachimsthal when they visited Paris in 1847, and Borchardt, in his capacity as an editor of the *Crelle’s Journal*, finally published his notes of these lectures in 1880. But in an otherwise faithful

publication he chose to replace Liouville's original proof with one of his own, based on the calculus of residues.

As mentioned in Sect. 3.6.2, in 1849 Hermite presented a memoir to the Académie on the theory of doubly periodic functions based on Cauchy's theory of functions of a complex variable. The manuscript was thought lost for a long time but was found by Belhoste in 1988 and published by him in (Belhoste 1996). We shall discuss it shortly, but first we deal with the controversy occasioned by Cauchy's report on it, given on 31 March 1851. He told his audience that Hermite had established a close connection between elliptic functions and doubly periodic functions and established the result that the sum of the residues of a doubly periodic function inside its period parallelogram is zero. This was another result known to Liouville, and Liouville at once rose to point out that he himself had had such a general theory of doubly periodic functions since 1844. This theory was based, he said, on the general principle that a doubly periodic function cannot have fewer than two infinities (in its period parallelogram) without reducing to a constant. This principle is not quite the one he had come to in 1844, but it is in fact equivalent to it, as the lecture notes taken by Borchardt establish.

Cauchy's responses to Liouville were discussed above (see Sect. 3.5.6). Provoked by Cauchy's priority claim, Liouville decided to lecture publicly on his work at the Collège de France in 1851. There he proved that a bounded complex function f is constant by arguing that $|f|$ takes its maximum value at a point z_0 say, and then the Cauchy integral formula implies that

$$|f(z_0)| = \left| \frac{1}{2\pi} \int_0^{2\pi} f(z_0 + re^{i\theta}) d\theta \right| \leq \int_0^{2\pi} \frac{1}{2\pi} |f(z_0 + re^{i\theta})| d\theta \leq |f(z_0)|, \quad (4.29)$$

which is impossible unless the function is constant (see Peiffer 1983). Among his audience were Briot and Bouquet. These two decided to publish what they had learned of complex function theory from Cauchy and about elliptic functions from Liouville, with generous acknowledgements to both, as a book. When it appeared, in 1859, it was the first textbook in the world to present the new theory, and so it did much to spread Cauchy's and Liouville's ideas. Liouville was not best pleased, however, and wrote in his notebooks later: "MM. Briot and Bouquet, cowardly thieves, but the most worthy Jesuits. Elected as thieves by the Academy!!!!!" (quoted in Belhoste and Lützen 1984, 28).

So should the honour for discovering the principle be divided? Even if it was Cauchy who extracted Liouville's principle from its original setting in the theory of elliptic functions and gave it a much more natural proof within a theory of complex functions, it remains true that it was Liouville who first discovered it. The more Cauchy showed how automatically it followed from his earlier work, the more surprising it is that he never thought to remark upon it himself. It had been within his grasp since 1825, but he had never seen it, perhaps because it lay outside his way of conceiving the subject. On the other hand, in the course of his thorough study of all Liouville's note books, Lützen has found evidence that Liouville had in any

case spotted the generalisation of his principle from the context of doubly periodic functions to all complex functions as early as 1844.

The trick Liouville spotted was to reduce the case of a bounded complex function to a bounded doubly periodic one by composing a given complex function f with the doubly periodic one $\sin am$. The composite function $f(\sin am z)$ is constant, and therefore f must be constant.

So priority resides with Liouville, although his strange attitude to publishing his results must have caused him distress. For once, the attribution of a name in the history of mathematics seems reasonably justified.

However, since as [Belhoste \(1996, 22\)](#) points out, the honour of being the first to study elliptic functions by making systematic use of Cauchy's theory of functions belongs with Hermite, it seems right to present an account of his long-lost memoir before turning to the better-known but later version of Liouville. Hermite wrote his memoir to show that every doubly periodic function is the quotient of two singly periodic functions (with the same period) which are defined everywhere. That is to say, symbolically, the function is of the form

$$\frac{\sum_m A_m \exp\left(2m \frac{i\pi x}{a}\right)}{\sum_m B_m \exp\left(2m \frac{i\pi x}{a}\right)}. \quad (4.30)$$

The general equations that result from insisting that the quotient also be periodic with period b Hermite found intractable, but he could solve special cases. He was therefore led to study doubly periodic functions of the form $F(x) = \frac{\Pi(x)}{\Phi(x)}$, where the functions $\Pi(x)$ and $\Phi(x)$ are periodic with period a .

Hermite defined the period parallelogram of such a function and used integration around the period parallelogram to obtain expressions for the Fourier coefficients of F . The argument shows, in particular, that the sum of the residues of a doubly periodic function in its period parallelogram vanishes. Then Hermite introduced a new function, which he denoted θ although it is not a theta function, and showed that the doubly periodic function F can be written in the form

$$A_0 + \frac{2i\pi}{a} \{r_1 \theta(z - z_1) + r_2 \theta(z - z_2) + \cdots + r_n \theta(z - z_n)\} \quad (4.31)$$

where the r_i are the residues of F at the points z_i . This shows that the function F is doubly periodic, for Hermite had defined the function to be periodic with period a and then showed that it satisfies the equation $\theta(z + b) = \theta(z) - 1$; that b is a period then follows from the fact that the sum of the residues vanishes. The memoir concludes with other expressions for doubly periodic functions.

Liouville's theory of elliptic functions, as presented in private lessons and eventually published by Borchardt as ([Liouville 1880](#)), starts from the attractive idea of looking at the class of doubly periodic functions, and showing that these are identical with the elliptic functions. To avoid needless complexity, in the first half of the memoir he showed that given a pair of periods $2\omega_1$ and $2\omega_2$, and a

doubly periodic function $\phi(z)$ with those periods and exactly two points where the function becomes infinite (in its period parallelogram), every doubly periodic function with those periods is a rational function of $\phi(z)$ and its derivative $\phi'(z)$, that is it can be written in the form $\frac{M+N\phi'(z)}{L}$, where L , M , and N are polynomials in ϕ . So understanding the nature of an arbitrary doubly periodic function is reduced to understanding one with only two infinities. Examples of these are the familiar elliptic functions, so it is natural to ask if Liouville's theory could answer these questions:

1. Are there doubly periodic functions having any pair of periods $2\omega_1$ and $2\omega_2$, assuming only that the quotient of the periods is not real?
2. Is there an elliptic function with the same periods, and if so what is the corresponding modulus?

Liouville did not, however, investigate these questions. Instead, he first showed that if there is a doubly periodic function $\phi(z)$ with periods $2\omega_1$ and $2\omega_2$, points α and β where the function becomes infinite, zeros a and b (when, necessarily, $a + b = \alpha + \beta$) then $\phi(z)$ satisfies the differential equation

$$\{\phi'(z)\}^2 = A\phi^4(z) + B\phi^3(z) + C\phi^2(z) + D\phi(z) + E \quad (4.32)$$

A, B, C, D , and E being constants. If $\alpha = \beta$, then, he said, it is easy to see that $A = 0$. It was therefore open to him to appeal to results in the theory of differential equations as a way of defining doubly periodic functions, and then to investigate how their periods depended on the constants $A, B, \dots, E$. Instead, he confined himself to showing that if one adds the extra condition that $\phi(z) = \phi(-z)$ then the function $\phi(z)$ is a constant multiple of either $\sin \pi z$ or its reciprocal. For then, he showed, the differential equation reduced to

$$\phi'(z)^2 = (\phi'(0))^2 \{1 - \phi^2(z)\} \{1 - k^2 \phi^2(z)\}, \quad (4.33)$$

where $k = \frac{1}{\phi\left(\alpha + \frac{\beta - \alpha}{2}\right)}$.

It might be that Liouville simply did not have the modern taste for establishing the existence of a function with certain defining properties before going on to deduce further properties of it. Certainly, his aim here was to show that his theory embraced the theory of Abel and Jacobi not to extend it. It may be the existence of doubly periodic functions with arbitrary periods was clear to him, perhaps on other grounds, perhaps because of the connection to differential equations, perhaps simply intuitively. In any case, the question of the connection between the periods $2\omega_1$ and $2\omega_2$ and the constants $A, B, \dots, E$ would surely be a subtle one. One might hope that a reduction theorem *à la* Legendre would reduce the quartic polynomial in $\phi(z)$ to a simple form that would enable one to elucidate the connection precisely. At all events, the opportunity was not taken, and in fact, as we shall describe in Chap. 6, the mathematician who pioneered this route successfully was Weierstrass.

As for the implications of the work of Liouville for complex function theory, Jacobi's comments (Jacobi 1847, 519), made after Borchardt had returned to Germany, are most interesting. He wrote:

Liouville and Hermite, in works of which only a suggestion has been published, have derived all the results about addition, multiplication, transformation, and division from the purely periodic properties of the transcendents θ One can say that ... the new and true foundations for the theory of elliptic functions have been found. Apart from the simplicity with which the entire structure can now be presented, it accords with the advantage of greater strength and clarity than was possible up to now in those treatments that began with integrals. For the theory of imaginary values of algebraic functions and their integrals is still not built up enough for the consideration of such integrals, over the entire domain of real and imaginary values with both constant and variable magnitudes equally involved, to be taken as basic. This shortcoming is not intrinsic to the method which served me in the *Fundamenta nova*, but goes back to a gap in the general method of analysis.

4.3 What is a Hyperelliptic Integral?

When Koenigsberger described Jacobi's intuitive use of Cauchy's theory of complex integrals to explore the way an elliptic integral can take infinitely many values (see above, Sect. 4.2.1), he went on to observe that Jacobi came in this way to a "theorem he had proved earlier, namely that a function of one variable cannot have more than two periods". Jacobi had published this result in Crelle's *Journal* in 1835, where he had shown that if a function of a single variable has three periods then either these periods are all obtainable from a pair of periods, or the function has arbitrarily small periods, a possibility he regarded as absurd (see Jacobi 1835, 32). This observation raised a difficult problem: what to make of even the simplest hyperelliptic integral:

$$u = \int_0^z \frac{dt}{\sqrt{(1-t^2)(1-k^2t^2)(1-m^2t^2)}}. \quad (4.34)$$

By analogy with the elliptic case, integral (4.34) must surely define by inversion a function with four distinct periods, yet according to Jacobi's theorem such a thing is absurd. Jacobi's way forward was to take two sums of such integrals and invert them simultaneously, thus obtaining two much better behaved functions of two variables. This, the first appearance of what has come to be called Jacobi inversion, was the crucial move, as we shall see, in the study of hyperelliptic and Abelian integrals, opening up the way for the later insights of Riemann and Weierstrass. As such, it would be hard to underestimate its importance. But it does not resolve the question of what sort of a function the integral (4.34) defines. Precisely for that reason, Jacobi's position tells us much about how complex functions were regarded and indeed were to be regarded for another 50 years.

As regards functions of a single variable with three periods, Jacobi was unambiguous: because of the absurd nature of functions with three periods, they do not

exist. Yet according to Cauchy's theory (4.34) defines an analytic function of its upper end point, and so there is an analytic inverse. Such may have been the opinion of Göpel, who asked in 1847, shortly before his untimely death, why Jacobi believed that this function is absurd. Does the absurdity reside in the very basis of the theory, he enquired, or in the idea of integral, or in the idea of function? Göpel did not answer his own question, but it provoked Jacobi to this reply. The integral (4.34) defines a function u of its upper end point z that can take values arbitrarily close to any given one, merely by varying the path of integration you choose. The infinitely many-valued nature of u is, however, not the heart of the problem, and Jacobi's remark does not therefore remove the difficulty. The astute R.L. Ellis, in his valuable report on elliptic functions commented that

[Jacobi's] conclusion is that we cannot consider the amplitude [z of (4.34)] of this integral as an analytic function of the integral itself. In the present state of our knowledge, this conclusion, though seemingly forced on us by the impossibility of recognising the existence of a quadruply periodic function of one variable, is not, I think, at all satisfactory. The functional dependence, the existence of which we are obliged to deny, may be expressed by a differential equation of the second order; and therefore it would seem that the commonly received opinion that every differential equation of two variables has a primitive, or expresses a functional relation between its variables, must be abandoned, unless some other mode of escaping from the difficulty is discovered. It is probable that some simple consideration, rather of a metaphysical than an analytical character, may hereafter enable us to form a consistent and satisfactory view of the question, and this I believe I may say is the opinion of M. Jacobi himself. Ellis (1846, 304–305).

Jacobi's position was much discussed in the nineteenth century. Dirichlet and Hermite were of the opinion that Jacobi only considered single-valued functions, and Gundelfinger pointed out that Jacobi himself said that he was considering functions only of that type (in Lecture 37 of his lecture course of 1835–1836; see Schlesinger 1905, 90). Schlesinger himself was of a different opinion, namely that it was the analyticity of the functions that Jacobi disputed. In Schlesinger's view, Jacobi called a function analytic only if the set of points where it took a prescribed value was discrete and did not contain points arbitrarily close together. Otherwise an intuitive argument suggests that a continuous function taking a value on a dense set must take the same value on the closure of that set. But in this case the closure is the whole complex plane, so the function must be a constant, which is absurd.

To have thought along those lines Jacobi would have had to have failed to appreciate fully the way in which a complex integral depends on the path of integration, and from what has been said already this cannot be ruled out. It seems to have been a deep-seated habit of even the best mathematicians of the first half of the nineteenth century to regard an integral expression as necessarily single-valued and to treat an integral as a single-valued function of its upper end point. To Jacobi, who perfectly understood that an elliptic integral defines a many-valued function, it may have seemed that the multiplicity of its values was surpassed by those of a hyperelliptic integral to such an extent that the function it defined was absurd. Such seems to have been the burden of his reply to Göpel. On the evidence available,

Jacobi's views cannot be elucidated completely. The truth probably lies closest to Stäckel's remark (quoted in [Schlesinger 1905](#), 91):

It may be valid to add that what we may now express shortly and precisely with modern terminology could only be apprehended by Jacobi after a lengthy discussion, and the entire matter may be an example of what Goethe once called the stupidity of genius, all for the lack of a clear expression that was gradually obtained by the later work of epigones.

The confusion was to persist even among leading mathematicians until the 1880s, catching out Fuchs in Berlin, although by then Weierstrass at least felt it was something of a howler. It was Casorati⁸ who sorted out the problem most clearly, in his (1863–1864), a couple of papers submitted by Hermite to the *Comptes rendus* of the French Académie (see [Bottazzini 1977c](#)), and eventually in the papers he published (1886a,b) correcting Fuchs's mistake. There is indeed an inverse function to the hyperelliptic integral, and it is analytic; but it is not single-valued and Jacobi's argument only applies to single-valued functions. In this way the existence of arbitrarily small periods is explained, and the confusion shown to derive from treating a many-valued function as a single-valued one. The confusion maintained in the theory of functions by treating single-valued functions on a par with many-valued ones was to last until the theory of Riemann surfaces was sufficiently well established to permit a rigorous treatment of the many-valued case. Casorati's insight created the possibility of conceiving of Riemann surfaces with infinitely many leaves, as he stated explicitly more than 20 years later, in the papers where he refuted the claims made by Fuchs and Hermite ([Casorati 1886a,b, 1887](#)).

4.4 Abel's Theorem

The most important single result in the theory of integrals of algebraic functions was also one of the first to be discovered: Abel's theorem. Abel discovered it during 1825 and submitted a lengthy paper concerning it to the Académie des sciences in Paris in October 1826, but a series of mishaps ensured that it was not published until 1841.⁹ Fourier, the secretary, referred it to Legendre and Cauchy for evaluation, but Cauchy failed to read it; he was to become notorious for his inability to respond appreciatively to the works of others, and in this case he seems simply to have been too busy with his own ideas.¹⁰ Abel waited patiently for any response and did not even raise the matter with Legendre when he wrote to him. He did, however, write a short paper on hyperelliptic integrals ([Abel 1828c](#)) in which he alluded in a footnote

⁸A student of Bordoni and Brioschi at the University of Pavia, he was appointed Extraordinarius for algebra and analytic geometry there in 1859, becoming Ordinarius in 1862. The following year he switched to the chair of calculus that he occupied for almost 30 years until the end of his life. For an account of his life and work, see Bertini's obituary in Casorati, *Opere* (1, 3–30).

⁹For the chronology, see [Koenigsberger \(1879\)](#).

¹⁰For this incident, see [Ore \(1957, 150–151\)](#); on Cauchy's notoriety, see [Lützen \(1990, 138\)](#).

to the existence of the more general paper submitted to the Académie. Jacobi, who read every word Abel wrote, was inspired by the footnote to write to Legendre to enquire about the fate of the manuscript. Two days after Abel's death, Legendre replied to Jacobi with a feeble excuse, but something, shame perhaps, drove him and Cauchy to action. On June 29, 1829, Legendre reported the death of Abel to the Académie, and the next week Cauchy reported on the paper itself. In guarded terms that showed he had failed to see the paper's great generality, he proposed that the memoir be published. When Jacobi came to Paris that autumn he pressed Fourier to see that the memoir be published quickly, for he knew that it was more significant than Cauchy thought. So too did Legendre, for he called it Abel's "*monumentum aere perennius*".¹¹ In May 1830 Fourier died, and in September Cauchy left Paris to travel with the overthrown court of the Bourbon King to Italy and later Prague.

The matter became a scandal. Jacobi began to suggest in print ([Jacobi 1832b](#), 10) that the Académie should publish the work at once. The Norwegians themselves started asking questions. Radicals within the Académie, such as the Italian Libri, and its French critics took up the cry. But where was the manuscript? The new secretary, Arago, was tackled in 1838 on behalf of the first editor of Abel's works; he placed the blame on the royal printing house, but the manuscript seemed to be lost. It took a formal complaint from the Norwegian government to stir up the Académie sufficiently to get Abel's great memoir printed. Libri was placed in charge and the work was done—except that in some mysterious way the manuscript disappeared. Or so Libri assured the readers of the published version. But in the 1840s Libri's own status came increasingly into question. Voices were raised suggesting that in the course of his extensive contacts with many libraries, Libri had acquired books and manuscripts dishonestly. In the revolutionary year of 1848 Libri lost the support of Guizot, his most influential political friend, and he fled to England, taking with him boxes containing his most valuable books. It emerged that the charges against him were in the main true, but nothing could be done, and Libri died a wealthy man in Fiesole, near his native Florence. The obvious suspicion was that Libri had stolen Abel's manuscript while it was being printed, but proof was lacking until 1952 when the collection of Libri's papers in the Biblioteca Moreniana in Florence was consulted. There, yellowed, but almost intact, were found all but eight of the original pages of Abel's greatest work; the remaining eight (only four of them belonging to Abel's original manuscript, the others being copies made by Libri) were only discovered in 2002.¹²

The 15-year delay in publishing Abel's paper meant that mathematicians were left to proceed as best they could, with no more than hints as to what his insights

¹¹ Abel's monument more lasting as bronze ([Legendre 1832](#)). The original is Horace, *Odes* 3.30, "*Exegi monumentum aere perennius*", "I have erected a monument more lasting than bronze", where he prophesied an eternal life for his poems and fame for himself.

¹²For a full account, upon which ours is largely based, see [Ore \(1957, 246–261\)](#). The discovery of the remaining eight pages is described in [Del Centina \(2002, 2003\)](#).

could have been.¹³ Most of what they accomplished turned out to be subsumed in that paper, consisting as it did of elaborations, proofs, and clarifications of the few remarks Abel had managed to publish. These had largely concerned the hyperelliptic case, although in letters to Crelle, Holmboe and others he had stated his main theorem. Of those involved, the only one capable of going beyond what Abel had done was, of course, Jacobi himself. Jacobi knew that Abel had opened up a whole new field of analysis and proposed that the new functions Abel had discovered henceforth bear the Norwegian's name ([Jacobi 1832b](#), 10). This came about, arguably a rare case of a just appellation in the history of mathematics.

Abel's insight was nothing less than the generalisation of Euler's addition theorem for elliptic integrals, a discovery that had eluded Euler, Lagrange, and Legendre. That theorem said that the sum of two elliptic integrals (with fixed lower end points and variable upper ones) was equal to a third elliptic integral, having the same lower end point and an upper end point which was an algebraic function of the first two. The question Abel solved was how to generalise this theorem to more complicated integrals.

A clear expression of Abel's most general version of it was sent to Crelle in a letter of 9 August 1826 ([Abel 1902](#), 39):

A general property of functions whose differential is algebraic is that the sum of an arbitrary number of these functions can be written as a definite number of the same functions.

We call this result the addition theorem.¹⁴ This means that for a given integral of an algebraic function, the sum of any number of them is equal to the sum of a definite number, determined by the integrand. In the case of elliptic integrals, the sum of two or more integrals is always equal to one integral; the generalisation raises this number to a quantity μ depending on the precise integral chosen. The questions then are: how is this number μ determined, and how do the end points of the new integrals depend on the original ones?

In a paper of 1829 [Abel \(1829a\)](#) showed how these questions could be tackled in an interesting if special case. There he considered two algebraic functions of x and y , $P(x, y) = 0$ and $Q(x, y) = 0$, where some of the coefficients of Q depend (linearly, one may suppose) on some parameters $a^{(i)}$, and the degrees of P and Q in y are n and $n - 1$, respectively. Then eliminating the variable y between P and Q yields an equation of degree μ in x , the roots of which depend on the $a^{(i)}$. Then, letting $f(x, y)$ be an arbitrary rational function of x and y , he defined the transcendental function $\psi(x) = \int f(x, y)dx$ and claimed that¹⁵

¹³The papers ([Broch 1840](#), 1842; [Jürgensen 1839](#), 1842; [Minding 1842](#); [Rosenhain 1844–1845](#)) are described in [Brill and Noether \(1894](#), 226–234) and following them ([Hancock 1897](#), 258–260).

¹⁴Following ([Kleiman 2004](#), 398).

¹⁵For a mathematical discussion, see [Kleiman \(2004](#), 404–405), who points out that the proof is an exercise in the use of the symmetric functions of the x_j , which depend on the coefficients of the polynomial Q .

$$\psi(x_1) + \psi(x_2) + \cdots + \psi(x_\mu) = u + k_1 \log v_1 + k_2 \log v_2 + \cdots + k_n \log v_n. \quad (4.35)$$

This requires careful reading. One is to think of the algebraic function P as fixed once and for all; it replaces the function $y^2 = (1-x^2)(1-k^2x^2)$ in the case of elliptic integrals. The function Q , on the other hand, depends on parameters, so the common solutions of the equations $P = 0$ and $Q = 0$ depend on these parameters. Fixing the parameter values for the moment gives μ values for x (and therefore for y); these are to be thought of as the upper end points of the integrals. It is clear that they are all related algebraically. The value of μ is determined by P and Q . The integrand $f(x, y)dx$ formed from the rational function $f(x, y)$ plays the role of $\frac{dx}{y}$ in the case of elliptic integrals. Note that the value of μ is determined by P but is independent of the choice of the function f . The definition of $\psi(x)$ is unclear in the fashion of the times: lacking any grasp of the way a complex integral depends on the path of integration, the problem was suppressed. But in an expression like $\psi(x_i)$ one is to integrate up to the end point (x_i, y_i) .

So now the left-hand side can be read: it is about the sum of a definite number of functions whose integrands, $f(x, y)dx$, are algebraic functions of x and y . The assertion is that this sum contains no new transcendental functions but is expressible as a rational function and a number, n , of logarithmic terms. To bring this into line with Abel's statement to Crelle, suppose that one is given a certain number, say r , of integrals of the form $\psi(x)$ with, for simplicity, fixed lower end points and upper end points (x_i, y_i) , $i = 1, \dots, r$. One takes an equation $Q = 0$ of suitable degree, $n > 1$, in y and adjusts its parameters so that the common roots of $P = 0$ and $Q = 0$ include the points (x_i, y_i) , $i = 1, \dots, r$. The other common points are also taken as end points, and the sum of all of the integrals so obtained is equal to a rational and a logarithmic function.

A good, simple case would be when the sum of the transcendental integrals is zero, and the rational and logarithmic terms vanish. This can certainly happen; it does so for hyperelliptic integrals, as we shall indicate below. Abel investigated it in his (1828c), for example. In general it happens when the rational function in the integrand is well chosen (what this means was to be the object of much subsequent interest, but everywhere finite on a non-singular curve is a good example). Let us imagine this has been done. One has been given an algebraic function P and a suitable integrand f . Given r upper end points, one then selects a suitable algebraic function Q depending on some parameters $a^{(i)}$. The upper end points determine the parameter values, the choice of Q being constrained, for the degree of P has determined the degree of Q and so the value of μ , and the conclusion is that the sum the original r integrals plus $\mu - r$ more is equal to zero. Putting that slightly differently, the sum of r integrals is equal to minus $\mu - r$ more. To see why the minus sign arises, consider again the case of elliptic integrals. Given two upper end points, the straight line they determine meets the curve $y^2 = \text{cubic in } x$ in one more point. Taking that as the third upper end point, one has that the sum of these three integrals is zero. To write this in the form of the Euler addition theorem, one has to find a new upper end point, which if the one just found is (x_3, y_3) happens to be $(x_3, -y_3)$.

The result “the sum of r integrals is equal to minus μ plus r more” invites a natural question: what is the smallest value of $\mu - r$? This would give the best possible result, because it would be the simplest sum to obtain. Abel’s full statement of his theorem dealt at length with this question.

It remains to discuss what Abel’s memoir was found to contain when it was finally published in 1841. The statement of Abel’s theorem was as we gave it above, so as it applies to the general and not just the hyperelliptic case. Abel also gave a proof, a surprisingly simple one that Jacobi praised as little more than an exercise in the calculus. He then investigated the minimum value of the quantity $\mu - r$ above and found that the minimum, γ , could be found explicitly once P was known and was attained generally, although cases could arise where it was not if the integrand and the end points were cunningly chosen. Since Abel spoke throughout of equations and their roots, and even avoided the language of curves and their points of intersection, γ was for him an integer attached to an equation $P(x, y) = 0$. Even though this integer played a decisive role in the study of integrals of algebraic functions involving P , he did not seek to interpret it. Indeed, from an algebraic point of view it might seem that it calls for no interpretation. From a geometrical standpoint, however, it could hardly remain unanalysed, and we shall see in the next chapter how important its elucidation was to become once the formal phase of this branch of complex function theory was superseded by a geometrical, even a topological point of view.

Dieudonné, in (Dieudonné 1974, 1, 44), made the strong claim that “even though at that time Cauchy had scarcely begun his work on the integrals of functions of a complex variable, the context shows that Abel understood by $\psi(x_j)$ the curvilinear integral that we now write $\int_{\Gamma} R(t, y) dt$, where Γ is a path from the origin a to an end point x_j in $\mathbb{C}$ and where one must substitute for y the continuous function of t on this path taking at the point a one of the values b that satisfy $F(a, b) = 0$, and such that $F(t, y(t)) = 0$ for every point t of Γ ”. This claim is excessive. Abel’s theorem is entirely formal, and nothing in it suggests that he understood the integrals involved to depend on a choice of path.¹⁶

The whole question of what we should understand by the term “Abel’s theorem” has been very carefully discussed by Kleiman, in (Kleiman 2004). He distinguishes four versions and indicates very carefully which mathematicians and historians have taken as *the* theorem of that name, before going on to describe the four statements and their inter-relations. The interested reader should certainly consult this work, which goes into much more detail than we can here. His conclusions are that the formulation in (4.35), which he calls the Elementary Function Theorem, is the only version Abel proved, and it is key to the other versions. However, Abel’s goal was always to obtain what was called the addition theorem above. Kleiman notes that Abel gave a proof in the hyperelliptic case and an unsuccessful attempt at a proof in

¹⁶For a similar conclusion, see Cooke (1989, 412).

the general case, and that it is this version that Jacobi regarded as the monument to Abel's genius. "Thus", writes Kleiman, "only the Addition Theorem can rightfully be called **Abel's Theorem!**" And we agree.

4.5 Jacobi Inversion

As Abel said in his (1828c, 444–445), and as Jacobi was to see clearly, Abel's theorem suggests very strongly that what one should consider when dealing with hyperelliptic integrals is not an integral on its own but the sum of a number of them. The integral (4.34), for example, should be treated by forming the sum of two, as it turns out (because the integral has four periods). What Jacobi sought was a generalisation of the inversion of an elliptic integral to the hyperelliptic case. He searched for four years, lost in his struggle with functions seemingly having periods of arbitrarily small (non-zero) absolute value before, "almost in desperation", as he said in Jacobi (1835, 45) he realised that with Abel's theorem he had the key in his hand. The crucial insight was to consider the sum of a suitable number of (linearly independent) hyperelliptic integrals instead of just a single integral. According to the theorem Abel stated in his 1826 Paris memoir, the sum of any number of integrals of the form $\int R(x, y) dx$ —where $R(x, y)$ is a rational function and $y = y(x)$ is an algebraic function defined by a (irreducible) polynomial equation $f(x, y) = 0$ —reduces to the sum of a number p of linearly independent integrals and of an algebraic-logarithmic expression. Abel's Paris memoir being still unpublished, Jacobi referred to an excerpt of it which Abel published in his (1828c), dealing with the particular (hyperelliptic) case of the theorem, when $f(x, y) = 0$, P is a polynomial of degree $n > 4$ having no multiple roots. In this case $p = [(n - 1)/2]$, and for hyperelliptic integrals of the first kind $\int \frac{Q(x)dx}{\sqrt{P(x)}}$ (Q is a polynomial of degree $\leq p - 1$) the algebraic-logarithmic expression vanishes.

Jacobi (1832b, 7) reformulated Abel's theorem as follows: $f(x) = X$ denotes any polynomial function of x of degree $2m$ or $2m - 1$, set

$$\int_0^x \sum \frac{A - kx^k}{\sqrt{X}} dx = \Pi(x) \quad (k = 0, 1, \dots, m - 2)$$

then, given m values $x_0, x_1, \dots, x_{m-1}$ of the variable x , $m - 1$ quantities $a_0, a_1, \dots, a_{m-2}$ can be algebraically determined from these, such that they satisfy the transcendental equation

$$\Pi(x_0) + \Pi(x_1) + \dots + \Pi(x_{m-1}) = \Pi(a_0) + \Pi(a_1) + \dots + \Pi(a_{m-2}).$$

The quantities $a_0, a_1, \dots, a_{m-2}$ are roots of an algebraic equation of degree $m - 1$, each of whose coefficients is expressed rationally in terms of $x_0, x_1, \dots, x_{m-1}$ and $\sqrt{X_0}, \sqrt{X_1}, \dots, \sqrt{X_{m-1}}$, where

$$X_0 = f(x_0), X_1 = f(x_1), \dots, X_{m-1} = f(x_{m-1}).$$

Specifically, Jacobi took two integrals and sought to invert them simultaneously. Writing

$$X = (1 - x^2) (1 - k^2 x^2) (1 - m^2 x^2)$$

he took

$$\int_0^z \frac{dx}{\sqrt{X}} = \Phi(z), \quad \text{and} \quad \int_0^z \frac{x dx}{\sqrt{X}} = \Phi_1(z) \quad (4.36)$$

and considered the simultaneous equations

$$\Phi(z_1) + \Phi(z_2) = u_1, \quad \Phi_1(z_1) + \Phi_1(z_2) = u_2. \quad (4.37)$$

As we have seen, full insight into the nature either of $u = \Phi(z)$ as a function of z or of its inverse, z as a function of u , eluded Jacobi. But he found that he could interpret z_1 and z_2 as functions of u_1 and u_2 . The new functions λ_1 and λ_2 had many analogies with the trigonometric and elliptic functions. In particular, they were quadruply periodic in each variable.¹⁷ That is, there were numbers i_1, i_2, i_3, i_4 and i'_1, i'_2, i'_3, i'_4 (the periods, which he defined explicitly in terms of the original integral) such that for any integers m_1, m_2, m_3 and m_4

$$\lambda_1 \left(u_1 + \sum_{j=1}^4 m_j i_j, u_2 + \sum_{j=1}^4 m_j i'_j \right) = \lambda_1(u_1, u_2) \quad (4.38)$$

and

$$\lambda_2 \left(u_1 + \sum_{j=1}^4 m_j i_j, u_2 + \sum_{j=1}^4 m_j i'_j \right) = \lambda_2(u_1, u_2). \quad (4.39)$$

However, since the functions satisfied algebraic addition theorems (as the elliptic functions did), there were relations between the periods and only four were independent. In the four-fold periodicity of these functions of two variables, Jacobi found the resolution of the absurdity that had plagued him earlier.

Jacobi himself commented on this work in 1847, on the occasion of Göpel's death. What he wrote is easier to understand, and neatly points out the connection to his later work on the hyperelliptic case of Abel's theorem and differential equations.

The hyperelliptic functions are roots of a quadratic equation $Ax^2 + Bx + C = 0$, in which A, B , and C are functions of two variables u and v , which are connected with the two roots that we shall denote x_1 and x_2 of the two equations

¹⁷The magnitude of Jacobi's achievement may be judged by Eisenstein's failure to follow him here, a failure the older man judged harshly. See [Hancock \(1897, 278\)](#).

$$\begin{aligned}
 u &= \int_0^{x_1} \frac{dx}{\sqrt{X}} + \int_0^{x_2} \frac{dx}{\sqrt{X}} \\
 v &= \int_0^{x_1} \frac{x^2 dx}{\sqrt{X}} + \int_0^{x_2} \frac{x^2 dx}{\sqrt{X}},
 \end{aligned} \tag{4.40}$$

where to abbreviate

$$X = (1 - x^2) (1 - \kappa^2 x^2) (1 - \lambda^2 x^2) (1 - \mu^2 x^2). \tag{4.41}$$

The coefficients A , B , and C of the quadratic equation are completely determined single-valued functions of the variables u and v , and they have four simultaneous independent periods in respect of these variables (Jacobi 1847, 516–517).

Jacobi, in each of the papers under discussion, also outlined how one could deal with hyperelliptic integrals $\int \frac{dx}{\sqrt{X}}$ where X is of degree $2m - 1$ or $2m$. He indicated that one should invert $m - 1$ sums of m integrals of the form $\int \frac{x^r dx}{\sqrt{X}}$, where $r = 0, 1, \dots, m - 2$, and study the resulting sets of $m - 1$ functions of $m - 1$ variables.

The new functions were eagerly received by mathematicians. The young Hermite did his first, brilliant work in investigating them. The Copenhagen Academy of Sciences offered a prize for their generalisation to the case of integrals of arbitrary algebraic functions, a sign that such an extension was felt to be highly desirable, but no-one entered. However, the prize was later offered by the Berlin Akademie. But the theory of complex integration, being inadequate to establish a satisfactory theory of elliptic functions at this time, fell even further short of upholding a theory of these new complex functions of two variables. The way forward found by Göpel and Rosenhain was to adapt Jacobi's theory of theta functions to deal with the new functions of two variables.¹⁸ Independently, each introduced 16 theta functions of two variables, of which this is the simplest:

$$\theta(u, v) = \sum_{m, n} \exp(a_1 m^2 + a_2 mn + a_3 n^2 + 2mu + 2nv), \tag{4.42}$$

where the sum is taken over all integer values of m and n . The others are obtained by shifting the argument of the variables by a half-period, just as Jacobi had earlier introduced the four theta functions of his theory. The four periods of the functions λ_1 and λ_2 permit one to choose the values of a_1, a_2 , and a_3 in such a way that the λ 's are quotients of theta functions. But the success was achieved at a price: there was simply no theory at all of complex functions of two or more variables. Indeed, the work of Jacobi and his immediate followers was largely formal and algebraic.

There rapidly grew up a formidable technical literature concerned with interrelations between the theta functions themselves, which we pass over, but one more

¹⁸See Göpel (1847) and Rosenhain (1851); described in Brill and Noether (1894, 236–239).

conceptual problem is worth mentioning.¹⁹ It would seem that the generalisation of the theory of theta functions from two to more variables would be obvious enough and such was Göpel's opinion. But Jacobi observed that there is instead a real difficulty. If there are k variables, then the expression $a_1m^2 + 2a_2mn + a_3n^2$ will have to be replaced. To see what should replace it, regard it as the quadratic form $(m, n)^T \begin{pmatrix} a_1 & a_2 \\ a_2 & a_3 \end{pmatrix} (m, n)$. The correct generalisation is then apparent; one wants the expression $(m_1, m_2, \dots, m_k)^T Q (m_1, m_2, \dots, m_k)$, where Q is a $k \times k$ symmetric matrix. But such an expression has $\frac{1}{2}k(k+1)$ coefficients, while the corresponding hyperelliptic functions have only $2k$ periods. So when k is greater than 3 not all matrices will give rise to theta functions, and the question is to characterise those which do. This problem is among the most difficult. It is known today as Schottky's problem because of his discussion of it in 1888 and it lies beyond the scope of this book to consider in detail (for a brief discussion of Schottky's contribution, see Sect. 9.3.2).

The chief significance of the work on hyperelliptic integrals, Jacobi inversion, and Abel's theorem in the first half of the nineteenth century for the history of complex function theory, was to indicate a need for the study of complex functions of several variables. It also ensured that the theory of hyperelliptic integrals could not permanently be left surrounded by the cloud of obscurity Jacobi had found it in. But the difficulties facing any mathematician who took up these challenges were formidable. We shall see in Chapters 5 and 6 how great an influence they were to be on Riemann and Weierstrass. But we conclude this chapter by considering how Jacobi and Carl Neumann exploited a striking connection between theta functions, Jacobi inversion, and problems in dynamics.

4.6 Theta Functions and Mechanics

The problems we shall consider here are well known: the motion of a top; and motion on spheres and ellipses. They turned out not only to be a fruitful ones for the application of elliptic function theory, but to lead directly to insights into the nature of Abelian functions as well. The motion of a top may sound frivolous, but it is not a problem whose only interest is that it can be solved. Tops include gyroscopes, lathes, the Earth itself, and the Moon, all of which raise worthwhile questions in applied mathematics.

As we saw in Sect. 1.1, elliptic integrals had long been established in applications of mathematics. Their recognition in this context goes back at least as far as Newton, who had to sought to apply Kepler's laws to the motion of planets. This led him to grapple with the length of elliptic arcs, and to recognise that they could not

¹⁹See, for example, Brill and Noether (1894, 234–239) and Houzel (1978).

be expressed in terms of known functions.²⁰ In Book I, Sec. 10 of the *Principia*, Newton gave a remarkably insightful discussion of motion on a surface of revolution, another source of elliptic integrals. Throughout the eighteenth century a number of other problems were formulated in such terms: the motion of a body attracted to two fixed points, by Euler and then Lagrange²¹; the motion of a top, again by Euler (1758); and the motion of a spherical pendulum (or, equivalently, motion on a sphere), by Lagrange in his *Mécanique Analytique*. These problems were then dealt with at length by Legendre, who added the motion of a simple pendulum (Legendre 1792), motion under certain central forces obeying laws other than the inverse square law, and motion on a spheroid (both in Legendre 1825–1832, 1).

Our concern here is with the way in which these questions were enriched by the introduction of specifically complex ideas. An elementary, amusing illustration is given by the motion of a simple pendulum. As noted above, in vol. 1 of his *Traité* (1825–1832), Legendre formulated the problem this way. He chose units for which the acceleration due to gravity was 1, let L be the length of the pendulum and H the height it would reach given its speed at its lowest point. He denoted the angle of the pendulum to the vertical by ψ and found by observing that the sum of the kinetic and potential energies was constant that

$$\sqrt{L} \frac{d\psi}{dt} = 2\sqrt{\frac{H}{2L} - \sin^2 \frac{\psi}{2}}, \quad (4.43)$$

from which he deduced that t and ψ are related by the elliptic integral

$$2t = \int \frac{\sqrt{L} d\psi}{\sqrt{\frac{H}{2L} - \sin^2 \frac{\psi}{2}}}. \quad (4.44)$$

Accordingly, he observed, the time for a full oscillation is given by the complete elliptic integral $2\sqrt{LF}^1\left(\sqrt{\frac{2L}{H}}\right)$. Once it was accepted that one should think not of elliptic integrals but elliptic functions, it was open to enquire whether the second, and in this case purely imaginary, period had a dynamical interpretation. This question was raised and solved by Appell in 1878, who showed that it would be obtained by supposing gravity to act upwards.²²

²⁰*Principia*, Book I, Section 6. For a discussion in modern terms, see Arnold (1990, 83–105).

²¹See Euler (1760, 1762b, 1763). Euler showed it was integrable and gave the “general solution as an equation between two elliptic integrals with separated variables” (Wilson 1994, 1054). For Lagrange, see his (1766–1769) and (1815, 101–114). Legendre (1811–1817, 3, §384) observed that the variables must be allowed to go complex if complete branches of the solutions are to be obtained.

²²See Appell (1879) and Whittaker (1904, 4th ed. 1937, 73), where the motion is expressed in terms of the Jacobian elliptic function sn .

The top studied by Euler, Lagrange, and Legendre in vol. 3 of his (1811–1817) rotates about a fixed point, which may or may not be its centre of mass. Equivalently, the top moves freely but is subject to no external forces. On the basis of their work, Poisson formulated the equations of motion of the top and showed that they could be solved in terms of elliptic integrals (Poisson 1811e, 1, 121–160). In particular, the time was expressible as an elliptic integral of the first kind involving the position of the top. In 1834 the main features of the motion of a top were described in simple geometrical terms in Poinsot (1834). For dynamical purposes the top may be replaced by an ellipsoid whose principal axes are equal in length to the moments of inertia of the top about those axes. Poinsot showed that this ellipsoid rolls on a fixed plane, called the invariable plane. The point of contact of the ellipsoid traces out a curve on the ellipsoid called the polhode, and the corresponding curve on the plane is called the herpolhode. In an unforgettable phrase: “the polhode rolls without slipping on the herpolhode in the invariable plane”. A detailed analysis of the rotationally symmetric top shows that the angle its body axis makes with the vertical oscillates between two limiting values, while the axis itself moves with the composite of two circular motions.²³

After the work of Poisson and Poinsot, the challenge was to provide a clear but quantitative analysis of the motion. As with the simple pendulum, so with the top, much interest lay in expressing the position variables as functions of the time, so it was appropriate to invert the elliptic integral. The first to apply the theory of elliptic functions to the motion of such a top was A.S. Rueb in his dissertation at Utrecht in 1834 (Rueb 1834). Klein and Sommerfeld remark that he showed how the polhode and herpolhode could be described by elliptic functions, and Jacobi that Rueb showed how to determine the position of the principal axes as functions of time by the same means.²⁴ Jacobi, in his (1850), preferred to use axes two of which lay in the invariable plane and rotated in a way that accurately reflected one of the circular motions of the body axis of the top.²⁵ He found that the direction cosines of the principal axes of the top at any moment of time with respect to these axes could be expressed as quotients of his theta functions and $H\left(\frac{2Kx}{\pi}\right) = 2\sum_{n=0}^{\infty} (-1)^n 2q^{(2n+1)^2/4} \sin((2n+1)x)$. The connections between the nine direction cosines giving the three columns of an orthogonal matrix translated into familiar formulae connecting elliptic functions. He was pleased to note that the rapid convergence of these series meant that accurate numerical studies were henceforth possible, and, as he had earlier pointed out in a letter to Hermite,²⁶ the formulae he found led easily and directly to the division formulae for elliptic functions.

²³In this case the polhode is a circle, and the angular momentum vector rotates around the body axis; the phenomenon of precession.

²⁴See Klein and Sommerfeld (1898, 2, 473) and Jacobi (1850, 307).

²⁵See Jacobi (1850, 293–294).

²⁶Jacobi to Hermite, 6 August 1845, see Jacobi (1846a).

A different problem in mechanics that led to the use of elliptic and Abelian functions was the study of the motion under gravity of a particle constrained to lie on a surface. This problem had been raised by Newton for surfaces of revolution (Newton *Principia*, Book 1, Sec. 10.) In his *Mechanica* of 1736 Euler had shown that when no forces act the motion is along a geodesic. But even the simplest cases led to complexity. The study of geodesics on an ellipsoid was shown by Jacobi in (1839) to involve Abelian integrals, albeit of the degenerate kind that reduce to elliptic integrals. Inspired by Lamé's work on the study of heat conduction and earlier work of Lambert's on conformal maps of the ellipsoid, Jacobi observed that the coordinates of a point on an ellipsoid could be expressed in a more productive way in an unconventional coordinate system. He spelled out the implications for its use in mechanics in his lectures in dynamics²⁷ of 1842–1843. This led him to a new perspective on Abel's theorem simultaneously outlined in Jacobi (1842).

The coordinate system in question is what are called elliptic coordinates (λ, μ) . As Jacobi explained,²⁸ the equation $\frac{x^2}{a+\lambda} + \frac{y^2}{b+\lambda} = 1$, $0 < a < b$, represents an ellipse when $a + \lambda > 0$ and a hyperbola when $b > -\lambda > a$. For fixed a and b , the equation represents a family of confocal conics as λ varies, and moreover each ellipse cuts every hyperbola at right angles. So a point P of the plane may be represented either by its Cartesian coordinates (x, y) or by the values of λ which select the unique ellipse and hyperbola of the family that meet at P .

In three dimensions the analogous equations define a system of confocal ellipsoids, hyperboloids of one sheet and hyperboloids of two sheets. Moreover, as Binet proved, each surface of one kind cuts those of the other two kinds in their lines of curvature.²⁹ In elliptic coordinates, the element of arc length of a space curve appears as an Abelian integral, and the element of area of a quadric as an expression in elliptic integrals. In this way Jacobi redescribed such problems as the motion of a particle attracted to two fixed points before turning in Lecture 30 to Abel's theorem. Jacobi viewed Abel's theorem as a theorem about a certain system of ordinary differential equations, the theorem supplying two different systems of integral equations of which one is purely transcendental and the other is purely algebraic. Nonetheless, the two systems of integral equations are entirely identical. To prove Abel's theorem, he proposed to go from the ordinary differential equations to a first order partial differential equation, to write down the complete solution of that, and thence to a system of integral equations. The solution of the partial differential equation could, of course, take very different-looking forms, although they must have the same significance.

The details are somewhat forbidding. The first-order non-linear partial differential equation

²⁷Published posthumously as Jacobi (1866).

²⁸See *Ges. Werke. Supplementband*, 207.

²⁹Binet (1811). This theorem was generalised in Dupin (1813).

$$\left(\frac{\partial V}{\partial x_1}\right)^2 + \left(\frac{\partial V}{\partial x_2}\right)^2 + \cdots + \left(\frac{\partial V}{\partial x_n}\right)^2 = 2h, \quad (4.45)$$

where h is a constant, is solved by passing to a system of linear ordinary differential equations which in this case reduce to

$$\frac{d^2 x_1}{dt^2} = 0, \frac{d^2 x_2}{dt^2} = 0, \dots, \frac{d^2 x_n}{dt^2} = 0, \quad (4.46)$$

whose solutions are simply the lines $x_i(t) = a_i t + \text{const}$. By passing to elliptic coordinates and separating variables by the method of partial fractions, as Lagrange had explained, a system of $n - 1$ ordinary differential equations is obtained, each being a sum of n terms of the form $\sum_j \frac{y_j^k dy_j}{\sqrt{f(y_j)}} = 0$, $k = 0, 1, \dots, n - 2$, and f is a polynomial of degree $2n$ in the variables $x_1, \dots, x_n$, and there is a further equation $\sum_i \frac{y_i^{n-1} dy_i}{\sqrt{f(y_i)}} = 4dt$.

As Jacobi pointed out, this is a system of differential equations equivalent to Abel's theorem, indeed, in the original form that Abel had presented it. The solutions give algebraic relationships between the transcendental functions defined by the integrals.³⁰

In his paper (Jacobi 1846b) Jacobi pursued the analogy between the system consisting of the first $n - 1$ equations above and the system consisting of the first alone, which is

$$\frac{dx_1}{\sqrt{f(x_1)}} + \frac{dx_2}{\sqrt{f(x_2)}} = 0, \quad (4.47)$$

and which, of course, yields Euler's differential equation for elliptic functions. Jacobi observed that Euler's equation has solutions x_1 and x_2 which satisfy a second order algebraic equation in $x_1 + x_2$ and $x_1 x_2$. Jacobi found that his system of $n - 1$ equations had solutions $x_1, \dots, x_n$ that satisfy a second order equation involving the first $n - 2$ symmetric functions of the x 's. He argued that the function $f(x)$ could always be written in the form

$$f(x) = (bx^n + b_1 x^{n-1} + \cdots + b_n)^2 + (cx^n + c_1 x^{n-1} + \cdots + c_n)^2 - (ax^n + a_1 x^{n-1} + \cdots + a_n)^2. \quad (4.48)$$

On the other hand, straight-forward calculation showed that when $x_1, \dots, x_n$ were the roots of an equation of the form

³⁰Jacobi pointed out that it was in this way that Lagrange had shown that the problem of attraction to two fixed centres led to a proof of the fundamental theorem for elliptic functions (corresponding to the case $n = 2$ here); see Lagrange (1766–1769).

$$(bx^n + b_1x^{n-1} + \cdots + b_n)^2 \cos \phi + (cx^n + c_1x^{n-1} + \cdots + c_n)^2 \sin \phi = (ax^n + a_1x^{n-1} + \cdots + a_n)^2 \quad (4.49)$$

for some angle ϕ , then they satisfied the original system of differential equations. Since every equation of the original type could be reduced to one of that form, he deduced his conclusion.

In two papers in 1842 and 1843 Richelot took up the theme. In [Richelot \(1842\)](#) he generalised Jacobi's treatment of the original differential equation $\frac{dx}{\sqrt{f_4(x)}} = \frac{dy}{\sqrt{f_4(y)}}$, where f_4 is a quartic polynomial in the indicated variable, to a system of differential equations. He first treated the simplest hyperelliptic case, where f_6 denotes a polynomial of degree 6, and the equations are:

$$\frac{dx}{\sqrt{f_6(x)}} + \frac{dy}{\sqrt{f_6(y)}} + \frac{dz}{\sqrt{f_6(z)}} = 0, \quad \frac{xdx}{\sqrt{f_6(x)}} + \frac{ydy}{\sqrt{f_6(y)}} + \frac{zdz}{\sqrt{f_6(z)}} = 0.$$

Here he found that the solutions of these equations are connected by two algebraic equations. He then found analogously in the n th order case that the corresponding system of $n - 1$ differential equations (where f_6 has been replaced by a polynomial of degree $2n$) are connected by a system of $n - 1$ algebraic equations. These are the differential equations that Jacobi had studied in his paper ([Jacobi 1832b](#)) and which had led him to Abel's theorem.

In his next paper ([Richelot 1843](#)) Richelot observed that Jacobi has solved these differential equations by passing to a certain partial differential equation. This method led to imaginary functions if the polynomial equation $f_n(x) = 0$ had imaginary roots, and so he now offered a method for avoiding these imaginary functions. More importantly, he said, he hoped in this way to give a most useful extension to Jacobi's method for solving differential equations and to illuminate the connection to Abel's theorem in the hyperelliptic case. Richelot then showed indeed that Jacobi's methods did lead directly to an expression which encapsulated the result known as Abel's theorem. The details are again forbidding and we omit them. It is more important here to notice that whatever Richelot had established about Abel's theorem he had not illuminated the corresponding hyperelliptic functions. That task was unsolved when Riemann and Weierstrass began their work.

In his paper (1859), Carl Neumann advanced the study of the hyperelliptic case while pursuing a problem in mechanics. Although published after the great papers of Riemann and Weierstrass it is best seen as advancing the older tradition of Göpel, Rosenhain, and Richelot, and so we mention it here.

Neumann was interested in the problem of the motion of a particle on the unit sphere $x^2 + y^2 + z^2 = 1$ subject to a potential of the form $ax^2 + by^2 + cz^2$, where a , b , and c are constants. "Let us have before our eyes", he wrote, "the example of the attraction of a homogeneous ellipsoid to an internal point". He introduced

coordinates³¹ λ_1 and λ_2 on the sphere by considering the point as lying on the sphere and the two ellipsoids with equations

$$\frac{x^2}{\lambda_1 - a} + \frac{y^2}{\lambda_1 - b} + \frac{z^2}{\lambda_1 - c} = 0 \quad \text{and} \quad \frac{x^2}{\lambda_2 - a} + \frac{y^2}{\lambda_2 - b} + \frac{z^2}{\lambda_2 - c} = 0$$

and derived the equations of motion of the point with respect to these coordinates. He then showed that these equations were solved by means of hyperelliptic integrals in this fashion³²:

$$\begin{aligned} \varepsilon_1 \int_{l_1}^{\lambda_1} \frac{d\lambda}{L_1} + \varepsilon_2 \int_{l_2}^{\lambda_2} \frac{d\lambda}{L_2} &= 0 \\ \varepsilon_1 \int_{l_1}^{\lambda_1} \frac{\lambda d\lambda}{L_1} + \varepsilon_2 \int_{l_2}^{\lambda_2} \frac{\lambda d\lambda}{L_2} &= t. \end{aligned} \tag{4.50}$$

Here the ε 's are ± 1 , $L_i = \sqrt{8(\lambda_i - a)(\lambda_i - b)(\lambda_i - c)(\lambda_i - A)(\lambda_i - B)}$, $i = 1, 2$, and A and B are arbitrary constants. He then showed how these equations could be solved explicitly in terms of the theta functions of two variables that Rosenhain had introduced.

4.7 Theta Functions and Number Theory

The connection between Gauss's theory of numbers and complex function theory was established through a series of remarkable discoveries which seem to have raised the esteem of both subjects. Two of these were of particular historical importance. Gauss's fourth proof of quadratic reciprocity, which was based on an analysis of Gauss sums, rested at a crucial point on the transformation of theta functions. The result was then re-derived by a number of people in various ways before Kronecker employed complex function theory to show how the different proofs were related. Kronecker's proof used the Cauchy integral theorem to evaluate an infinite sum; other, deeper methods were later introduced by Weber and Lindelöf. The second area was the connection between quadratic reciprocity and the class number of quadratic forms. This was first conjectured in 1832 by Jacobi and eventually proved by Dirichlet in (1839, 1840). In so doing he was led to establish his famous theorem on the existence of primes in arithmetic progressions. The difficult part of the proof led him, to his surprise, back to the class number. An analysis of each of these investigations shows how complex analysis came to establish itself as the natural tool with which to investigate what, in German universities at least, was coming to be regarded as the paradigm topic in pure mathematics.

³¹They have an ambiguous sign factor that Neumann kept track of.

³²Neumann's notation is ambiguous and this expression is our interpretation of what he meant.

4.7.1 Quadratic Reciprocity

Gauss's fourth proof of quadratic reciprocity, based on Gauss sums, was closely connected to (unpublished) Gaussian material on the transformation of theta functions. Here, following the account in [Ireland and Rosen \(1982\)](#), we introduce some notation. We define $\zeta = e^{2\pi i/p}$, a primitive p th root of unity, and a (quadratic)

Gauss sum $g_a = \sum_{n=0}^{p-1} \zeta^{an^2} = \sum_{t=0}^{p-1} \left(\frac{t}{p}\right) \zeta^{at}$ where we denote the Legendre symbol by $\left(\frac{a}{p}\right)$. It is easy to show (see [Ireland and Rosen 1982](#), 71) that $g_a = \left(\frac{a}{p}\right) g_1$, and so, if $a \not\equiv 0 \pmod{p}$, $g_a^2 = g_1^2$. The common value of this squared term is $g_1^2 = (-1)^{(p-1)/2} p$.

The deep and difficult question is to determine the sign of g_1 . Gauss recorded in his diary in May 1801 (entry number 118)³³

$$g_1 = +\sqrt{p} \text{ if } p \equiv +1 \pmod{4}, \text{ and}$$

$$g_1 = -\sqrt{p} \text{ if } p \equiv -1 \pmod{4}.$$

But a proof eluded him for more than four years, even though he told his friend Olbers that he had tried almost every week to discover one, until finally “As lightning strikes was the puzzle solved”.³⁴

His proof was based on some formal manipulation with series. It will not be described, because it makes no use of complex analysis.³⁵ But the connection to the theory of theta functions is worth describing, since Gauss seems to have noticed it, and it was indeed taken up explicitly by later writers, such as Hermite.³⁶ Because the mathematics is rather delicate, we first explain the mathematical connection. It is required to prove that

$$\sum_{n=0}^{p-1} e^{2\pi i n^2/p} = \sqrt{\left(\frac{-1}{p}\right) p} \quad (4.51)$$

with positive or positive imaginary square root. The crucial observation is that

$$\sum_{m \in \mathbb{Z}} e^{\pi i \tau m^2} = \sum_{a=0}^{p-1} \zeta^{a^2} \sum_{m \equiv a \pmod{p}} e^{-\pi m^2 w^2}, \quad (4.52)$$

where $\tau = \frac{2}{p} + iw^2$.

³³See Gauss *Werke* 10.1, 560–561 or also Dunnington (2004, 481).

³⁴See Gauss *Werke* 10.1, 25.

³⁵A summary will be found in [Berndt and Evans \(1981\)](#).

³⁶Gauss's unpublished remarks are analysed by [Schlesinger \(1912, 122–3\)](#). See also Hermite, *Oeuvres* 1, 486.

This relates the object under study to the object defined on the left, which is a theta function in z , evaluated at $z = 0$. The trick is to evaluate both sides in a certain limit obtained as w tends to zero. The details of the limiting argument are quite subtle, and we have suppressed them; the point is that by suitably choosing the path along which w tends to 0 the sign of the Gauss sum is obtained.³⁷ Then to evaluate the limit crucial use is made of the transformation formula for theta functions, $\theta\left(\frac{1}{z}\right) = \sqrt{z}\theta(z)$, where $\theta(z) = \sum_{m \in \mathbb{Z}} e^{-\pi m^2 z}$, in the form

$$\sum_{m \in \mathbb{Z}} e^{\pi m^2 i t} = \frac{1}{\sqrt{-it}} \sum_{m \in \mathbb{Z}} e^{-\pi m^2 i/t} \quad (4.53)$$

so indeed the theory of Gauss sums and theta functions are deeply connected.

The proof Gauss published was eventually discussed by others, not least because its elementary character seems to have suggested that much lay hidden beneath its formal surface. Dirichlet derived it by means of Poisson summation in 1835 (Dirichlet 1835), and in 1840 Cauchy gave a proof that used the transformation formula for theta functions.³⁸ Smith in his report of 1863 compared Eisenstein's proofs by cyclotomy with those using lemniscatic functions. The first proofs that invoked complex analysis were given by Kronecker in 1889 and Weber as late as 1903 (on the occasion of the centenary of Abel's birth). Weber used the method of Abel–Plana summation, and his approach was adopted by Lindelöf in his important book on complex function theory (Lindelöf 1905, 73–75).

Kronecker took the opportunity to use complex function theory to explain how the various known proofs were related.³⁹ He observed that Jacobi's proof of the transformation formula used the same means as Dirichlet had to evaluate the Gauss sum, so Cauchy's and Dirichlet's proofs had the same source. But he said, one could just as well use the theory of Gauss sums to derive the transformation formula for theta functions, so "it is worthwhile for the development of mathematical knowledge to put Cauchy's approach in a clear light" (Kronecker 1880, 278). Accordingly Kronecker gave a Green's theorem approach to the proof of the Cauchy integral theorem. He then applied that theorem to the function $F(z) = \sum u \log(z)^2$, where $|u| > 1$ and the summation is over all values of $\log z$. He wrote

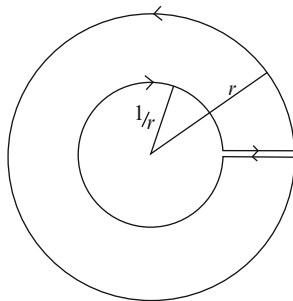
$$F(z) = \frac{1}{2\pi i} \int \frac{F(\zeta) d\zeta}{\zeta - z}, \quad (4.54)$$

³⁷They can be found in Schoeneberg (1974, 220).

³⁸See Cauchy (1840b). Cauchy published this paper twice, first in the *Comptes rendus* of the Académie and then in the *Journal de mathématiques*.

³⁹See Kronecker (1880). This proof was flawed, and in Kronecker (1889) he gave a better proof. See also the account in Landau (1958, 203–207).

Fig. 4.2 Kronecker's path of integration (drawn by Jeremy Gray)



where the integration is taken over the circle of radius $r > 1$ in the usual direction and over the circle of radius $1/r$ in the opposite direction (Fig. 4.2).

He deduced that the coefficients of z^n and z^{-n} in the power series expansion of $F(z)$ were

$$\frac{1}{2\pi i} v^{-n^2\pi} \int F(\zeta) d\log \zeta = v^{-n^2\pi} \int_{-\infty}^{\infty} u^{-4\pi^2 w^2} dw, \quad (4.55)$$

where the first integral is taken around the circle $|\zeta| = 1$, and the relation between u and v is $4\pi \log u \cdot \log v = 1$. This begins to make the connection with the Gauss sum clear. A limiting argument then enabled Kronecker to derive the sign of the sum. In his view Cauchy's approach made the sign of the Gauss sum natural and evident, and he made a comparison with the integral

$$\int_{-\infty}^{\infty} e^{-u^2\pi/a} du = +\sqrt{a}, \quad (4.56)$$

where a was a complex number with positive real part, arguing that the sign was clear in one place and therefore clear in the limit. The insight is not impaired by the fact that Kronecker's argument was flawed by a loose exchange of limits; this was put right much later by Hardy, Littlewood, and Mordell.⁴⁰

4.7.2 The Class Number

Another object of great interest was the number discovered by Lagrange⁴¹: the class number $h(\Delta)$, which is the number of inequivalent forms $ax^2 + 2bxy + cy^2$ with a given discriminant ($\Delta = b^2 - ac$). By definition, two forms, one in (x, y) the other in

⁴⁰See, e.g. Mordell (1918) and Landau (1958).

⁴¹See Lagrange (1775–1777).

(x', y') are said to be (properly) equivalent if and only if one is obtainable from the other by a transformation of the form

$$\begin{pmatrix} x' \\ y' \end{pmatrix} = \begin{pmatrix} \alpha & \beta \\ \gamma & \delta \end{pmatrix} \begin{pmatrix} x \\ y \end{pmatrix},$$

where $\begin{pmatrix} \alpha & \beta \\ \gamma & \delta \end{pmatrix}$ is a matrix with integer entries and determinant $+1$. (If the matrix has determinant -1 it establishes an improper equivalence.) Equivalent forms obviously represent the same set of numbers, but the converse is false: the quadratic forms $3x^2 + 2xy + 4y^2$ and $3x^2 - 2xy + 4y^2$ represent the same set of numbers $\{3, 4, 5, 9, 12, \dots\}$ but they are not equivalent.⁴² They also have the same discriminant, -11 , which establishes that $h(-11) \geq 2$ and in fact $h(-11) = 4$.

Two questions about the function $h(\Delta)$ stand out: given Δ , is there any way of finding $h(\Delta)$? and, conversely, given an integer, h , how many systems of forms are there with whatever discriminant Δ such that $h(\Delta) = h$? The latter question is much harder, as one would expect. Gauss conjectured, on the basis of his customary thorough look at the experimental evidence, that the set of discriminants with a given value h is always finite, and proposed specifically that the largest for which $h(\Delta) = 1$ is 163, for which $h(\Delta) = 2$ is 427, and that for which $h(\Delta) = 3$ is 907. Because proofs of these assertions were only found quite recently,⁴³ we confine our attention on historical grounds to the first question: the determination of the class number, $h(\Delta)$.

The connection between questions about quadratic reciprocity and the class number is particularly striking. The first insight was gained by Jacobi in 1832, when he conjectured that when q is a prime of the form $4k - 1$,

$$h(-q) = -\frac{1}{|\Delta|} \sum_{m=1}^{q-1} m \left(\frac{-q}{m} \right) = -\frac{1}{q} \sum_{m=1}^{q-1} m \left(\frac{m}{q} \right). \quad (4.57)$$

This quantity is an integer, H say, for Euler had shown that

$$\frac{1}{q} \sum_{m=1}^{q-1} m \left(\frac{m}{q} \right) \equiv \sum_{m=1}^{q-1} m^{(q+1/2)} \equiv 0 \pmod{q}. \quad (4.58)$$

Jacobi was led to this conjecture because he could show at the end of a complicated argument involving Gauss sums that for every prime $p \equiv 1 \pmod{q}$, $p^{|H|}$ can be written in the form $x^2 + 4y^2$. It follows from the elementary part of

⁴²Good introductions to this material are [Cox \(1989\)](#) and [Goldstein et al. \(2007\)](#).

⁴³See [Heegner \(1952\)](#) for a treatment of $h = 1$ and for entirely rigorous accounts ([Baker 1966](#)) and ([Stark 1967](#)); for $h = 2$ see [Baker \(1971\)](#) and [Stark \(1971\)](#); for $h = 3$ see [Gross and Zagier \(1983\)](#). In each case, Gauss's conjecture was affirmed. For an informative and historical account of these developments, see [Goldfeld \(1985\)](#) and [Zagier \(1984\)](#).

the theory of quadratic forms that the same statement is true with $|H|$ replaced by $h(-q)$. Jacobi then looked at several particular cases before conjecturing that $h(-q) = |H|$. His investigations were purely empirical, and in his obituary of Jacobi Dirichlet hailed them as “a remarkable example of astute induction, even though a rigorous proof cannot be based on cyclotomy but rather seems to require essentially different principles involving the integral calculus and power series, which were only introduced into the subject later”.⁴⁴

It is in this context, and with this evidence to go on, that in 1837 Dirichlet not only gave a formula for all class numbers but also proved the lemma Legendre had perforce assumed in his proof of quadratic reciprocity.⁴⁵ The lemma states that in every arithmetic progression $a + kb$ with a prime to b there are infinitely many primes. Dirichlet proved his theorem in two stages, the first when b is itself a prime p , and the second when b is not. He took his inspiration from Euler’s proof (1748a, 1, Chap. 15) that there are infinitely many primes. Euler had considered the infinite product

$$\prod (1 - p^{-s})^{-1} = \sum n^{-s}, \quad s > 1, \quad (4.59)$$

where its expression as an infinite sum is an immediate consequence of the factorisation theorem for integers. Taking the logarithm of both sides gave him

$$\begin{aligned} -\sum \log(1 - p^{-s}) &= \log\left(\sum n^{-s}\right) = \\ \sum p^{-s} + \frac{1}{2} \sum p^{-2s} + \frac{1}{3} \sum p^{-3s} + \dots \end{aligned} \quad (4.60)$$

Now if $s = 1$, then $\sum n^{-s} = \sum \frac{1}{n}$ and both it and its logarithm are infinite. On the other hand not only are the remaining terms $\sum p^{-2}$, $\sum p^{-3}$, $\dots$ all finite but their sum is less than $\sum p^{-2} (1 + \frac{1}{2} + \frac{1}{4} + \dots)$ and so finite as well. Consequently $\sum p^{-1}$ must be infinite, and so in particular there must be infinitely many primes.

Dirichlet mimicked this proof, but just for the primes in a given arithmetic progression, by considering (in the case when $b = p$) the $p - 1$ equations

$$\prod_{q \neq p} (1 - \omega^{\gamma} q^{-s})^{-1} = \sum \frac{\omega^{\gamma}}{n^s} = L_{\omega}(s) \quad (4.61)$$

where ω is to be any primitive root of $\omega^{p-1} = 1$. The different values of $L_{\omega}(s)$, are all powers of one value, which Dirichlet denoted Ω . Describing the proof a few years later, in 1841, he found it remarkable not only that it was not arithmetic in nature but analytic, but also that once you had embarked on the proof it “seems to

⁴⁴See [Dirichlet \(1852, 241\)](#), in his *Math. Werke* 2, quote on p. 241. Dirichlet’s paper on the class number formula is his (1839, 1840). A lucid and informative account of Dirichlet’s proof of the class number formula will be found in [Scharlau and Opolka \(1985, 121–143\)](#).

⁴⁵See [Dirichlet \(1837b\)](#). There are many good accounts in the literature, e.g. [Apostol \(1976\)](#), [Davenport \(1967\)](#), [Serre \(1973\)](#).

take shape with the greatest ease and, so to speak, entirely of its own accord ...” (Dirichlet 1841a, 503–504). Until, that is, when b is compound and he encountered a nearly insurmountable obstacle at the end, which was only able to overcome after many fruitless attempts. The problem was to show that a certain infinite sequence which could easily be shown to converge, converged to a non-zero value. His earlier rigorous proof scarcely satisfied him, he wrote, because it was so complicated and indirect, and he looked long and hard for a proof more in accordance with the nature of the thing. Finally he found such a proof, which led him to the unexpected result that his original problem was connected with the determination of the class number. Indeed, the sought-for limiting value could not be zero because it appeared as a factor of the class number which, trivially, is not zero.

Dirichlet first showed that, for each $\omega \neq 1$, $L_\omega(s)$ converged as an infinite sum, and indeed that it converged absolutely provided $s > 1$. He distinguished very carefully between the convergence of a series when the sum depends on the order of the terms and when it does not. Then he investigated the limit of the sum as $s \rightarrow 1$. When $s = 1$, the corresponding limit (L_0 in his notation) is infinite, but in the remaining $p - 2$ cases the limits $L_\omega(1)$ are finite. However, Dirichlet, like Euler, needed $\log L_\omega(1)$ to be finite, so $L_\omega(1)$ had to be non-zero. This turned out to be straight-forward enough provided $\omega \neq -1$ and when $\omega = -1$ considerations of quadratic Gauss sums which Gauss had recently published (and Dirichlet reproved in connection with biquadratic reciprocity) enabled Dirichlet to calculate the limit explicitly. It depends on the numbers of residues and non-residues mod p in $0, \dots, p - 1$, and was visibly non-zero.

For Dirichlet, to prove that every arithmetic progression of the form $\{a + kp : p \text{ a prime not dividing } a\}$ has infinitely many primes was now easy. He showed that

$$\sum \frac{1}{q^{1+p}} + \frac{1}{2} \sum \frac{1}{q^{2+2p}} + \dots \quad (4.62)$$

where the first sum is over all primes q in the arithmetic progression, the second over all primes whose squares are of the form $a + kp$, the third over all primes whose cubes are of that form, and so on, is a sum of terms involving powers of Ω and the logarithms of the L_i . Because the limits as ρ tends to 0 of $\log L_1, \dots, \log L_{p-2}$ are all finite but $\lim \log L_0$ is infinite, the right-hand side becomes infinite as ρ tends to 0. So the left-hand side must also become infinite and, exactly as in the Eulerian case, the sum $\sum \frac{1}{q}$ must therefore be over infinitely many terms. It was in extending this proof to the case of the general arithmetic progression $\{a + kb\}$, where a and b are relatively prime but b is not necessarily a prime, that Dirichlet encountered the obstinate difficulty referred to above, and which he resolved on passing to the more general setting of determining the class number. In a further paper Dirichlet (1841b) extended his results to encompass complex primes in arithmetic progressions of complex integers.

From our stand point, all this work has an ambiguous character. It is certainly about complex numbers, but while it involves complex functions (the lemniscatic functions and the Gauss sums) it does not invoke any complex function theory.

Dirichlet's work on the class number formula and his introduction of the L functions represents the first step in that direction, but the variable remained real. Nonetheless, it was clear to the experts by 1840 that important results in number theory led one into the newly discovered complex elliptic functions.⁴⁶

Appendix. Gauss on Higher Reciprocity Laws

In 1805 Gauss had turned to the theory of cubic and biquadratic reciprocity (as questions about third and fourth powers are called) and soon realised that to resolve them it was necessary to use a wider class of integers. "Theorems about biquadratic residues only attain their true simplicity and natural beauty", he wrote "when the field of arithmetic is also extended to include the imaginary integers" (Gauss 1832, §30). Thus forced to explain what he understood by a complex number, he gave the account we described above (see Sect. 1.5.3). He then introduced the Gaussian integers, numbers of the form $m + ni$ where m and n are (ordinary) integers.

Before entering the details, one example will illustrate the nature of the connections Gauss was able to make between biquadratic reciprocity and his theory of elliptic functions. It is his famous final entry to the mathematical diary he kept, made on 9 July 1814.⁴⁷

I have made by induction the most important observation that connects the theory of biquadratic residues most elegantly with the lemniscatic functions. Suppose $a + bi$ is a prime number, $a - 1 + bi$ divisible by $2 + 2i$, then the number of all solutions to the congruence $1 \equiv xx + yy + xxyy \pmod{a + bi}$ including $x = \infty, y = \pm i, x = \pm 1, y = \infty$ is $= (a - 1)^2 + bb$.

Bachmann, in his edition of Gauss's diary, commented⁴⁸ that the connection remains to be cleared up, but in fact, as Weil pointed out, the substitution $z = y(1 + x)^2$ reduces the curve to $z^2 = 1 - x^4$, which makes the connection plain (see Weil (1974, 298)). Taken $\pmod{p}$ it becomes a question about biquadratic residues, and via the study of expressions of the form $ax^4 - by^4 \pmod{p}$ it is connected to the theory of higher Gauss sums. It is interesting to see that Gauss was thinking projectively and counting points at infinity. The projective equation of the curve is $z^2y^2 = y^4 - x^4$. The closely related curve $z^4 = 1 - y^4$, which projectively is $z^4 = x^4 - y^4$, was already known to Fermat to possess no integer points. Since it was studying this entry in Gauss's diary that led Weil to propose his celebrated conjectures, there can be few more important curves in the history of number theory.

⁴⁶Landau's function-theoretic proof that the L series do not vanish when $s = 1$ is much later (Landau 1910). He defined $\zeta_m(s) = \Pi_{\omega} L_{\omega}(s)$. The function $L_1(s)$ has a simple pole at $s = 1$. If any $L_{\omega}(s)$ were zero, $\zeta_m(s)$ would be holomorphic at $s = 1$, but this contradicts other conclusions that can be reached using the theory of Dirichlet series.

⁴⁷See Gauss *Werke* (10.1, 571) or also Dunnington (2004, 484).

⁴⁸See Gauss *Werke* (10.1, 572).

When Gauss came to publish his ideas about biquadratic reciprocity, he explained carefully what was meant by a prime in this context, and a unit (the numbers $1, -1, i$, and $-i$ that must be excluded from a theory of prime factorisation). The ordinary prime 2 must be handled separately, because $2 = (1+i)(1-i)$. Ordinary odd primes remain prime as Gaussian integers if and only if they are of the form $4n+1$. (The “only if” is easy: a prime p of the form $4n+1$ is the sum of two squares, so if $p = a^2 + b^2$ then it factors as $p = (a+bi)(a-bi)$; so, for example, $13 = 9^2 + 2^2 = (3+2i)(3-2i)$ and is no longer prime as a Gaussian integer). He showed that it was possible to do arithmetic modulo a Gaussian prime. To generalise the law of quadratic reciprocity, Gauss saw that the residue symbol for p with respect to q , which he called the character, would now take four values (the four units). We shall denote the character of $p \pmod q$ by $\chi_q(p)$. Gauss then stated, but did not prove, this law of biquadratic reciprocity: given two relatively prime Gaussian integers $p = m + ni$ and $q = m' + n'i$, they have the same character with respect to each other unless both are congruent to $3 + 2i \pmod 4$ in which case their characters with respect to each other are equal but opposite: $\chi_q(p) = -\chi_p(q)$.

Chapter 5

Riemann's Geometric Function Theory

Previous methods of treating these functions always based the definition of the function on an expression that yields its value for each value of the argument. Our study shows that, because of the general nature of a function of a complex variable, a part of the determination through a definition of this kind yields the rest.

Riemann

5.1 Introduction

Riemann's entry takes us from France to Germany. Riemann is one of the three creators of the theory of complex functions (Fig 5.1), but he wrote relatively little. Unusually for a major mathematician, his *Werke*, even augmented as they are by notes from lecture courses and papers left unpublished at his death, fill only a single volume. Each of his papers on complex function theory extended the subject greatly and then applied it to solve a substantial problem. As a result his successors found themselves with much to do, yet it can be argued that the profundity of his best ideas hindered their immediate reception, and it was to be a generation before those ideas were finally taken up.

Riemann's importance is not illuminated by knowing his biography, which has an independent interest, well conveyed in Laugwitz's account (Laugwitz 1999). It is the intellectual connections Riemann made and the depth to which he saw that marks his great contribution, and we shall discuss these below. But his personal life was quiet. He was the archetype of the shy mathematician, not much drawn to topics other than mathematics, physics, and philosophy, devout in his religion, conventional in his tastes, close to his family and awkward outside them. As a child, he was taught by his father, a pastor, and then for some years at school before going to Göttingen University. There he had initially intended to study theology, in accordance with his father's wishes—Göttingen was the only university in Riemann's native Hannover

Fig. 5.1 Bernhard Riemann
(from his *Gesammelte
Mathematische
Abhandlungen*, 3rd ed.
Springer ©1990,
frontispiece)



with strong links to the Hannover church—but his remarkable ability at mathematics led him to switch subjects.

However, the level of mathematical education in Göttingen was not particularly high, and in accordance with the German freedom to study anywhere Riemann left Göttingen for Berlin in 1847, where he spent two years learning from Dirichlet about potential theory and partial differential equations, number theory and theory of integration. He also attended Jacobi's lectures on analytical mechanics and higher algebra and Eisenstein's lectures on elliptic functions. Each of these connections was to prove important for Riemann, and we shall discuss them further below. The most important and lasting influence was that of Dirichlet, while that of Eisenstein was more of an instructive contrast of styles. It is probably with Eisenstein that Riemann began to study Cauchy's ideas on complex function theory, although he had begun to read Cauchy's *Cours d'analyse*, the *Calcul différentiel*, and the *Exercices de mathématiques* already in Göttingen in 1846/47.¹ Finally, the influence of Jacobi's lectures might well have been to stimulate Riemann's tendency to think in an abstract and sophisticated way about the relation of mathematics to physics and the real world (see [Jacobi 1996](#)).

But Berlin was a bustling capital, and in other ways Göttingen was the ideal choice for Riemann. He returned there in 1849, concerned that threats made to students who had participated in the political activities of 1848—the year of revolutions—might result in him being banished from attending university; he had been briefly involved himself. Back in Göttingen he attended Wilhelm Weber's lectures of mathematical physics and for a while he devoted himself to studies

¹See [Neuenschwander \(1981b, 91\)](#), citing Library records.

in physics and Naturphilosophie (see [Bottazzini and Tazzioli 1995](#)). This, as we shall argue below, influenced his way of dealing with complex functions. There he soon met Richard Dedekind, who was 5 years younger, and who was also a truly conceptual thinker, but unlike Riemann one who was to move slowly and steadily throughout a long life. In 1855 matters improved greatly for Riemann, because Dirichlet was called there as the senior professor after the death of Gauss, who for some years had been a real but distant presence in Göttingen. Socially there was something of a strain between the sociable and highly musical Dedekind and Dirichlet, who was married to a sister of Mendelssohn-Bartholdy, on the one hand, and the gauche Riemann. But mathematically, Dirichlet, who may be regarded as the man who brought rigorous mathematics to Germany, if not indeed to mathematicians everywhere, was the ideal abstract thinker to guide Riemann, and the young man remained very grateful to him. His influence is visible in Riemann's work, not just the published papers but the lectures he began to give once he was working for his Habilitation. These covered definite integrals, partial differential equations with applications to physics, and his new approach to complex function theory. These highly interesting lectures were sometimes written up for publication after Riemann's death, although one notes that they were probably not much understood by their original, and very small, audience; von Bezold, an important source of lecture notes, admitted he found them incomprehensible.² Ernst Abbe, a devoted student of Riemann's who came to Göttingen expressly to hear his lectures, wrote in May 1861 that "one could see his tiredness and lassitude, his thoughts frequently failed him and he was unable to explain the simplest things".³

Dirichlet died in 1859 and Riemann was very quickly appointed his successor (the more charismatic figure of Dedekind being by then a professor in Zürich). He still attracted only a few students, and one wonders how matters might have developed had he not contracted pleurisy in July 1862, almost immediately after his marriage in June of that year to Elise Koch, a friend of his sisters. The illness led to a permanent weakening of his lungs, and he was advised to spend as much time as possible in the South. He accordingly spent as much time as he could in Italy, living on a government grant secured for him by Gauss's old friends Wilhelm Weber and Sartorius von Waltershausen, but his health deteriorated and on 20 July 1866 he died near Lake Maggiore, where he is buried. He was only 39. He left behind a number of published papers, several more in a good enough state to be published, and yet more that could be edited and printed in the first edition of his *Werke* (edited by Dedekind and Heinrich Weber in 1876). He exerted an indirect influence through his few students, and the publication of his lecture courses as books at various dates. Not only are they an important source for historians seeking to understand

²On von Bezold, see Laugwitz (1999, 32). He told Fuchs of his notes, in Gabelsberger shorthand, when they were colleagues in Berlin in the 1890s and Fuchs arranged for them to be transcribed into ordinary German script.

³Quoted in Laugwitz (1999, 32), originally published in [Neuenschwander \(1987, 9\)](#).

the development of his ideas; in their day they were useful to his contemporaries seeking to augment the little Riemann published.

The first of his publications was the privately distributed doctoral thesis (1851) on the foundations of a theory of functions of a complex variable. Here the idea of a “Riemann surface” was presented for the first time, and the paper concludes with a remarkable theorem about complex functions (the Riemann mapping theorem) that implies the equivalence of all simply connected domains for the purposes of complex function theory. The thesis was accepted and his defence successfully conducted in December 1851. In 1854 he successfully presented his *Habilitationschrift* and gave his *Habilitationsvortrag* (or lecture).⁴ He was now able to take up his chosen career as a mathematician. In 1857 Riemann published two remarkable papers involving complex function theory: one on the hypergeometric function and one on Abelian functions. These seem for a while to have exhausted him mentally. Dedekind wrote to his sister that Riemann had become extremely hypochondriacal and mistrustful of other people and himself.⁵ Another remarkable paper, on analytic number theory, followed in 1859. In a short paper of 1862 he added to his theory of Abelian functions, and among the papers edited for publication shortly after his death is another fine one, showing how analytic functions enter the study of minimal surfaces in differential geometry. There is a further short paper on theta functions (1865) and a posthumous work on multiply connected domains. But that is all he wrote about complex function theory, apart from his various lecture courses.

Riemann is a prime example, with Galois, of the turn towards conceptual thinking and away from prodigious calculation that characterises the nineteenth century, and it was his conceptual novelties that made his work hard to accept. He was deeply involved for a time in studying the philosophy of Herbart⁶ and worked enthusiastically in experimental physics, where Wilhelm Weber exerted a life-long influence upon him, even though Riemann’s ideas in physics on action at a distance became distinctly unWeberian.⁷ In 1851–1852 he worked with Listing and others in Weber’s laboratory determining the magnetic moment of bismuth bars, part of a series of experiments aimed at providing a quantitative theory of diamagnetic polarity (see Darrigol 2000, 106). This involvement in both philosophy and physics enabled him to reformulate the concept of a mathematical quantity. He came to argue that mathematics is about “ n -fold extended quantities”⁸ (“ n -tuples of real numbers” is the equally unattractive modern term) to which is added

⁴The paper (1854a) was on trigonometric series, the accompanying lecture (1854b) on the foundations of geometry, both were published only in 1867.

⁵Quoted in Laugwitz (1999, 29), who noted that the information was omitted from Dedekind’s later biography of Riemann, probably out of respect for Riemann’s widow Elise. He also speculated that Riemann’s depression seems to have persisted to the end of his life.

⁶For the influence of Herbart on Riemann’s philosophical ideas, see Scholz (1982).

⁷For a discussion of Riemann’s physics, see Bottazzini and Tazzioli (1995); the unWeberian paper is Riemann’s posthumous paper on electrodynamics, Riemann (1858).

⁸*n-fach ausgedehnte Grösse*, see Riemann (1854b, Sect. 1).

some appropriate extra structure. In particular, complex numbers are pairs of real numbers; the extra structure provided by complex differentiability and the Cauchy–Riemann equations enabled him to define analytic functions. This new metaphysical basis for mathematics marks a break with the old theory of magnitudes that still haunted Gauss. In the context of differential geometry it freed the mind to propose many different descriptions of physical space (any set of n -fold extended quantities with a metric will do) and to regard Euclidean geometry as just one possibility among many. It also fits very well to the view that physics is about variable quantities which one can measure and whose variations obey some physical “laws”.

But Riemann’s reformulation of complex function theory had scarcely begun. He immediately saw that the global structure of the set of pairs of real numbers need not be the plane: any two-dimensional region will do, and the function may perhaps be continued outside an initial region of definition along strips and this continuation is unique because of the Cauchy–Riemann equations. Two-dimensionality is both necessary and sufficient: as Riemann remarked “Of course, this extension must not occur on bare lines where a partial differential equation cannot be applied, but in strips of finite width” (Riemann 1857a, 100). The global nature of the domain could therefore be a torus or a more complicated surface. Pursuing this insight led him to the notion we define (more precisely) today: the Riemann surface. Its topological structure is determined by two numbers: the genus of the surface, which Riemann defined topologically and then showed is the same as the number of linearly independent everywhere finite integrands it can support; and the number of its boundary components. This was the basis of his theory of Abelian functions. Letting the Riemann surface have infinitely many leaves allowed him to extend this idea to the study of linear ordinary differential equations. It was these deeper ideas that proved largely unacceptable to the likes of Cayley, Hermite, and Weierstrass.⁹

It was not only the difficult and boldly original nature of Riemann’s ideas that hindered their reception. He had had a few good students, and of them, Hankel and Roch died young:—Hankel in 1873 age 34, and Roch in 1866 age 26—and Hattendorff died in 1882 age 48. After 1866 the Göttingen tradition then lapsed into the hands of Schering, who was not a profound mathematician. More might have come from Clebsch’s discovery of Riemann’s ideas, had he also not died, aged 39, in 1872. Thereafter, the German scene was increasingly dominated by the school around Weierstrass and Kronecker, which placed much more emphasis on algebra than geometry, let alone topology. Exceedingly difficult papers, obscure lecture notes, and a half-dozen papers ready for publication at his death formed the content of the first edition of Riemann’s *Werke*. To master any one paper would mean hard lonely work, for only Prym and Riemann’s Italian friend Betti remained to remember what Riemann might have said on any point.

Other problems were caused not just by the genuine profundity of his ideas, but by their unfortunate expression. Riemann tried to be a lucid writer, but he was not always accurate. Many of his best ideas lack sufficient detail to be clear and

⁹See our discussion of Hermite’s ideas on p. 506.

seem open to refutation as a consequence. If his papers are read as a sketch of what is, or might be, the case then it is not clear that they offer the techniques and richness of conception to allow the reader to fill in the gaps. It can seem that other methods entirely will be needed to deliver what Riemann promised. His use of what is called Dirichlet's principle has become famous. Although, as we describe below, the usual historical accounts are misleading on this point, it is indeed true that Riemann's contemporaries found his use of Dirichlet's principle dangerous and were for that reason inclined to try and avoid it, even if that meant denying its many useful consequences. Last but not least, there was a striking insularity in the response. The mathematicians in Berlin wanted to work out only their own ideas, and to rework Riemann's in their own, different, spirit; mathematicians in France stayed with the tradition established by Cauchy and Briot and Bouquet. Riemann's ideas had to wait.

Each of Riemann's papers is densely written and carefully focused on a particular problem. To keep matters as simple as possible, in this chapter we concentrate on his three principal papers on complex function theory: the doctoral thesis; the paper on Abelian functions; and his paper on differential equations. We look at the implications he drew from them and at their immediate reception, but our interest is chiefly in the theory of complex functions that they presented. Papers and lectures that he gave on algebraic geometry and analytic number theory are also discussed here, but the one on minimal surfaces is taken up in Sect. 7.8, where the historical context can be dealt with more fully, and some more minor papers are discussed where it seems more appropriate.

What were Riemann's sources? Until recently little was known about them, but the recent biography by Laugwitz and archival studies by E. Neuenschwander have shed considerable light on the matter.¹⁰ Riemann first enrolled at the university of Göttingen, and while there started to read papers by Cauchy and Gauss. He was always happy to acknowledge the influence of Cauchy's pioneering work; Neuenschwander quotes convincing evidence that Riemann's students regarded his lectures as proceeding in the manner of Cauchy. When he transferred to the University of Berlin in 1847 he continued to read papers by Gauss, notably those on potential theory, where Gauss used his form of the Dirichlet principle. In 1849 Riemann tracked down Gauss's memoir on conformal maps (Gauss 1825); we shall argue that Riemann was the first to appreciate the intimate connection between conformal mappings and complex function theory. Despite the steady decline in his health, Gauss also discussed mathematics with Riemann: as Weil showed, quoting a letter from Betti to Tardy, the idea of cutting a surface came from a conversation with Gauss on mathematical physics.¹¹ Nonetheless, the great influence on Riemann was undoubtedly that of Dirichlet, who recognised and fostered Riemann's remarkable

¹⁰See Neuenschwander (1981a,b) and the references cited there, as well as his later papers.

¹¹The letter was first published in Loria (1915) in an obituary of Tardy, and then in Weil (1979b). Brill and Noether (1894, 254–255) also document interesting examples of ideas that anticipated the idea of cuts in the work of Kirchhoff in 1848 and Helmholtz in 1853.

talent. When he came to Göttingen university for the year in 1852, he and Riemann talked almost every day. It is in recognition of this debt that Riemann, after reviewing the use of it by Gauss, Thomson and Kirchhoff, called the principle that guarantees the existence and uniqueness of a function satisfying the Laplace equation and given boundary conditions after Dirichlet. For Dirichlet had, as he put it, “informed me that he had been using the principle in his lectures since the beginning of the 1840s, if I’m not mistaken”, see [Neuenschwander \(1981b, 98\)](#).

5.2 The Doctoral Dissertation of 1851

5.2.1 *Fundamentals*

In the opening half of this, his first major paper, Riemann developed three important ideas: the definition and basic properties of a complex function, the idea of a (Riemann) surface, and the fundamental role of the theory of harmonic functions. He began by defining what it is for a function to be continuous on a (closed) interval. Although the published version is couched in intuitive terms, surviving notes show that Riemann based his ideas of continuity on the epsilon–delta definition of continuity familiar to him from Cauchy’s *Cours d’analyse* or, perhaps more likely, from Dirichlet’s lectures on the theory of integration that Riemann followed in Berlin. Indeed, in his lectures Dirichlet used to prove in great detail that a continuous function on a closed interval enjoys the property that to any ρ as small one wishes one can associate a σ such that on every sub-interval $\leq \sigma$ the variation of the function is at the most ρ . Stated in modern terms, Dirichlet proved that a continuous function on a (closed) interval is uniformly continuous there.¹² Apparently, Riemann took this property as a definition of continuous function. His idea of continuity therefore corresponds to our uniform continuity. It later became current when it was advocated by Weierstrass.¹³ For a complex variable w to be a complex function of another complex variable z Riemann said that the value of the differential quotient $\frac{dw}{dz}$ had to be independent of the value of the differential dz . As Riemann observed, this is the case whenever the rules for differentiation are applied purely formally. He then devoted this paper to finding what properties are possessed by a general complex function.

Riemann began by regarding a complex function as a map from the $z = x + iy$ plane to the $w = u + iv$ plane. It followed directly from the definition (Sect. 3) that the map is conformal provided the derivative does not vanish. This emphasis on the conformal properties of a complex function was original; it cannot be found in the

¹²A set of the relevant lecture notes was published as [Dirichlet \(1904\)](#). The theorem is stated and proved on p. 4ff.

¹³For a discussion of the emergence of the modern concept of function, see Bottazzini, *The Higher Calculus*.

work of Cauchy, but is very much in line with the views of Gauss. Riemann also showed that the definition of a complex function implies that it satisfies what are nowadays called the Cauchy–Riemann equations:

$$\frac{\partial u}{\partial x} = \frac{\partial v}{\partial y} \quad \text{and} \quad \frac{\partial v}{\partial x} = -\frac{\partial u}{\partial y}. \quad (5.1)$$

From these equations it followed at once (Sect. 4) that the functions u and v are harmonic¹⁴; they satisfy

$$\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} = 0 \quad \text{and} \quad \frac{\partial^2 v}{\partial x^2} + \frac{\partial^2 v}{\partial y^2} = 0. \quad (5.2)$$

The Cauchy–Riemann equations are therefore necessary for a complex function to be holomorphic on a domain, and Riemann seems to have assumed that they are sufficient. As a result he later used the Cauchy–Riemann equations as the defining property of a complex function. The definitive modern result is the Looman–Menchoff theorem, which states that if f is a continuous function on an open set in $\mathbb{C}$ that has partial derivatives $\frac{\partial f}{\partial x}$ and $\frac{\partial f}{\partial y}$ which satisfy the Cauchy–Riemann equations at every point of the domain, then f is holomorphic in the domain.¹⁵ A proof of this theorem and the construction of complex functions that obey the Cauchy–Riemann equations but are not holomorphic everywhere in a domain is therefore subtle and beyond the technical reach of nineteenth-century mathematicians; it will not be pursued here.

In order to present a complex function more effectively, in particular when it was what nineteenth-century mathematicians called a many-valued function, Riemann next advocated in Sect. 5 that it be thought of as a surface spread out over the z -plane. There might be several isolated points on the surface corresponding to the same point of the z -plane, but a line of such points, corresponding to a fold in the surface was not allowed. The surface may, however, wind around certain points, which Riemann called branch points (*Windungspunkte*); the way in which it joins up in the neighbourhood of a branch point was explained only obscurely. Although we know that Riemann had read Cauchy’s report on Puiseux’s memoir by December 1851 it seems unlikely that Riemann had anything to learn from Puiseux by the time he was writing his doctoral thesis. According to [Markushevich \(1996, 204\)](#), although it was not mentioned in the dissertation, “Riemann could not have overlooked Puiseux’s paper”. This view was shared by [Neuenschwander \(1980, 9\)](#) who has provided evidence that “in an unpublished draft of December 16th 1851

¹⁴Riemann used no specific word for this property.

¹⁵The theorem was first stated by [Montel \(1913\)](#). Looman produced a proof in 1923 that, however, had a gap that was eventually filled in ([Menchoff 1936](#)). For a modern proof ([Narasimhan and Nievergelt 2001](#), 48–50).

for the defence of his doctoral dissertation” Riemann referred to [Cauchy’s \(1851g\)](#) review of Puiseux’s paper, which had just appeared in the *Comptes rendus* of the Paris Académie. In Markushevich’s opinion, “the attentive reader of [Riemann’s] paper would be able to see” in the concept of a multi-sheeted surface “an excellent geometric commentary and at the same time what appears to be a profound summary of Puiseux’s work”.

We cannot agree with Markushevich; the idea of Riemann surface is much more than this. The Riemann surface is not simply a “picture” for representing the multi-valuedness of functions. In Hermann Weyl’s suggestive words (1913, v–vi), it is “the foundation” of the theory, “it must throughout be regarded as the prius, as the mother earth in which functions can first of all grow and flourish”. Moreover, Riemann’s primary aim was not the investigation of the behaviour of algebraic functions, as it had been for Puiseux. Instead, as we will see below, he was searching for a general method for embracing and treating large classes of functions in a unified manner and precisely this search led him to his concept of a “Riemann” surface. Finally, it is hard to imagine that a paper of the originality of Riemann’s thesis is brought about by a reading of Puiseux’s memoir; it is much more likely that Riemann had his ideas in place well before he read either the memoir itself or Cauchy’s commentary upon it.

Riemann then attempted to explain in Sect. 6 how a surface could be broken down into manageable pieces by means of cuts. He said a piece of surface was connected when any two points in it can be joined by a curve (in modern terms, this defines path-connected). He decomposed the surface by means of boundary cuts (*Querschnitte*) which were simple curves joining two points on the boundary; pre-existing such cuts also counted as parts of the boundary.¹⁶ A surface was said to be simply connected if any boundary cut broke it into two pieces (and he proved that a simply connected surface is cut by a boundary curve into precisely two pieces). He argued, by looking at the effect of following one system of cuts by another, that if a surface T fell into m_1 simply connected pieces via a system of n_1 boundary cuts, and via a second system of n_2 boundary cuts into m_2 simply connected pieces, then $n_1 - m_1 = n_2 - m_2$. Consequently, if a surface is cut into m simply connected pieces by n boundary cuts, then the number $n - m$ is a constant which he proposed to call the order of connectivity of the surface. A surface originally in one piece was said to be n -fold connected if a system of $n - 1$ boundary cuts were required to make it simply connected. Riemann claimed that the surface defined by an algebraic equation $F(x, y) = 0$ has an order of connectivity $2p - 1$ for some integer p ; the number p was later called the genus of the surface in [Clebsch \(1864, 190\)](#).

¹⁶According to [Brill and Noether \(1894, 259\)](#) the concepts of multi-sheeted surface and of crosscuts were first introduced “in essence” in an (unpublished) note on a problem of electrostatic or thermal equilibrium on the surface of a cylinder with circular crosscuts. Referring to [Riemann \(1876b\)](#) they wrote that “We are inclined to call this note one of Riemann’s earliest works, or rather its way of thought the starting point for Riemann’s works on function theory”.

5.2.2 The Introduction of Harmonic Functions

To study complex functions, Riemann now introduced the harmonic function v that is conjugate to u . Riemann here made ingenious use of the relationship between the integral of a function taken over a region and the integral of another function taken over the boundary of that region. This relationship forms a theorem known already to Cauchy, Green, and Gauss (undoubtedly Riemann's source for it, perhaps via Dirichlet):

$$\int \left(\frac{\partial X}{\partial x} + \frac{\partial Y}{\partial y} \right) dT = - \int (X \cos \xi + Y \cos \eta) ds, \quad (5.3)$$

where the first integral is taken over the surface and the second along the boundary, and the angles ξ and η are the angles the normal makes with the x and y axes respectively.

Riemann proved this theorem and then applied it in the special case when the surface integral vanished. He decomposed the surface T with cuts until he obtained a simply connected surface T^* . Applying the theorem in this case he showed that the integral along the boundary was a single-valued function of its upper endpoint (the lower endpoint being assumed fixed) provided that the upper endpoint was not allowed to vary over the boundary cuts. If the upper endpoint was moved across a cut, then the value of the integral increased by a constant quantity, independent of the position where the cut was crossed, but depending on the cut in question. Each cut determined an independent quantity.

Then, in Sect. 10, he considered a function u defined on a surface T that covers the plane once and that is harmonic—except perhaps at a set of points that does not fill a region, and for which the points where u , $\frac{\partial u}{\partial x}$, and $\frac{\partial u}{\partial y}$ are discontinuous do not fill a line, and for which the discontinuities in $\rho \frac{\partial u}{\partial x}$ and $\rho \frac{\partial u}{\partial y}$ go to zero with ρ , and for which u has no removable singularities.¹⁷ He showed that such a function and all its partial derivatives are finite and continuous at all interior points of the surface.

In Sect. 11 Riemann then established several of the key properties of such function. He proved that for such a function if along a curve λ both u and its normal derivative $\frac{\partial u}{\partial p}$ vanish then $u = 0$ everywhere. The proof proceeded by showing first that λ cannot be the boundary of a region where $u > 0$ and then that the boundary of a region where $u > 0$ is a curve like λ ; this contradiction proves the theorem. Riemann then deduced that if u and $\frac{\partial u}{\partial p}$ are given prescribed values on a curve then the function u is prescribed everywhere it is defined. He then showed that if a function u is not constant the regions where u takes a constant value divide the regions where u is larger from regions where u is smaller. This followed from the observations that u cannot have either a maximum or a minimum at a point within T ; u cannot be constant only in a part of the surface, and a curve along which $u = a$,

¹⁷The awkward language shows how little point-set topology was available at this time.

a constant, cannot separate two regions on which $u - a$ has the same sign. These properties are to be found, explicitly or just below the surface, in Gauss's study of potential theory, his *Allgemeine Lehrsätze (General Propositions)* of 1840.

In (Sect. 12) Riemann again took up the consideration of a function $w = u + iv$ which, except at isolated lines and points, is continuous and satisfies the (Cauchy–Riemann) equations (5.1) on a simply connected single-sheeted surface T , where it has no removable singularities and investigated what happens when the function becomes infinite. He first proved that “If a function w of z presents discontinuities which do not form curves, and if in addition for any arbitrary point O' of the surface, where $z = z'$, $(z - z')w$ becomes infinitely small as O' approaches the point O indefinitely, then w is necessarily finite and continuous, together with all of its derivatives, at all points in the interior of the surface”. To prove this, he observed that the hypotheses imply that the integral over the boundary

$$\int \left(u \frac{\partial x}{\partial s} - v \frac{\partial y}{\partial s} \right) ds = 0.$$

Moreover, the integral has the same value along any curve joining an arbitrary point P to O and for a fixed P is a continuous function, U , of x, y except at some isolated points. But then the partial derivatives of U satisfy

$$\frac{\partial U}{\partial x} = u \quad \text{and} \quad \frac{\partial U}{\partial y} = -v$$

at every point. Therefore, by the earlier result (Sect. 10) that said a harmonic function is infinitely differentiable, the function U is infinitely differentiable everywhere in T , and the same holds for the complex function $w = \frac{\partial U}{\partial x} - i \frac{\partial U}{\partial y}$ and its derivatives with respect to z .

If instead (Sect. 13) the hypothesis is that $(z - z')w$ does not tend to zero with $z - z'$ but that $(z - z')^\rho w$ does tend to zero with $z - z'$, then the function

$$w - \left(\frac{a_1}{z - z'} + \frac{a_2}{(z - z')^2} + \cdots + \frac{a_{n-1}}{(z - z')^{n-1}} \right)$$

is finite and continuous at O' , where n is the least integer greater than or equal to ρ . For now $(z - z')^n w$ tends to zero with ρ , and so $(z - z')^{n-1} w$ is a function of z . He denoted its value at z' by a_{n-1} and by repeating the argument $n - 2$ times showed that the function w can be changed into one that is finite and continuous in a neighbourhood of the singular point z' by subtracting an expression of the form¹⁸

$$\frac{a_1}{z - z'} + \frac{a_2}{(z - z')^2} + \cdots + \frac{a_{n-1}}{(z - z')^{n-1}}. \quad (5.4)$$

¹⁸A Laurent tail, as we would call it; Riemann gave it no special name.

This means, he observed, that if a function becomes infinite to some finite order at a point then this order of infinity is necessarily an integer.

Riemann next observed (in Sect. 14) that none of the results so far depend on the domain of the complex variable z covering the plane simply but are valid on an arbitrary patch of surface; however, winding points do require special treatment. This he gave by introducing polar coordinates in a disc that mapped one-to-one onto a disc-like neighbourhood of the branch point.¹⁹ The paradigm example is the function $w = (z - z')^{1/n}$ which has a branch point of order $n - 1$ in Riemann's terminology. He showed that if a function becomes infinite to some finite order at such a branch point, then it can be made finite and continuous there by attaching to it an expression of the form

$$\frac{a_1}{(z - z')^{1/n}} + \frac{a_2}{(z - z')^{2/n}} + \cdots + \frac{a_m}{(z - z')^{m/n}}, \quad (5.5)$$

and noted that the attached term depends on m arbitrary complex constants (Fig. 5.2).

Finally he showed that a complex function maps a surface continuously onto another surface. This move is characteristic. The independent complex variable is assumed to be free to move in a two-dimensional region, which may be a part of the complex plane or merely related to it in some way. To be a complex quantity, on Riemann's approach, is to be a two-dimensional real quantity, nothing more. And a quantity, Riemann was to explain in his *Habilitationsschrift*, was anything that admitted different determinations; in short, a variable.

The philosophical subtleties of this position need not detain us here, but it was a break with contemporary views of mathematics as being about magnitudes, quantities that could, for example, be compared in point of size. Although probably uncontroversial among mathematicians by 1851 as a definition of a complex quantity, Riemann's emphasis that any two-dimensional domain could be regarded as the domain of a complex variable was wholly novel and unexpected. The only other paper to reject the tendency to regard a complex function as defined on the complex plane was Puiseux's conception of an algebraic function. But despite the way his paper opened, Riemann's theory of complex functions is not about functions defined on the plane of complex numbers; rather, it is about them and their generalisations defined on any two-dimensional patch whatever.

Riemann emphasised the novelty and generality of his approach to complex functions by stating that

Previous methods for handling these functions have always given as the definition an expression of the function in which its value was given for every value of the argument. Through our research it has been shown that, as a consequence of the general character of a function of a complex variable, in a definition of this kind a part of the determining elements is a consequence of the rest, namely the range of the determining elements is traced back to those necessary for determining [the function]. (Riemann 1851, Sect. 20).

¹⁹Nowadays called a local uniformising parameter.

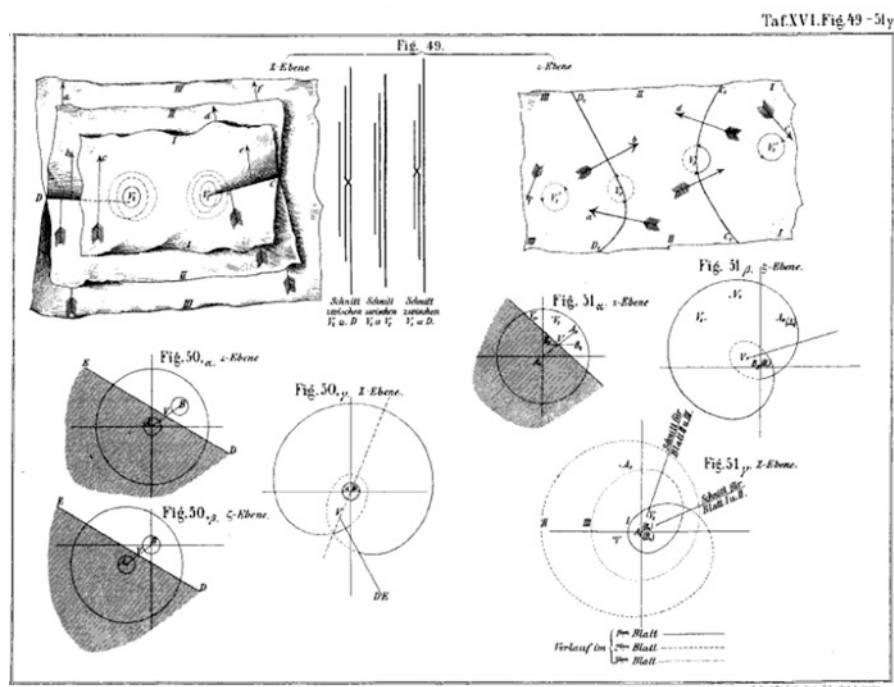


Fig. 5.2 A picture of branch points (Holzmüller *Einführung in die Theorie der isogonalen Verwandtschaften und der conformen Abbildungen*, figure 49)

As an example of this, he considered algebraic functions. If the domain of the variable z covers the entire infinite plane simply or multiply and the function is allowed to be discontinuous only at a finite number of isolated points (where it becomes infinite to finite degree, i.e. it has poles, as we would say today), then the function is algebraic, and reciprocally every algebraic function satisfies these conditions. (He tacitly assumed that at all the remaining points the function has a derivative.)

5.2.3 The Dirichlet Principle

It is often said that Riemann's proofs rested at crucial points on an appeal to Dirichlet's principle, and that for this reason were not widely accepted.²⁰ Indeed, this principle was widely regarded as of heuristic value only, and as much else in Riemann's argument was likewise to be contested, this part of the paper was to

²⁰See, for example, the "source" book on its history, Monna (1975), and the discussions in Bottazzini, *The Higher Calculus* (1986) and Gray (2000a).

occupy mathematicians for the next 70 years. But in fact Riemann's approach was rather different and, more to the point, was soon shown to be fatally flawed.

Riemann did not naively apply something called Dirichlet's principle. This principle, which he only referred to by name in his paper on analytic functions of 1857, is the claim that among the functions u defined on the boundary of a connected domain²¹ T and which are continuously differentiable inside the domain, there is at least one that minimises the integral

$$\int_T \left(\left(\frac{\partial u}{\partial x} \right)^2 + \left(\frac{\partial u}{\partial y} \right)^2 \right) dT.$$

It is easy to show that such a minimiser is unique, and then to deduce that if a continuous function is given at every point of the boundary of a simply connected region, then there is a unique harmonic function defined on the whole region and agreeing with the given function on the boundary. In 1857 Riemann explained that in his (1851) he had been seeking to generalise this principle to cases where prescribed discontinuities are allowed. To do this in his (1851) he first asserted that if a certain integral over a surface T ,

$$L(\alpha, \beta) = \int \left(\left(\frac{\partial \alpha}{\partial x} - \frac{\partial \beta}{\partial y} \right)^2 + \left(\frac{\partial \alpha}{\partial y} + \frac{\partial \beta}{\partial x} \right)^2 \right) dT, \quad (5.6)$$

is finite, where α and β are two arbitrary functions of x and y , then by varying α by a continuous function (or one discontinuous only at single points) which is zero everywhere on the boundary of T , the integral attains a minimal value and moreover this minimum is attained by a unique function if one excludes the points of discontinuity. This unique minimising function is harmonic.

Riemann then considered a function λ that vanished on the boundary, could be discontinuous at isolated points, and for which the integral

$$L(\lambda) = \int \left(\left(\frac{\partial \lambda}{\partial x} \right)^2 + \left(\frac{\partial \lambda}{\partial y} \right)^2 \right) dT \quad (5.7)$$

is finite. He let $\alpha + \lambda = \omega$, and considered the integral

$$\int \left(\left(\frac{\partial \omega}{\partial x} - \frac{\partial \beta}{\partial y} \right)^2 + \left(\frac{\partial \omega}{\partial y} + \frac{\partial \beta}{\partial x} \right)^2 \right) dT, \quad (5.8)$$

which he denoted Ω . "The totality of these functions", he wrote (1851, Sect. 16), "represents a connected domain closed in itself, in which each function can

²¹Dirichlet in fact stated this principle for three-dimensional domains (Dirichlet 1876, 127–128).

be transformed continuously into every other, and a function cannot approach indefinitely closely to one which is discontinuous along a curve without L becoming infinite". So for each λ, Ω only becomes infinite with L , which depends continuously on λ and can never be less than zero; consequently has at least one minimum. The uniqueness followed more straight-forwardly from looking at functions $u + h$ near to a minimum u .

The claim about the effect of a line of discontinuities was proved in Sect. 17. In Sect. 18 Riemann showed that if $L(\alpha, \beta)$ were finite on a connected surface T which can be cut up into a simply connected region T^* , then there is always a unique function $\mu + \nu i$ of z such that μ vanishes at all but finitely many points on the boundary, ν takes an arbitrary value at an arbitrary point, and μ is continuous in T and ν is continuous in T^* except possibly at isolated points and then only in such a way that $L(\mu)$ and $L(\nu)$ remain finite and $L(\nu)$ takes the same values on opposite sides of the cuts.

This result is absolutely central to Riemann's whole approach, for it leads immediately (Sect. 19) to the claim that a finite and continuous function $w = u + \nu i$ is given uniquely on a simply connected domain when u is an arbitrary continuous function on the boundary, when ν takes a given value at an arbitrary point. As he remarked: "[These] principles open the way to the study of definite functions of a complex variable independent of an expression for it" (1851, Sect. 19).

This is a striking philosophical formulation of the burgeoning subject of functions of a complex variable. In this connection, it is worth reading what Riemann's student Prym wrote to Felix Klein on February 6, 1882:

According to a report Riemann gave to me in spring 1865 during my stay in Pisa, he himself was led to a theory of functions of a complex variable magnitude by the observation that the relationships among functions, obtained through the expansion of the relevant functions in series, persisted even if one went outside the domain of convergence of the representing series, and that in many cases one obtains correct results when operating with divergent series as e.g. Euler repeatedly did. Then he [Riemann] asked himself what properly does the continuation of the function from one domain into the other one [involve], and he was led to the opinion that this is done by the partial differential equation. Dirichlet, with whom he discussed this subject, agreed completely with this opinion; this idea occurred to Riemann in his student days, before the preparation of his Inauguraldissertation.²²

Riemann then sketched, in Sect. 20, how the new approach to complex functions could go²³:

Previous methods of treating these functions always based the definition of the function on an expression that yields its value for each value of the argument. Our study shows that, because of the general nature of a function of a complex variable, a part of the determination through a definition of this kind yields the rest. Indeed, we reduce this part of the determination to that which is necessary for complete determination of the function. This essentially simplifies the discussion. For example, to show the equality of two expressions for the same function, one formerly needed to transform one into the other: that is, show

²²Klein Nachlass NSUB Göttingen, Prym–Klein, quoted in (Bottazzini 2003, 233).

²³This translation is taken from Riemann (2004, 35–36).

that the expressions coincide for every value of the variable. Now it suffices to prove the expressions coincide in a much more restricted domain.

A theory of these functions based on the principles introduced here would fix the form of the function (that is, its value for every value of the argument) independently of a means of determining the function via operations on quantities. We determine the function by appending to the general idea of a function of a complex variable just those features that are necessary for the determination. Only at this stage do we pass to the various representations that the function permits. The common character of a class of functions formed in a similar way by operations on quantities, is then represented in the form of boundary conditions and discontinuity conditions imposed on them. Suppose for example that the domain of a complex variable extends over the plane in either simple or multiple fashion, that the function has only isolated points of discontinuity interior to the domain, and only has infinities of finite order. (For infinite z , the quantity z itself; for finite z' , the order of $\frac{1}{z-z'}$, is an infinite quantity of first order.) Then the function is necessarily algebraic and conversely each algebraic function fulfills these conditions.

We leave aside for the present the development of this theory which, as observed above, will shed light on the simple laws of dependence arising from operations on quantities, since we exclude for the present the discussion of expressions for a function. On the same grounds we do not consider here the utility of our results as the basis of a general theory of the laws of dependence. For this, one would need to show that the concept of a function of a complex variable, taken as fundamental here, coincides fully with that of a dependence²⁴ that expressed in terms of operations on quantities.

Set aside for the moment the fact that Riemann's claim in the final paragraph proved to be incorrect, as Weierstrass was to show (see Sect. 6.9) and we see that Riemann offered a sweeping new approach intended to remove computational or algebraic approaches from their central position and subordinate them to conceptual arguments that would control their use.

Riemann's approach breaks entirely with the idea that the subject has essentially to do with formal expressions such as power series expansions. Instead it asserts that the proper way to think of a complex function is in terms of a pair of component harmonic functions, one defined on the boundary of the domain of definition of the function, and the other prescribed at one arbitrary point. Riemann pointed out that the region T can be multiply connected, for one can simply pass to a simply connected region T^* by making boundary cuts. (In this way a many-valued function is obtained which jumps as one crosses the cuts.) Moreover, functions can be allowed to have singularities at single points, as he had explained earlier in the memoir. So a complex function maps one surface conformally onto another and is determined completely when its boundary values and its behaviour at singular points is known. This, he argued, greatly simplified such tasks as deciding when two expressions in fact represented the same function. So it is immediately clear, he said (Sect. 20), that if a function is defined everywhere on a region that covers the entire plane once or several times, and has "points of discontinuity" at only finitely many points and "finitely many infinities only of finite orders" then it is an algebraic function.

²⁴Riemann here added this footnote: "The dependence expressed here denotes dependence via a finite or infinite number of the four simplest operations, addition, multiplication, subtraction, and division. The expression 'operation on quantities' (by contrast to 'operations on numbers') indicates operations in which the rationality of the quantities does not play a role".

“To this extent it is right”, Prym went on in his letter to Klein quoted above, “to say that Riemann came to his researches through mathematical physics. His Inauguraldissertation is in many places nothing other than the Gauss–Dirichlet potential theory reduced to two dimensions”. Or, in Ahlfors’ words (1953a, 4): Riemann “virtually puts equality signs between two-dimensional potential theory and complex function theory”. Contrary to the conception of a function as something given by an analytical expression, this approach leads to the determination of a function from its singularities, as Riemann repeatedly stated, and it is founded on existence and uniqueness theorems, which he proved by means of a variational principle (Dirichlet’s principle) already known and used by Gauss and Dirichlet in their works on potential theory.

5.2.4 *The Riemann Mapping Theorem*

Given this emphasis on defining complex functions via harmonic functions and harmonic functions via continuous functions defined on the boundary of a region, it was natural for Riemann to consider the question of whether two simply connected regions whose boundaries correspond one-to-one can sustain significantly different families of complex functions. In the last part of the paper Riemann showed that the answer was negative and offered a proof that

Two given simply connected plane surfaces can always be mapped onto one another in such a way that each point of the one corresponds to a unique point of the other in a continuous way and the correspondence is conformal; moreover, the correspondence between an arbitrary interior point and an arbitrary boundary point of the one and the other may be given arbitrarily, but when this is done the correspondence is determined completely. (Riemann 1851, Sect. 21).

This important result, in a somewhat more precise form, is nowadays called the Riemann mapping theorem. It establishes not only that there are complex functions defined on any simply connected region, and indicates how they can be defined, but also that any two such regions are equivalent for the purposes of Riemannian complex function theory. One should infer from the reference to boundary points that the entire complex plane is excluded from consideration (as it must be, by Liouville’s theorem).

As an indication of what could usefully be done from his general standpoint that was not based on the study of explicit representations for functions, Riemann ended his paper by showing that two simply connected regions can be mapped conformally onto one another.²⁵ Riemann argued that it was enough to show that any such region T could be mapped conformally onto the unit disc, $\mathbb{D}$, and outlined a proof in two stages. To obtain the conformal map of T to the unit disc and to show that

²⁵The regions are assumed to have more than one point on their boundaries, but the nature of the boundary is nowhere discussed. It is unlikely Riemann had anything other than a simple closed curve in mind, since he took one region to be a disc. See Gray (1994).

it extended to the boundary, Riemann took local coordinates $z - z_0 = re^{i\varphi}$ defined on a suitably small disc around an arbitrary interior point z_0 of T , and looked at the map $\log(z - z_0) = \log(r) + \varphi i$ defined on that small disc, which he imagined cut along a radius. He then extended this cut to a specified point on the boundary of T . He extended the function defined on the small disc by a continuous function to a function $\alpha + i\beta$ on the whole of T with the properties that:

1. α vanished on the boundary of T ,
2. $\alpha + i\beta$ jumped across the cut by $2\pi i$ (like $\log$), and
3. the new function agreed with the old one on the boundary of the small disc.²⁶

Riemann obtained a new function as follows. It has the same real part on the boundary of T as does α , and the harmonic function defined on the interior of T with these boundary values is, say, $u(z)$. The harmonic conjugate of $u(z)$ is, say, $v(z)$ (which is determined up to an arbitrary constant), and the new function on T is $u(z) + iv(z)$. The function $F(z) = \alpha(z) + i\beta(z) - (u(z) + iv(z))$ then has the following properties. It has a logarithmic singularity at z_0 , it has zero real part on the boundary of T and therefore a purely imaginary part on the boundary of T . The exponential of this function will therefore map z_0 to the centre of the disc and the boundary of T to the circle, provided that the map is one-to-one everywhere including the boundary.

Riemann's argument shows quite clearly that the real part of the function $F(z)$ takes every value a from $-\infty$ (at z_0) to 0 (on the boundary of T). Riemann claimed that the inverse images $\text{Re}(F^{-1}(a))$ were single simple closed curves for every value of a , arguing that this followed from the facts that T is simply connected and the function u is harmonic. Indeed, the function $F(z)$ cannot have a branch point in the interior of T for these reasons. It follows that $F(z)$ is one-to-one on the interior and therefore also the boundary of T .

Consequently, he concluded, the sought-for function was

$$e^{F(z)} : T \rightarrow \mathbb{D}. \quad (5.9)$$

as required.

It is remarkable that this argument, which derives essentially from the early parts of Riemann's paper, and which is close to a Greens's function argument, has not been better appreciated. Applied to simple shapes such as a square it leads directly to an argument that establishes a conformal map of the square to the disc, whenever a Poisson integral argument is available to define the harmonic function u . It seems that Riemann's argument was too conceptual for the people in Berlin even when it could be made quite concrete.

After a few remarks claiming, none too plausibly, that the entire theory could readily be extended to regions which are not simply connected, this remarkable paper came to an end.

²⁶If the region T is a subset of the plane, it is enough to consider the function $\log(z - z_0)$.

5.3 Reactions to the Paper of 1851

The first person to respond to Riemann's doctoral thesis was, of course, Gauss, who reported on it for the Göttingen Faculty ([Remmert 1998b](#), 207). In Remmert's translation of Gauss's report, Gauss wrote

The paper submitted by Mr. Riemann bears conclusive evidence of the profound and penetrating studies of the author in the area to which the topic dealt with belongs, of a diligent, genuinely mathematical spirit of research, and of a laudable and productive independence. The work is concise and, in part, even elegant: yet the majority of readers might well wish in some parts a still greater transparency of presentation. The whole is a worthy and valuable work, not only meeting the requisite standards which are commonly expected from doctoral dissertations, but surpassing them by far.

Reading this report one is inclined to say that Gauss's comments only too obviously meets the requirements for such reports, but no more. As Remmert comments, not even the subject matter of the thesis is mentioned, but it is clear Gauss appreciated the remarkable talents of the young Riemann and praised the thesis highly. According to [Dedekind \(1876, 545\)](#) "Gauss . . . informed Riemann at his interview that he had been preparing a paper for several years that dealt with the same subject, but was not restricted to it".²⁷ [Remmert \(1998b, 208\)](#) has suggested that this refers to the extensive unpublished drafts of a paper collected in Gauss *Werke* 10.1, 400–419, where Gauss ruminated on the nature of complex functions and how many-valued functions and their integrals could be handled. If so, it is clear that Gauss would have been in a good position to appreciate how much further Riemann had been able to go.

However, the most significant problem with the diffusion of Riemann's message was that his thesis was not properly published. It was printed as a separate thesis and distributed to a number of German universities, as was the custom, but it was not reprinted in a journal such as Crelle's *Journal*. This fact alone would go a long way to explaining why there are no signs of his approach winning converts in the early 1850s. But for any who took the trouble to consult the paper, its many novelties worked against its immediate acceptance. So too did its vagueness at certain crucial points and its often murky language. The novelties included the emphasis on such topological concepts as cuts and connectivity and on the central role of harmonic functions, but most of all on the way a complex function was to be specified and hence thought about. The mathematical vagueness was most pronounced in the use of Dirichlet's principle, especially in the attempt to extend it, but the references to multiply connected regions were also often obscure. So, too, were many provocative asides made in the paper and omitted from the above discussion. Finally, as one might expect of a paper which is the author's first, the organisation of the topics and indeed the organisation of some of the sprawling sentences is far from helpful. Thus Ahlfors wrote a century later that Riemann wrote "almost cryptic messages to the

²⁷This was confirmed by Riemann himself in a letter to his brother Wilhelm on November 24th, 1851. See [Neuenschwander \(1981a, 104\)](#).

future” and stated his mapping theorem in a form that “would defy any attempt at proof, even with modern methods” (Ahlfors 1953a, 3–4).

One of the few to come to the defence of Riemann’s doctoral dissertation was the young Heinrich Weber in his (1870). He made two assumptions: that the boundary was smooth enough away from a finite number of corners to have a well-defined normal and finite curvature everywhere; and that the function on the boundary was continuous and has at least one extension to a continuous function defined everywhere inside the domain and for which the integral

$$\iint \left(\frac{\partial u}{\partial x} \right)^2 + \left(\frac{\partial u}{\partial y} \right)^2 dx dy$$

is finite. He then described an iteration process that in principle, although not usefully in practice, led to a suitable harmonic extension of the given function on the boundary and so, under his restrictive hypotheses, to a proof of the Dirichlet principle. These assumptions do suffice to protect the contentious principle, although his proof is certainly not adequate by modern standards, and we suppose that it did not make a lasting impression on Weber’s contemporaries because its hypotheses were both vague and unduly restrictive.

Weber’s remarks in defence of the principle indicate how poorly understood the difficulties with it were. In advance of Weierstrass’s published comments, Weber singled out as the nub of prevailing criticism that it was not considered to be clear that the calculus of variations applied to functions with a priori completely unknown properties (1870, 29). This is a powerful remark because the whole subject of arbitrary functions was opening up at the time and little was known except that there was much to be discovered. Since this sweeping criticism would render invalid any use of the calculus of variations, which is aimed at finding which unknown function maximises or minimises a given integral, it is hard to see this as a criticism of the Dirichlet principle. Rather, the anxiety seems to be focussed on a claim that, even if valid, provides no information about the solution function (other than, of course, that it is harmonic and has prescribed boundary values).

For all these reasons his students and contemporaries were to turn eagerly to the other papers Riemann wrote and the lectures he gave. Some of their comments relate to the entire body of Riemann’s ideas as they came to perceive them, and we shall look at them later. But one point is worth taking up now: the sharp break between his ideas and those of Eisenstein. In this connection, Dedekind remarked in his biography of Riemann that

Riemann later explained that he (and Eisenstein) had discussed the introduction of complex quantities into the theory of functions but had been of markedly different opinions about the principles that should be taken as basic. Eisenstein stood by the formal calculus, while he himself had seen in the partial differential equation the essential definition of a function of a complex variable. (Dedekind, in Riemann 1990, 576).

For Eisenstein, a master of formal manipulation, the main aim of his research was to make the theory of elliptic functions yield results in the theory of numbers. The

foundations of the subject need not have exercised him, and geometrical ways of thinking were not congenial. In his approach, complex functions were given, indeed defined, by certain formal expressions: infinite series, infinite products, implicit functions, and integrals. This was the style of which Jacobi was the master, and which all who sought to work on elliptic functions had to learn. It dealt with complex functions as they naturally arose and was a style in which new results were to be found. One of the most novel features of Riemann's presentation was the way it changed the type of questions one could ask about a complex function, away from the computational and towards the conceptual. As we have just seen, Riemann even raised explicitly the question of whether the class of complex functions obtainable by his means agreed with the class of functions definable by a finite or infinite number of operations of addition, multiplication, subtraction and division. He felt that the two classes did coincide. Riemann added that a general theory of the "laws of dependence" (*Abhängigkeitsgesetze*) based on his own assumptions would require a proof of the fact that "the concept taken here as fundamental for a function of a complex variable agrees completely with the concept of dependence expressible through operations on magnitudes". However, subsequent research by [Seidel \(1871\)](#) and [Weierstrass \(1880c\)](#), see Sect. 6.9, showed that this is not the case.

To see what sorts of question arose within the Riemannian framework, it is most natural to consider how his own ideas developed. After the completion of his thesis, Riemann himself gave a hint of his research projects by writing in an undated note now published among his "Fragments with philosophical content" (*Werke*, 539–570) that his first concern was

to introduce the imaginary into the theory of other transcendental functions in the same way as they have already been introduced with such great success into the [theories of] algebraic functions, the exponential or trigonometric functions, the elliptic and Abelian functions; I have set out the necessary, general preliminary work for this in my Inauguraldissertation ([Riemann 1990](#), 539).

This programme strikingly echoes Gauss's plea that new functions should always be considered as complex functions. They were next presented in public when Riemann lectured on complex functions, in particular elliptic and Abelian functions, in 1855/56. It will be most convenient if we discuss these lectures together with their second presentation, that of 1861/62.

5.4 The Lectures from 1855/56 to 1861/62

There are various sources for this material. Extracts were published in the first edition of the *Werke*, and more in the *Nachträge* that accompanied the second edition. Riemann's student Roch published his version of Riemann's course of 1861/62 as [Roch \(1863a, 1865c\)](#), and it was one of the few published sources for Riemann's ideas for quite a number of years. [Roch \(1863a, 12\)](#) commented: "The lectures of my revered teacher Riemann have been extensively used, as much for

the manner of representation as for the detailed examples, and he has graciously given me permission to use their content completely". In 1896 and 1899 H. Stahl published his own notes on the topics of the second half, the theory of Abelian functions and the theory of elliptic functions respectively.²⁸ Various other sets of notes survive, on the basis of which a detailed version has been published (see Neuenschwander 1988; Riemann 1996). The Göttingen archive (Cod. Ms. Riemann Akt nr. 37) contains Schering's 192 pages of notes covering the general theory (see Gray 1997).²⁹

The lecture course of 1861 begins with an account of the geometric meaning of complex quantities and their arithmetic. This was followed by a philosophical passage in which the extension of the number concept from the natural numbers to the integers, the rationals, and finally the complex numbers was defended. The extension proceeds from an intuitive insight into the new quantities via the idea of inverting allowed operations, for example inverting addition to obtain subtraction. When, asked Riemann, could one imagine the opposite of a quantity? He answered: when what is at stake is not a pure quantity but a relationship between two quantities. This extension of the idea of quantity follows a modification in the meaning of the arithmetical operation. For example, complex quantities are first thought of as defining changes of position in the plane. One can then ask for the conditions under which a meaning can be given to imaginary quantities. Riemann's lectures are obscure at just this point (which may be why Roch omitted this passage).³⁰ He presented both an interpretation of $\sqrt{-1}$ as a mean proportional between $+1$ and -1 , and an endorsement of Gauss's views of 1831, before offering the view that the geometric representation of complex quantities, although it established their reality, was more important for making them intuitive.

From there, Riemann turned to define a complex function, w , as one which satisfied the equation

$$\frac{\partial w}{\partial y} = i \frac{\partial w}{\partial x}. \quad (5.10)$$

This ensured that the function w was a function of the single variable $z = x + iy$, and, he said (incorrectly) that its derivative depended only on the position of z but not on the direction of dz . Conversely, such independence for a function guaranteed that it was truly a complex function. Such a function is conformal except where the derivative vanishes said Roch (but not Riemann) in 1861, and the real and imaginary parts u and v are also harmonic, said Riemann, although he did not use the word. The integral of a complex function along a path was then defined, and the question raised of when the integral around a closed path vanished. Riemann answered it by a Green's theorem approach, proving that

²⁸See Stahl (1896) and Riemann (1899).

²⁹Another glimpse is given in the four pages of notes described in (Elstrodt and Ullrich 1999).

³⁰Instead Roch gave a careful account of the number e (defined as $\lim(1 + 1/x)^x$ for $x = \infty$) and the exponential function.

$$\int \int \left(\frac{\partial Y}{\partial x} - \frac{\partial X}{\partial y} \right) dx dy = \int X dx + Y dy. \quad (5.11)$$

Since $\frac{\partial Y}{\partial x} - \frac{\partial X}{\partial y} = 0$ when $X = w$ and $Y = iw$, Riemann concluded that the integral of a complex function around a closed contour vanished, provided that the conditions of his version of Green's theorem applied, in particular that the function w was everywhere finite and continuous inside the contour.

The example of the log function and the integral $\int_1^z \frac{dz}{z}$ motivated an investigation of when the integral around a closed contour does not vanish. Riemann showed that at points a_i where $\lim_{z \rightarrow a_i} f(z)$ is infinite as $z \rightarrow a_i$, but $\lim_{z \rightarrow a_i} (z - a_i) f(z)$ is finite and equal to c_i , say, the integral enclosing the point $z = a_i$ once picks up a contribution of $2\pi i c_i$. Thence he deduced that if the function is analytic everywhere in the interior, and t is an arbitrary interior point, then

$$f(t) = \frac{1}{2\pi i} \int \frac{f(z)}{z-t} dz, \quad (5.12)$$

with the familiar implications for the n th derivatives:

$$f^{(n)}(t) = \frac{1 \cdot 2 \dots n}{2\pi i} \int \frac{f(z)}{(z-t)^{n+1}} dz. \quad (5.13)$$

These are the Cauchy integral formulae, which Riemann could have read in Cauchy's *Exercices de mathématiques*, which he borrowed from the Göttingen University Library when he was a student in 1847.

The lectures then continued with an account of how a complex function can be developed inside a disc as a convergent power series and inside an annulus as a convergent Laurent series. A function cannot vanish along a curve without all the coefficients in a power series expansion vanishing, so a complex function cannot vanish along a curve. Consequently any identity between functions that holds for real values of their arguments holds also for complex values.

Turning next to the power series expansion of a single-valued or monodromic function, Riemann showed that such a function was known up to a constant as soon as functions are given which have the same infinities. In particular, if $f(z) = \infty$ when $z = a$ but $f(z)(z-a)^m$ does not, then f has an infinity of order m , with similar definitions for $a = \infty$ and for zeros of order m , called by Riemann points where f is infinitely small of order m .³¹ So if a function has only finitely many points, including $z = \infty$, where it is infinite, then it is a rational function, and if it is never infinite even at $z = \infty$, then it must be a constant. Riemann gave two proofs of Liouville's theorem: one by looking at the power series expansion, and another direct proof by observing

³¹ Plainly the condition $f(z)(z-a)^{m-1} = \infty$ is tacitly supposed.

that the integral $f(t) = \frac{1}{2\pi i} \int \frac{f(z)}{z-t} dz$ taken on a circle centre the origin approaches a constant value as the radius tends to infinity.

The last result in this collection was the fundamental theorem of algebra, but it required, said Riemann, some preliminary considerations of zeros and infinities of finite order. These were summed up in the theorem that the expression

$$\frac{1}{2\pi i} \int \frac{f'(z)}{f(z)} dz = \frac{1}{2\pi i} \int \frac{d \log f(z)}{dz} dz \quad (5.14)$$

evaluated on the boundary of a region is equal to the number of zeros minus the number of infinities inside the region, counted according to multiplicity. Two proofs of the fundamental theorem of algebra were then given. First by observing that a polynomial of order n is infinite to order n at $z = \infty$, but the expression (5.14) is zero for such functions. So the single n th order infinity must be balanced by a total of n zeros. Second by evaluating (5.14) for the function $\frac{f(z)}{z^n}$, and interpreting the answer, n . This, Riemann remarked, is essentially Gauss's third proof of the fundamental theorem, that is, Gauss (1816).

Riemann then gave a summary of how definite integrals can be evaluated by contour integration, before concluding the elementary part of his lecture course with a brief description of the branch points of a many-valued function.

The comparison with the version published by Roch is instructive. Faced with the question of the existence of a minimising function for the integral

$$L(\lambda) = \iint \left(\left(\frac{\partial \lambda}{\partial x} \right)^2 + \left(\frac{\partial \lambda}{\partial y} \right)^2 \right) dT \quad (5.15)$$

Roch first obtained some necessary conditions by looking at the variation of the integral. He found that the minimising function must satisfy the Cauchy–Riemann equations, that one must specify its value on the boundary in advance, and that it must change only by a constant value on crossing any cuts. He deduced correctly that an extremal for this integral is a minimum, and that it is unique if it exists, but he gave no argument for its existence. It is not clear if he thought it obvious, established by his mentor, or too difficult to secure; there is not even an appeal to Dirichlet's principle.

Riemann's avoidance of Dirichlet's principle here, when it generally plays such a central role in his thinking about complex functions, is intriguing. It had been present in the earlier lecture course, the one of 1855/56 which his friend Dedekind attended, as the content of Akt Nr. 37, mentioned above, attests.³² Here are many

³²The historians' first problem with these notes, however, is dating them. Immediately beneath the heading $\frac{55}{56}$ is a reference to Briot and Bouquet's book of 1859, which is confusing enough. However, the legible script is soon replaced by the traditional, and often impenetrable, German hand used for the bulk of the notes. The legible script returns on pages 15, 42, and 43 for a few more references, for example, to Cauchy and Hermite, but nothing that would betray a date. It

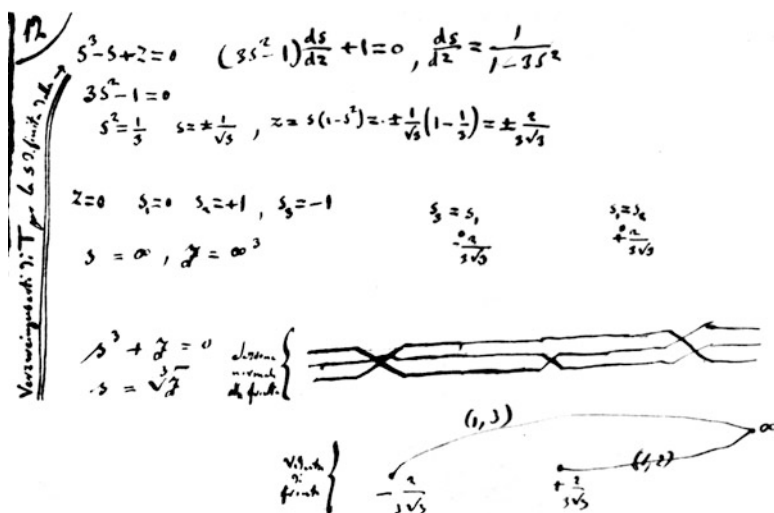


Fig. 5.3 Casorati's sketch of the Riemann surface $s^3 - s + z = 0$ (Neuenschwander "Der Nachlass von Casorati (1835–1890) in Pavia", *AHES* 19, Springer ©1978, p. 19)

more references to original papers, and progress to the study of algebraic functions is altogether more rapid. Something like Roch's account of the Riemann mapping theorem can, however, be found, on pages 19–28. Did he repent of its proof? Perhaps he merely withdrew it from what he judged to be the truly elementary part of the subject suitable for a lecture course.

Information from Roch's notes made its way into one of the first books on complex function theory (Casorati 1868a), for on his way to Berlin to visit Weierstrass in 1864 (as we discuss in Sect. 6.4.1) Casorati stopped for a week in Dresden to meet Roch and to become acquainted with the content of Riemann's lectures. In particular, Casorati asked Roch a number of questions dealing with the idea of a Riemann surface and Riemann's theory of Abelian functions that Roch was able to answer on the basis of his own lecture notes of Riemann's courses.³³

would seem therefore that the legible script was chosen for the titles of works in French and that the notes date from after 1859. But then one finds a clean start is made on page 111 with the date 1856. From then on the notes are the basis of Stahl's version of Riemann's theory of elliptic and Abelian functions, which Stahl himself said derived from Riemann's lectures of 1856 and 1861. The title of the lecture course is closest to the one listed in Riemann's *Werke* for 1861, which would make them Hattendorff's copy. The simplest interpretation is that these notes are of a course Riemann gave in 1855/56 with later additions made by Schering for his own use.

³³Casorati recorded his questions and Roch's answers in separate sheets held in Casorati's *Nachlass* in Pavia. A picture of one of them is given in Neuenschwander (1978b, 19). It deals with the Riemann surface associated with the equation $s^3 - s + z = 0$ and offers an intuitive drawing of it, see Fig. 5.3.

Casorati also asked Roch for a copy of Riemann's lectures, but he was only able to get the first half of them; Roch had sent the second half to Weierstrass and Kronecker in Berlin, where apparently they were lost.

5.5 Riemann's Theory of Elliptic Functions

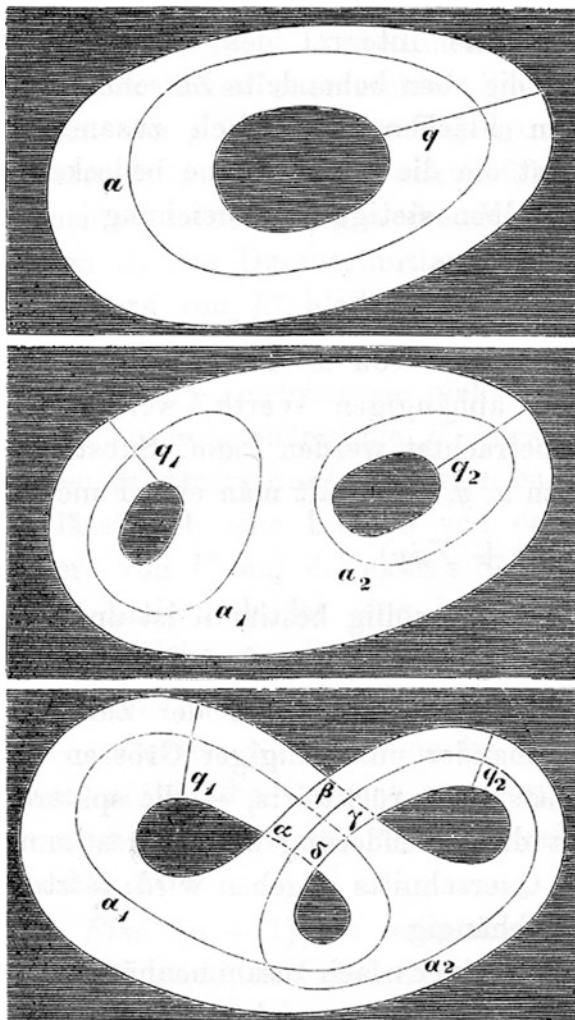
Riemann learned elliptic functions from the works of Jacobi, but also, intriguingly, from Gauss, as we learn from a letter of Betti to Tardy. Commenting on vol. 3 of Gauss's *Werke*, which had just appeared, Betti wrote to Tardy on January 4, 1869, to say "I saw these works on elliptic functions when I was with Riemann; there were many detached sheets, on almost every page of which Gauss had put the day and the year in which he wrote it" (in [Cerroni and Martini 2009](#), 129). Nonetheless, the specifically geometric interpretation of the theory seems to be Riemann's idea.

Riemann's lectures on complex function theory were usually offered before a course on elliptic and algebraic functions. Notes from the course of 1861 were published by Stahl in 1899, with a chapter on the presentation of the theory of theta functions taken from the course of 1856. The lectures are interesting because in them he showed for the first time how geometric considerations of the Riemann surface defined by the function $y^2 = \text{cubic or quartic in } x$, illuminated the whole study of elliptic integrals and elliptic functions. In so doing he solved a problem which had defeated both Jacobi and Cauchy and showed for the first time how Cauchy's approach to complex function theory could be made to apply to many-valued integrands. In the lectures he only dealt with the case when the roots of the quartic are either real or come in complex conjugate pairs, but there is no reason to suppose his analysis was restricted to this case (as had been Abel's and the first attempt of Jacobi's). The approach passed rapidly into the German textbooks [Durège \(1864\)](#) and [Schlömilch \(1866\)](#), as we shall discuss in Chap. 10.

Riemann took the Riemann surface, which he denoted T , cut it into a simply connected region T' by two cuts a and b , and showed that the elliptic integral in its normal form mapped T' onto a rectangle in the complex plane. This illustrated the many-valued nature of the integral and the double periodicity of its inverse. He showed that the periods arose as integrals along the cuts; they could now be seen to arise by integrating along non-contractible closed curves on a torus. He defined a theta function by the power series $\theta(v) = \sum_{-\infty}^{\infty} (-1)^j q^{j^2} e^{2ji\pi v}$ in which q is a real parameter and $|q| < 1$, and speedily obtained the Jacobian elliptic functions as quotients of theta functions, by writing down a quotient that had the right zeros and infinities, multiplying by an exponential factor to get the periods right, and then using a little trick to get the correct constant multiple ([Riemann 1899](#), 42). This is a rather attractive use of Liouville's theorem.

In the course of 1856 he had given an alternative account: he defined the period parallelogram, $P = 0, 1, \tau, 1 + \tau$ for the theta function, pointing out that it was not, strictly, a periodic function. Integration around the boundary showed that it had exactly one zero inside P , which could be found explicitly from the power series

Fig. 5.4 One doubly, and two trebly connected surfaces (Riemann, *Gesammelte Mathematische Werke*, 3rd ed., Springer ©1990, pp. 127, 128)



($v = \tau/2$). Thence, following Jacobi, the other three theta functions, and a suitable quotient ($x = a^2 \left(\frac{\theta_1(v)}{\theta(v)} \right)^2$) was then doubly periodic with a double infinity at $\tau/2$. From which it followed by counting infinities that $\frac{dv}{dx}$ was the square root of a cubic, and so $\sqrt{x} = \text{sn}(2Kv)$ (Fig. 5.4).

The lecture course of 1856 is therefore more traditional in its approach, but it is also interesting because it was a try out for some of the material that was later published in the great memoir of 1857 on Abelian functions, to which we now turn. Indeed, the paper on Abelian functions (or integrals, as we prefer to say today) originated from the very first course Riemann gave in Göttingen in 1854/55 as Privatdozent.

5.6 Riemann Surfaces and Complex Functions

The depth of originality in Riemann's remarkable paper (1857c) can be measured by the fact that on reading it no less an authority than Weierstrass withdrew a paper of his own on the same subject, preferring to wait until he had assimilated what Riemann had to say. In it Riemann tackled a problem which had been outstanding since the time of Jacobi, and with which Weierstrass had made his name by resolving a special case of: the study of the integrals of arbitrary algebraic functions. While Weierstrass had successfully treated integrals of the form $\int \frac{w dx}{y}$ on a curve with equation $y^2 = f(x)$, the so-called hyperelliptic case, Riemann dealt with integrals of the form $\int g(x,y) dx$ on any algebraic curve $f(x,y) = 0$ whatever, although details were given only for curves having at worst double points but no more complicated singularities. To accomplish this feat, Riemann showed how his ideas of 1851 could be extended to provide a remarkable theory of complex functions on almost any surface. Because Riemann's ideas are still difficult to describe, we have divided our exposition into two. The first part concentrates on Riemann's ideas about complex function theory; the second part on his approach to Abel's theorem and his new theory of theta functions and Abelian functions.

Riemann began by explaining his approach to complex functions once again. A lot depended on the concept of analytic continuation of a complex function. Without naming Cauchy, he observed at the start of his paper (1857c, 120) that:

According to a known theorem from the differential equation [the Cauchy–Riemann equations (5.1)] it follows that a function w , which is continuous and single-valued in a disk with center a and radius given by the distance from a to the discontinuity closest to a , can be expanded there in a power series $\sum a_n(z-a)^n$. Once the function is given on a however small a curve starting at a , the method of undetermined coefficients allows one to determine the coefficients a_n completely.

“Such reflections”, Riemann added, “should suffice to convince everybody “easily” that a function, which is given in a part of the (x,y) -plane, can be continued continuously from it onwards in one manner only” (Riemann 1857b, 121). As Klein was to observe (1926–1927, 1, 254), the method of analytic continuation of convergent power series was a suitable method for Riemann, it was a fundamental principle for Weierstrass and that was the crucial difference between the two.

Riemann then ran through the basic geometric aspects of his theory of complex functions. He indicated what he meant by single- and many-valued functions, cuts, and branch points. He described how a surface T spread simply or multiply over the complex plane (later he would show that such a surface is constituted by an algebraic curve) can be dissected by either closed curves or curves with end points on the boundary of the (partially dissected) surface until a simply connected surface T' is obtained. He defined a surface to be $(n+1)$ -fold connected if there is a system of n curves that does not disconnect it but any system of $n+1$ curves does separate it into at least two pieces. Here he spoke of the curves as forming a complete boundary

of a part of the surface. He indicated that the order of connectivity was independent of the choice of curves.³⁴

Riemann then showed how to think of the integrals of complete differentials on a surface. He indicated that the integral will be independent of the path between its end points once the surface has been made simply connected, but will jump if the path of integration crosses a cut. At this point he supplied figures to indicate some elementary types of connected surface.

Riemann then summarised his earlier use of Dirichlet's principle to define functions on the surface in this somewhat modified form: there is a unique function on T' having prescribed singularities and whose real part may be defined arbitrarily along the cuts. The preliminaries over, he indicated that his present aim was to study Abelian functions in this way and to solve the Jacobi inversion problem. He pointed out that Weierstrass had recently solved this problem in the hyperelliptic case, but added that he had not been idle meanwhile³⁵:

The present work, with the exception of the last two sections, 26 and 27, whose subject-matter could be touched upon only briefly in my lectures, is based on a part of my lectures in Göttingen from Michaelmas 1855 to Michaelmas 1856. As regards the discovery of particular results, those in Sections 1 to 5, 9 and 12, and the necessary preliminary theorems which I had to elaborate for my lectures in the manner explained in this memoir, were found in autumn 1851 and the beginning of 1852 in the course of researches on the conformal representation of multiply connected surfaces. I was, however, diverted from these investigations by another matter. It was not until Easter 1855 that I resumed my research, and during the Easter and Michaelmas vacations progressed as far as Section 21; the remaining sections were added by Michaelmas 1856. Some complementary results were added in various places while writing up the work.

He then began the first part of the paper proper.

5.6.1 *The Riemann Inequality*

Riemann began with an irreducible equation in s and z of degree n in s and m in z . He associated this with a n -sheeted surface spread out over the plane, which can, he said, be regarded either as one whose boundary is infinitely far away or as a closed surface. He chose to regard it as a closed surface T by adding a point on each of the n sheets corresponding to $z = \infty$ except when ∞ is a branch point. Thus the Riemann sphere slipped unobtrusively onto the stage.

Riemann now shifted attention to the abstract surface, noting that every rational function of s and z is single-valued on T and has the same branching behaviour as s , and he promised to prove the converse. The integral of a rational function yielded a many-valued function whose different analytic continuations differed only by constants, since their derivatives at the same point necessarily agreed. His method

³⁴His sketchy argument was later refined in [Tonelli \(1875\)](#).

³⁵See [Riemann \(1857c, 134\)](#).

for studying what he called (in Sect. 1) “equivalently branching algebraic functions and their integrals” was to “define them via their discontinuities, using Dirichlet’s principle”.

Riemann began by indicating how local coordinates can be used to define the order of vanishing of a function, in particular what is meant by the terms “infinitely small of the first order” (a simple zero in modern terminology) and “infinitely great of the first order” (a simple pole in modern terminology) and how a “discontinuous” function (i.e., one with infinities of some finite order) can be represented as a (Laurent) series by methods due to Cauchy. Next he supposed that the surface T was turned into a simply connected surface T' by a system of $2p$ cuts. To obtain a function on T' he took a function with first order zeros at finitely many arbitrarily chosen points $\varepsilon_1, \varepsilon_2, \dots$. He joined each point ε_j to another arbitrary point O by a system of non-intersecting curves ℓ_j that lie in T' . He now specified boundary conditions as follows: the values of a function on the two boundary edges of T' corresponding to the j th cut differ by a constant h_j , and the values on the positive (left-hand) exceed those on the negative sides of the curve ℓ_j by $-2\pi i A_j$. The constants h_j Riemann called the moduli of periodicity of the function. The constant A_j appears in the expansion of the function in local coordinates at ε_j :

$$A_j \log r_j + B_j r_j^{-1} + C_j r_j^{-2} + \dots$$

These boundary conditions determine a harmonic function inside the region T' from which the curves ℓ_j have been removed if and only if the sum of the A_j vanishes. Another such function is found by insisting that its moduli of periodicity are purely imaginary. The difference of these two functions is then a complex analytic function on T' , and so there is a complex analytic function on T' with the prescribed local behaviour and whose moduli of periodicity have arbitrary real parts. This function is determined up to an additive constant.

Riemann then constructed functions on T' as follows. The simplest are those many-valued functions w which are always finite, of which there are at most p linearly independent such, $w_1, w_2, \dots, w_p$. For, if $\{w_1, w_2, \dots, w_n\}$ is any such family, and the moduli of periodicity of w_j are $h_j^{(r)}$, then the modulus of periodicity of the linear combination

$$\alpha_1 \omega_1 + \alpha_2 \omega_2 + \dots + \alpha_n \omega_n \quad (5.16)$$

on the r th cut is

$$h^{(r)} = \alpha_1 h_1^{(r)} + \alpha_2 h_2^{(r)} + \dots + \alpha_n h_n^{(r)} \quad (5.17)$$

Write $\alpha_j = \gamma_j + i\delta_j$ and it is apparent that the real parts of the $2p$ quantities $h^{(1)}, h^{(2)}, \dots, h^{(2p)}$ depend linearly on the quantities $\gamma_1, \gamma_2, \dots, \gamma_n, \delta_1, \delta_2, \dots, \delta_n$. So if the functions w are linearly independent the quantities α can be determined so that the real parts of the moduli of periodicity all vanish, whence the real part of w vanishes and so w reduces to a constant. So there are at most p linearly

independent everywhere finite functions w . Following the classification of elliptic integrals, these were called integrals of the first kind by Riemann; they arise from integrating everywhere holomorphic integrands.

Then there are functions with a higher order infinity at a single point, called by Riemann integrals of the second kind; they arise from integrating rational functions without simple infinities. Riemann denoted them $\beta_j t_j$. Finally there are functions having two equal but opposite logarithmic infinities; these were called integrals of the third kind; they arise from integrating functions with simple infinities having opposite residues.

So, said Riemann, the general expression for a complex function with m simple infinities on a surface was of the form

$$s = \beta_1 t_1 + \beta_2 t_2 + \cdots + \beta_m t_m + \alpha_1 \omega_1 + \alpha_2 \omega_2 + \cdots + \alpha_p \omega_p + \text{const.}, \quad (5.18)$$

where the t 's were arbitrary and the α 's and β 's were constants. The $2p$ moduli of periodicity of this function s depend linearly on the $p + m$ α 's and β 's. So as soon as $m = p + 1$, $2p$ of the α 's and β 's can be determined (in terms of the others) so that all the moduli of periodicity vanish. The corresponding function s then depends linearly on $m - p + 1$ arbitrary constants, or, put another way, it is a sum of $m - p$ functions each having $p + 1$ infinities. In this way Riemann constructed linear spaces of functions having prescribed numbers of singular points on a surface. The result he obtained, which gives a lower bound on the dimension of such a space, is nowadays called the Riemann inequality.

The first interesting case is $m = p + 1$, for then there are $2p$ equations for $p + m = 2p + 1$ ratios between the α 's, the β 's, and the unknown constant. It follows that the function s contains two arbitrary constants, and so it does not reduce to a constant. The β 's are determined by the positions of the singular points (they are the residues at the singular points). It could be that for some of these positions the corresponding β 's vanish. Suppose that $m - \mu$ vanish, so that the function s has only μ infinities. These μ points must then be such that of the $2p$ equations between the remaining $p + \mu - 1$ ratios, $2p - (p + \mu - 1) = p - \mu + 1$ are a consequence of the rest. If, moreover, the values of k of these β 's determine the remaining $\mu - k$ of the β values, then $\mu - k = p + 1 - \mu$, and so $k = 2\mu - p - 1$ of the β 's are independent.

Suppose that the function is such that μ is as small as possible. Any first degree rational function in s has the same property, so the position of one of the μ points is arbitrary. The position of the others is determined, and so, if the branch points of the surface T are not to be subjected to any special equations, $p + 1 - \mu = \mu - 1$, or $\mu = \frac{1}{2}p + 1$. So in this case too the number of arbitrary constants in a function is $2m - p - 1$. Put another way, when $m = p + 1$, the smallest number of infinities a function can have is $\frac{1}{2}p + 1$ and the dimension of the space of such functions is $2m - p - 1 = p + 1$.

Riemann deduced that any such function s was the root of an equation of degree n in s whose coefficients were polynomial functions of degree m in z . In other words, s satisfies an algebraic equation of the form $F(s, z) = 0$, where F is a power of an irreducible polynomial. This theorem establishes that an abstract Riemann surface,

thought of as a branched covering of the sphere, can be thought of as a surface in $\mathbb{C}^2$. As such, it was to be the key to the passage from a Riemann surface to any of its equations, and so to be crucial in linking the highly abstract approach of Riemann to the more concrete, equation-based study of contemporary algebraic geometers.

It will be enough to understand the algebra in the simplest case, rather than in the full generality that Riemann gave it. Let us assume that the m infinities of s are all simple and (which is no restriction at all) that they occur at points for which the corresponding z values are all finite, say $\zeta_1, \dots, \zeta_m$. For an arbitrary value of z the corresponding values of s will be denoted $s_1(z), \dots, s_n(z)$. Then the function σ defined by the expression

$$(z - \zeta_1) \dots (z - \zeta_m)(\sigma - s_1(z)) \dots (\sigma - s_n(z))$$

is a single-valued function of z that is finite for all finite values of z and infinite of order m when $z = \infty$, so indeed s satisfies an irreducible algebraic equation of degree n in s and m in z .

To show that F is a power of an irreducible polynomial, Riemann argued that any rational polynomial factor of F vanishes for some of the roots $s_1(z), \dots, s_n(z)$ and so must, as a function of z , vanish in some part of T' . But T' is connected, and so the factor vanishes everywhere. Two factors can only vanish together if one is a multiple of the other, and so the conclusion follows.

Finally, Riemann observed that if w is any function defined on T and having only simple infinities, then $\frac{dw}{dz}$ is another function of the same kind. In particular it can only have infinities where the function w does. Therefore all functions defined on T are either functions branched like T or are integrals of such functions. Moreover, the system of such functions is determined when the surface T is given and depends only on the position of the branch points.

5.6.2 Branch Points and Coverings

Riemann next (Sect. 6) considered (rather obscurely, it must be said) the behaviour of single-valued functions on a given surface T as functions of the variable z alone. These functions have zeros and infinities of finite orders on T , and Riemann investigated the number and position of the branch points, and what happens at each branch point. He took as given one (many-valued) function s of z for which the corresponding surface is branched like T . He called a point where $\mu + 1$ branches came together a μ -fold branch point; in particular, if two branches came together at a point he said the point was a simple branch point. Riemann then argued that if all the branch points are simple then the maximum number of them was $2m(n - 1)$, which in language that was coming in but which Riemann did not use, is the degree of the resultant of F and $\frac{\partial F}{\partial s}$ as a function of z . Riemann argued that this is the number of repeated roots of $F(s) = 0$, and at such points $F'(s) = \frac{\partial F}{\partial s} = 0$. However, as

he noted, this number counts both simple branch points and double points (singular points at which branches are not interchanged). A double point can be understood as the coming together of two simple branch points (for example, as $a \rightarrow 0$, the simple branch points at $(s, z) = (0, 0)$ and $(0, a)$ on $s^2 = z(z - a)(z - b)$ come together and the equation becomes $y^2 = z^2(z - b)$ but the point $(0, 0)$ is no longer a branch point; it is a double point). So if there are w simple branch points and $2r$ points coming together in pairs to make r double points, then

$$w + 2r = 2m(n - 1). \quad (5.19)$$

Riemann also gave the simple test given by Plücker that distinguishes a double point from a branch point. At a double point both first derivatives $\frac{\partial F}{\partial s}$ and $\frac{\partial F}{\partial z}$ vanish, and $\frac{\partial^2 F}{\partial z^2} \frac{\partial^2 F}{\partial s^2} - (\frac{\partial^2 F}{\partial s \partial z})^2$ does not vanish. At a branch point, only $\frac{\partial F}{\partial s}$ vanishes while $\frac{\partial F}{\partial z}$ and $\frac{\partial^2 F}{\partial s^2}$ do not vanish.

Henceforth Riemann worked under the assumption that a given surface had only simple branch points and that more complicated cases could be obtained by letting simple branch points coalesce. By this he meant that if one has a sequence of simple branch points at which, successively, branches 1 and 2 are exchanged, branches 1 and 3 are exchanged, and so on until branches 1 and k are exchanged at the final branch point, then pushing them together gives a branch point at which the branches are permuted so that 1 goes to 2, 2 to 3, and so on with branch k exchanging with branch 1.

Riemann next observed (Sect. 7) that if one had a simply connected surface, then the number of its simple branch points and the number of times its boundary wraps round an interior point are related: the second number is one greater than the first. This is best understood by thinking of a branch point of high order as the limiting case of several simple branch points coming together. This simple observation could be generalised, he said, to a multiply connected surface, to provide information about the surface T that was “independent of metrical relations and belongs to analysis situs”. To prove this, Riemann again used Dirichlet’s principle, this time to give himself a suitably defined branch of the function $\log z$ on the cut-up surface T' —this amounts to a use of the Riemann mapping theorem. A Cauchy-integral type argument following the function round all the cuts needed to produce T' from T then established that

$$w - 2n = 2(p - 1) \quad (5.20)$$

whence

$$p = (n - 1)(m - 1) - r. \quad (5.21)$$

Both these formulae are interesting. Brill and Noether (1894, 272) called the first result “very remarkable”, but they replaced Riemann’s topological argument with an algebraic one based on the fact that everywhere finite integrands on the surface have

the form $\frac{\phi(s,z)}{\frac{\partial F}{\partial s}}$. The first result is now known as the Riemann–Hurwitz formula,³⁶ and we note that it was established using the contentious Dirichlet principle. The second result is due to Clebsch and relates the genus to the degree of the defining equation and the number of double points.

Riemann then showed in Sect. 8 how a function on T having a specified number of simple infinities could be represented as a quotient of two polynomial functions in s and z , and so as a rational function on T , as he had promised to do. In particular, the functions $\frac{dw}{dz}$ are of this kind, and it followed from the details of the construction of the rational functions themselves that

$$w = \int \frac{\phi(s,z)dz}{\frac{\partial F}{\partial s}} = - \int \frac{\phi(s,z)ds}{\frac{\partial F}{\partial z}}, \quad (5.22)$$

where the function ϕ is of degrees $n-2$ in s and $m-2$ in z . By counting constants and allowing for r double points, Riemann confirmed that there were indeed p linearly independent expressions of this kind.

Prym's later objection to this passage is indicative of the problems Riemann made for his readers. In his paper (Prym 1877) Prym rightly observed that while Riemann's proof that every rational function of the variables s and z on a Riemann surface will have a finite number of simple poles is valid, the converse claim that Riemann made in Sect. 8 was not properly established by him. He also noted that while Betti had subsequently given a different and more algebraic proof of the uncontested first part, he had reproduced the dubious second part almost unaltered in his (Betti 1862). However, Riemann's argument proceed by counting constants and so it was flawed by Roch's later contribution (the Riemann–Roch theorem, see p. 317). This showed that the dimensions of the spaces of functions (the number of available constants) could be affected by the position of the poles. The consequences of this being impossible to oversee, as Prym put it, he gave instead a different proof that took greater care with the nature of the divisors (the sets of poles) and the consequent algebra.

Riemann himself had proceeded to consider what could be done with the equation representing a given surface. A function z_1 branched like T and having n_1 simple infinities provides a conformal map from T to a surface T_1 , and all the rational functions and their integrals on T can be taken over to T_1 . If s_1 is another function branched like T and having m_1 simple infinities, then not only do z_1 and s_1 satisfy an equation of the form $F_1(s_1, z_1) = 0$, but this equation can be obtained from the original equation $F(s, z) = 0$ by a rational change of variable. It follows that

³⁶Hurwitz's contribution is the formula $2P-2 = w + n(2p-2)$, where P is the genus of a Riemann surface that is an n -fold cover of a Riemann surface of genus p branched at points $a_1, a_2, \dots, a_w$ at which $c_1, c_2, \dots, c_w$ leaves come together, so the total branching order is $W = (n-c_1) + (n-c_2) + \dots + (n-c_w)$. It is given in Hurwitz (1893, 416), reprinted in *Mathematische Werke*, I, nr. XXIII, 391–430, see p. 404, not in XXI, p. 376 as stated in (Freudenthal, 1972, 572).

either equation can be taken to represent the surface, and Riemann indicated what could be obtained as the simplest type of equation for a surface of a given genus.

Of greater importance, in Sect. 12 Riemann also counted the number of different equations there were that described Riemann surfaces of a given genus (greater than one), where different means not to be obtained one from another by means of a rational transformation. He supposed that the surface T was $(2p + 1)$ -fold connected, and that ζ was a function on it with μ simple infinities. Then it depended on $2\mu - p + 1$ arbitrary constants, and the number of branch point values of functions branched like ζ depended on $2(\mu + p - 1)$ arbitrary constants. So $2\mu - p + 1$ of these branch point values could be determined, leaving $3p - 3$ out of reach. Since there was a surface for any set of branch point values, it followed that the family of inequivalent surfaces of a given genus depended continuously on $3p - 3$ complex parameters which, he said, "shall be called the moduli". The space of moduli has continued to attract a great deal of interest ever since. A short separate argument then showed that there is a one-parameter space of all Riemann surfaces of genus 1.

5.7 Riemann's Paper of 1857 and Abelian Functions

The second half of Riemann's paper was devoted to the study of rational and algebraic functions on a given Riemann surface. For this Riemann had the idea of generalising what he called Jacobi's theory of theta functions to functions of several variables and showing that quotients of products of the new theta functions represented algebraic functions. So it is interesting to note that in the opinion of Dedekind and Stahl, Riemann was unaware of Jacobi's attempts in this direction and learned about theta functions of several variables only from the papers of Göpel and Rosenhain (see Sect. 4.5), whose names Riemann did not mention. Indeed, several passages in his lectures on elliptic functions had been taken over verbatim from Rosenhain.

Riemann's presentation can be briefly summarised, if one is willing to lose a considerable amount of detail. It begins in the last three sections of the first part of his paper with some theorems derived from what Riemann called Abel's theorem and treated, following Rosenhain (and indeed Jacobi) as a theorem about systems of p first order differential equations in $p + 1$ unknowns. Recall that Abel (see Sect. 4.4) had considered a sum of everywhere finite integrals (with a suitable integrand) on a given curve with a fixed initial point and variable upper endpoints, $\{u_1, u_2, \dots, u_m\}$, say. He had shown that such a sum was always equal to the negative of a sum of p such integrals with the same integrand but with different upper end points, $\{v_1, v_2, \dots, v_p\}$, say, and that the new end points were algebraic functions of the original ones. This could be rephrased as saying that certain sums of integrals vanished and that when they did their upper end points were algebraically related. Jacobi had shown that there was a linear space of such integrands of dimension p , so one could choose a basis for such a space and by using each basis element in turn as the integrand obtain a set of p equations simultaneously satisfied by the endpoints.

Differentiating them gave a set of p differential equations satisfied by the endpoints. This was the form of Abel's equation that Jacobi had preferred.

Riemann argued that if $w^j(t)dt$ was such an integrand, then any system of p differential equations of the form

$$w^j(t_1)dt_1 + w^j(t_2)dt_2 + \cdots + w^j(t_{p+1})dt_{p+1} = 0, \quad j = 1, \dots, p \quad (5.23)$$

was solved by a set of $p + 1$ variables t_k such that p of them are functions of the remaining one, and for which one arbitrary set of values of the variables, say $t_1^0, t_2^0, \dots, t_{p+1}^0$ satisfies the equation. Now by the Riemann inequality there will be a function ζ on the Riemann surface such that the function $\frac{1}{\zeta}$ has $p + 1$ simple infinities at arbitrary points. The function ζ plainly has simple zeros at these points and they are arbitrary. In the generic case, the space of such functions has dimension 1, so ζ is unique up to multiplication by a constant. Since the function takes every value equally often, it takes every value $p + 1$ times. It follows that a point t_{p+1} for which ζ takes a prescribed value ζ_0 say (so $\zeta(t_{p+1}) = \zeta_0$) determines p other points $t_1, \dots, t_p$ at which ζ takes the same value ($\zeta(t_k) = \zeta_0$ for $k = 1, \dots, p$). So the existence of this function yields a solution to the system of differential equations. Moreover, if the rational function ζ can be written as a quotient of two polynomials χ and ψ , so $\zeta = \frac{\chi}{\psi}$, then the points are the common zeros of the equation of the Riemann surface and the equation $f(t) = \chi(t) - \zeta(t)\psi(t) = 0$.

If one reformulates Abel's theorem in terms of a fixed upper end point for the integrals and variable lower ones and subtracts these integrals from the ones described above, one obtains a formulation in which the upper and lower end points in a sum of integrals that vanishes are related algebraically. In Riemann's formulation, this says that the upper and lower end points are the zeros and infinities of a rational function on the Riemann surface.

5.7.1 Theta Functions

At the start of the second section proper, Riemann defined a theta function of several variables:

$$\theta(v_1, v_2, \dots, v_p) = \sum_{-\infty}^{+\infty} e^{\sum a_{jk} m_j m_k + 2 \sum v_j m_j}. \quad (5.24)$$

This is an infinite sum of exponentials, where the sum is over all p -tuples of integers and the exponential terms involve a symmetric negative definite $p \times p$ matrix (a_{jk}) . These conditions are imposed to ensure the convergence of the sum. Like their single variable counterparts, theta functions of several variables are designed to satisfy certain properties.

In the single variable case, the theta function is

$$\theta(v) = \sum_{m=-\infty}^{m=\infty} e^{m^2 a + 2mv},$$

and it satisfies the identities

1. $\theta(v + \pi i) = \theta(v)$;
2. $\theta(v + b) = e^{-(2v+b)} \theta(v)$.

Indeed the function is characterised up to a constant multiple by these equations. The same is true of the several variable theta function: its value does not change if any v_j is replaced by $v_j + \pi i$ and

$$\theta(v_1, \dots, v_p) = e^{2v_j + b_{jj}} \theta(v_1 + b_{1j}, \dots, v_j + b_{jj}, \dots, v_p + b_{pj}).$$

So each theta function in p variables comes with a lattice (to use a more modern term) in $\mathbb{C}^p$, the one spanned by the vectors that are the real and imaginary parts of the columns of the matrices $(\pi i)I$, where I is the identity matrix, and (b_{jk}) . If the variable moves along the lattice in some basis directions the theta function does not alter, while if it moves along the other basis directions it increases by an exponential factor. The matrix in the theta function and the lattice determine one another, up to conjugation and change of basis respectively.

Each Riemann surface of genus p also defined a lattice, Λ_p , where the j th basis vector is composed of the jumps of the j th everywhere finite integrand on each of the $2p$ cuts. A natural question is: does the lattice coming from a Riemann surface determine a matrix that gives a theta function? Riemann showed that the answer is yes, it does. Recall that for each integral $\int w_j$ it is possible by a change of basis argument to arrange that $\int_{a_k} w_j$ vanishes unless $j = k$, when the value of the integral is πi . The values $\int_{b_k} w_j = b_{jk}$ must form a negative definite symmetric matrix, a fact Riemann established by contour integration.

Riemann then defined a many-valued map from a given Riemann surface of genus p to $\mathbb{C}^p$ as follows. First he cut up the Riemann surface by a system of $2p$ cuts $\{a_j\}$ and $\{b_j\}$, $j = 1, \dots, p$ to obtain a simply connected domain T' . We shall assume, as we may, that these cuts have a common start and finish point O ; Riemann joined them by ancillary cuts c_j which he incorporated into his analysis, but nothing is lost by ignoring them here. Riemann then supposed $z \in T'$ and defined a many-valued function as follows:

$$z \mapsto \left(\int_{z_0}^z \omega_1, \dots, \int_{z_0}^z \omega_p \right) = \mathbf{u}(z), \quad (5.25)$$

where $\omega_1, \dots, \omega_p$ are a basis for the everywhere finite integrands upon it.

Just as he did in the lectures on theta functions of a single variable, he studied the theta function of T' by applying the methods of Cauchy. For any single-valued

function f , contour integration round the boundary of $d \log f$ will yield the number of zeros and infinities of defined in T' in terms of the value of the integral of that function along the individual pieces $\{a_j\}$ and $\{b_j\}$. In the case of the theta function, it can never be infinite in T' , so integration around the boundary of T' (Sect. 22) showed that for any vector $\mathbf{e}$ in $\mathbb{C}^p$ the theta function $\theta(\mathbf{u}(z) - \mathbf{e})$ as a function of $z \in T'$ has p simple zeros that depend on the choice of $\mathbf{e}$. These he denoted $\eta_1, \dots, \eta_p$, and he defined $\alpha_j^k = u_j(\eta_k)$.

To obtain a domain T^* on which $\log \theta$ was single-valued he introduced further cuts ℓ_j from the points of η_j to O . A more complicated contour integral calculation, which we omit (Sect. 22), then showed that the arbitrary constants that entered the above arguments can be chosen so that, modulo the period lattice, $\log \theta \left(\dots, u_j - \sum_k \alpha_j^k, \dots \right)$ is a function with logarithmic infinities at the points $\eta_1, \dots, \eta_p$ and jumps of $-2\pi i$, 0, and $-2 \left(u_j - \sum_{k=1}^p \alpha_j^k \right)$ at the cuts ℓ_j , a_j , and b_j , respectively.

Riemann then established three fundamental facts. First, if the theta function does not vanish identically, then each set of points $\eta_1, \dots, \eta_p$ corresponds to a unique point in $\mathbb{C}^p / \Lambda_p$. Second, the canonical differentials dw_j each have $2p - 2$ zeros. Third, the values of $\log \theta(0)$, the so-called theta null values, which depend on the moduli for the curve, can be used as a source of knowledge about the moduli.³⁷ As Riemann pointed out, his approach here was the straight-forward generalisation of Jacobi's determination of the value of $\theta(0)$ in the theory of elliptic functions. It led to family of first order partial differential equations for $\theta(0)$ generalising those discovered by Weierstrass in the hyperelliptic case, and which, said Riemann, require a theory of differential equations with algebraic coefficients for the treatment. He proposed to return to this theme at a later date, but never published anything; material from his lectures and his *Nachlass* occupy several pages of his *Werke*. The study of the theta null values was therefore central to the function theory of a Riemann surface and was to be taken up by Clebsch and Gordan, Weierstrass, and Klein, as we shall see.

Riemann also showed how algebraic and rational functions on a Riemann surface could be represented explicitly as quotients of products of theta functions, multiplied by an exponential factor, just as Jacobi had represented elliptic functions and as Riemann had too, in his lectures. Riemann considered expressions of the form $\frac{P}{Q} \cdot e^{-2\sum h_k u_k}$, where the h 's were integers, numerator and denominator were each products of the same number of theta functions, and the theta functions have different zeros. Arguments about the jumps across cuts that were standard by now showed that every algebraic function was necessarily of such a form. Finally, Riemann showed that algebraic functions on T that were not rational could be written in this form and P and Q were each a single theta function.

³⁷See Mumford (1975).

5.7.2 *Riemann's Abelian Functions in Review*

This monumental paper can justifiably be taken as the central one in his study of functions of a complex variable, but it by no means exhausted it. Riemann's paper found its continuation in his WS 1861/62 "Lectures on the general theory of the integrals of algebraic differentials" (Riemann 1990, 597–666) and in the paper (Riemann 1865)—the last one he published during his life. According to Narasimhan (in Riemann 1990, 9–10) "taken together, these works constitute one of the great treasures of mathematics", which is at the basis of a number of major subjects of modern mathematics. Among them Narasimhan lists the following:

1. The structure and topology of compact surfaces.
2. The relationship of the topology of a compact Riemann surface (or a compact manifold in general) to analysis and function theory on the surface (manifold) in particular, the Riemann–Roch theorem.
3. The use of variational principles to study analysis on a compact manifold.
4. The intimate relationship between the geometry of an algebraic curve and its Jacobian variety. [...]
5. The birational geometry of plane curves. This led to the study of the birational geometry of varieties in general and provided a powerful impetus to algebraic geometry.
6. The study of the family of all (isomorphism classes of) compact Riemann surfaces of a given genus. This has led, inevitably, to the study of families of higher dimensional varieties, thus to deformation theory and moduli problems.

In his lectures Riemann pursued two other natural connections between the paper and the existing theories of theta functions and elliptic functions. Jacobi's theory of theta functions had presented not one but four, all represented elegantly by Riemann in his lectures. The theory of hyperelliptic functions of genus 2 due to Göpel and Rosenhain had presented 16 theta functions of two variables. Riemann saw that his theory naturally associated with a Riemann surface of genus g a set of 2^{2g} theta functions, and in his lecture course he described them and their geometrical significance. We shall look at this further in Sect. 5.11.6, when we briefly consider the history of algebraic curves. The theory of elliptic functions (strictly, elliptic integrals) as presented by Legendre naturally focused on a differential equation that Gauss had then situated within the theory of the hypergeometric equation. This prompted Riemann to look at such differential equations from the perspective of his own theory of complex variables, as we shall now see. That done, it will be possible to examine the thorny question of the responses to Riemann's paper of 1857.

5.8 Complex Functions and Differential Equations

Riemann thought of an algebraic function geometrically. Even if the starting point was not a surface representing it conformally, or even an equivalence class of such surfaces, but was still quite closely linked to an equation, nonetheless the drift of his analysis was clear: an algebraic function is to be regarded as a surface of a certain type and can be precisely specified by describing its branch point behaviour and hence the moduli. In this context there is no surprise that a function is specified by requiring that it pass through a few points; that is what algebraic curves do. The great novelty was in defining functions on such a surface and not on the z -plane that the surface covers.

Another surprisingly powerful idea was his topological treatment of the surface, and his analysis of how this determined the nature of the functions and their integrals that the surface could support. In Ahlfors' opinion, the Riemann surface "is a skillful fusion of two distinct and equally important ideas:

1. A purely topological notion of covering surface, necessary to clarify the concept of mapping in the sense of multiple correspondence.
2. An abstract conception of the space of the variable; with a local structure defined by a uniformising parameter. The latter aspect comes to the foreground in the treatment of branch points" (Ahlfors 1953a, 4).

What Riemann did for algebraic curves and their integrals he also did for differential equations, which is perhaps more unexpected. He published a paper on a case of Bessel's equation that arose in a physical problem (Riemann 1855), and a short but profound analysis of the hypergeometric equation (Riemann 1857a). We also know from his lecture notes and the *Nachlass* that he had in mind a general theory applicable to all linear ordinary differential equations. His point of view, as in the algebraic case, was to think of a differential equation as defining a function, and to think of this function geometrically as a surface spread out over the z -plane. Then, because a solution to an n th order differential equation is given by specifying it and its first $n - 1$ derivatives at a point, it should be possible to specify a solution exactly by just a finite amount of information. Once you think in this way, some things are obvious. The usual approach was, and remained, to think locally, and to write down n linearly independent power series at a point, each representing a solution of the differential equation. From a Riemannian point of view, it would seem likely that these solutions will all be branches of one solution function. The data one has about the first $n - 1$ derivatives picks out, to be more precise, a branch of a solution. If indeed the local approach is picking out branches of the same function, then of course there must be points where these branches join up: branch points. Next, and this marks a difference with the algebraic case, the number of branches will often be infinite. It becomes an interesting question to ask when the number of branches is finite and the solutions to a differential equation are algebraic functions.

The paper on Bessel's equation was published in 1855. Strictly speaking, Bessel's equation does not appear in it, but a partial differential equation does that yields

Bessel's equation on applying the method of separation of variables. What this paper demonstrates is Riemann's facility at contour integration and his willingness to use series that do not converge. For these reasons, the appropriate place to consider it is in Sect. 7.5.

5.8.1 Riemann's *P-Functions*

From that paper, which is one of the first to use complex variable methods in the study of a differential equation, it is still a long step to Riemann's confident handling of the hypergeometric equation with the methods of his complex function theory. This appeared in his short paper (1857a). There Riemann stated (1990, 99) that his new method for dealing with complex functions could be applied "essentially to any function which satisfies a linear differential equation with algebraic coefficients". Very little had been said about differential equations for functions of a complex variable before then. Gauss's own study of the hypergeometric equation (discussed in Sect. 1.5.2) was still unpublished, although Riemann tells us that he had read it, and although Kummer's published study (Kummer 1836) had concluded with a section where the variable became complex, this was little more than a formal extension of the real case. Cauchy (1846i) had indicated that one might think of a differential equation on a surface instead of an elliptic integral over the z -plane, but had speedily retreated from this insight. The field was completely open. What was known was Kummer's discovery that the (real) hypergeometric equation admits 24 interesting solutions, which have useful relations between them, and this discovery provided the context for Riemann's own investigations.

Kummer's 24 solutions to the hypergeometric equation (1.71)

$$(z - z^2) \frac{d^2 F}{dz^2} + (\gamma - (\alpha + \beta + 1)z) \frac{dF}{dz} - \alpha\beta F = 0 \quad (5.26)$$

are obtained as follows. The equation has three singular points, the points $z = 0$, $z = 1$, and $z = \infty$. There are six interesting changes of variable which permute these points around:

$$z \mapsto \frac{1}{z}, z \mapsto 1 - z, z \mapsto \frac{1}{1 - z}, z \mapsto \frac{z}{z - 1}, z \mapsto \frac{z - 1}{z} \quad (5.27)$$

and the identity. There are four solutions valid in a neighbourhood of the origin that are of the form hypergeometric series in z multiplied by some power of z and some power of $1 - z$, and the six transformations applied to these four yield the 24 solutions. In regions where three of the 24 are all defined, there must be a linear relation between them, and Kummer indicated how they could be found. An example illustrates what he did. Three solutions are

$$f_1 = F(\alpha, \beta, \gamma, z), \quad (5.28)$$

$$f_2 = F(\alpha - \gamma + 1, \beta - \gamma + 1, 2 - \gamma, z)z^{1-\gamma}, \quad (5.29)$$

$$f_3 = F(\alpha, \beta, \alpha + \beta - \gamma + 1, 1 - z). \quad (5.30)$$

Kummer found that

$$f_1 = af_2 + bf_3, \quad (5.31)$$

where a and b were constants determined by the coefficients α, β , and γ of the hypergeometric equation in a way that involved Gauss's factorial function.

Riemann began his paper by observing that it would be possible to study the solutions to the complex hypergeometric equation by expressing them as integrals (as Kummer had done in the real case), but that this required extending the theory of complex integrals. Consequently, he preferred another route which would lead to the main results without too much troublesome calculation. As he stated in the "Personal summary" of his (1857a) paper (Riemann 1857b, 117), he was pleased to remark that by his method he could obtain "almost without any calculation" the results that Gauss, Kummer and others had found earlier by laborious calculations. Both Gauss and Kummer (1836) had based their research on the Eq. (5.26). With his method, Riemann added (1990, 99), the previous results follow "almost directly from the definition". As Narasimhan (in Riemann 1990, 17) has observed, in this paper "Riemann not only ignores any analytic expression of the function, he even ignores the differential equation it satisfies". This is typical of his thorough-going conceptual approach, which emphasised the importance of characterising the problem in abstract terms before proceeding to the detailed calculations (not that Riemann disdained calculation). To this end he proposed to study a complex function P —he took this piece of notation directly from Gauss's unpublished study (Gauss 1812b)—having three branch points, $z = a, b$, and c , such that any three branches of the function satisfy a linear relation, and such that near each singular point the function satisfies the following condition (here stated for the branch point $z = a$). There are constants α and α' such that P is a linear combination of two branches $P^{(\alpha)}$ and $P^{(\alpha')}$ near $z = a$, and $(z - a)^{-\alpha} P^{(\alpha)}$ and $(z - a)^{-\alpha'} P^{(\alpha')}$ are single-valued and neither zero nor infinite at $z = a$. So the function P is determined by these rules, the three points $z = a, b, c$, and the six so-called exponents $\alpha, \alpha', \beta, \beta', \gamma$ and γ' .

The analogy with the solutions of a second-order linear differential equation are clear, and Riemann set to work to show that indeed the P -function and the hypergeometric function are essentially the same. Without any loss of generality he supposed that the points $z = a, b$, and c are the points $z = 0, \infty$, and 1 , respectively. Then he considered how the branches of the P -function behave under analytic continuation around a branch point. It is enough to consider two independent branches, P' and P'' . After being continued around the branch point $z = a$, they return as P'^* and P''^* , say, and by hypothesis these branches are linear combinations of the originals:

$$\begin{aligned} P'^* &= a_1 P' + a_2 P'', \\ P''^* &= a_3 P' + a_4 P''. \end{aligned} \quad (5.32)$$

So what happens near $z = a$ is determined by a matrix³⁸ $A = \begin{pmatrix} a_1 & a_2 \\ a_3 & a_4 \end{pmatrix}$. There are similar matrices B and C to describe what happens near $z = b$ and $z = c$, respectively. A curve that surrounds all three branch points can be regarded as enclosing none, so Riemann obtained the equation

$$CBA = \begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix}.$$

A suitable choice of P' and P'' at each branch point then showed that this equation implied that $\alpha + \alpha' + \beta + \beta' + \gamma + \gamma'$ had to be an integer.

One could choose branches of the function P so that the monodromy matrix $A = \begin{pmatrix} e^{\alpha 2\pi i} & 0 \\ 0 & e^{\alpha' 2\pi i} \end{pmatrix}$ and the whole monodromy behaviour was therefore given by eight constants. A more complicated argument about analytic continuation showed that of those eight constants, three are known when the remaining five of them are. These can, he said, be determined once the P -function is given, but Riemann did not do so explicitly. However, as the first editors of his *Werke* pointed out, the expressions for all eight coefficients had been found explicitly by Riemann in July 1856. Instead, Riemann offered another approach. First he showed that up to a constant the function P is determined by a choice of the three branch points and the six exponents. This followed by supposing that P_1 was another P -function with the same coefficients on taking a careful look at the behaviour of the quotient $\frac{P}{P_1}$.

Riemann then considered what happens when the exponents of a P -function are varied by integers. This is reminiscent of Gauss's study of what he had called contiguous functions. It followed from Riemann's study of the monodromy matrices that changing the exponents by integers does not change the matrices. Consequently their behaviour under analytic continuation was the same, and so Riemann could deduce what Gauss had shown before him: any three contiguous P -functions satisfy a linear relationship whose coefficients are functions of z . As an example of three such functions, Riemann took P , P^α , and $P^{\alpha'}$, where for simplicity he now assumed that the exponent $\gamma = 0$. From the contiguity relation he deduced a differential equation that was satisfied by P , and by looking at a power series expansion of a solution he deduced (1990, 113) that

$$P^\alpha \begin{pmatrix} 0 & \beta & 0 \\ \alpha' & \beta' & \gamma \end{pmatrix} x = \text{Const. } F(\beta, \beta', 1 - \alpha', x) \quad (5.33)$$

³⁸Monodromy matrices were used in [Hermite \(1851, 279\)](#), the term monodromy group first appears in [Jordan \(1870, 278\)](#).

or

$$F(a, b, c, x) = P^\alpha \begin{pmatrix} 0 & a & 0 \\ 1 - c & b & c - a - b \end{pmatrix}^x. \quad (5.34)$$

From all this it was then simple for Riemann to write down the expression for the general P -function with arbitrary exponents as a hypergeometric function.

The paper concluded with a study of what happens when the variable is transformed by any of the six transformations considered by Kummer. By considering the domain of convergence of a branch of a P -function [Riemann \(1857a, 115\)](#) finally deduced that “there are in general 24 different hypergeometric series that can represent a P -function, . . . , of which half converge for any given value of z ”.

Riemann’s first editors, Dedekind and Weber, included a lecture he had given in 1857 in which he generalised these ideas to ordinary differential equations of order n and characterised those equations whose solutions have only finite infinities. This work, although incomplete and in part erroneous, came close to foreshadowing discoveries Fuchs published in 1870, and we shall consider it again in Sect. 7.3.1 when Fuchs’s papers are described. In the *Nachträge*, published in the second edition, the new editors, Noether and Wirtinger, included more from that lecture course, which showed how much of Schwarz’s ideas about when the hypergeometric equation has only algebraic solutions had been known earlier to Riemann and also how Riemann had extended similar considerations to the special case of Legendre’s differential equation and thus illuminated the elliptic modular function in the manner later published by Dedekind and Klein.³⁹

Riemann was well aware of the far reaching implications of the basic ideas of monodromy introduced in this paper. As Narasimhan has remarked (in [Riemann 1990, 17](#)), this is clear from both the fragment ([Riemann 1990, 411–422](#)) and a course of lectures Riemann gave in WS 1858/59. The basic idea of the theory of algebraic differential equations outlined in this fragment was later and independently developed by Fuchs (see [Gray 2000a](#)). The problem of the “determination of the form of the differential equation”, which is the problem Riemann tackled in the second part of the fragment and in his WS 1858/59 lectures, is at the origin of the “Riemann problem” on the existence of linear differential equations with prescribed monodromy group, listed by Hilbert in 1900 as the 21st among the 23 Mathematical Problems he presented at the Paris Congress.

Riemann devoted his WS 1856/57 lectures to the theory of hypergeometric series. Here, as first noted in [Laugwitz and Neuenschwander \(1994, 64\)](#), Riemann outlined, in symbols and without any words, a proof of the Cauchy–Hadamard formula for the radius of convergence of a power series. They go on to speculate plausibly that the absence of this argument from Riemann’s publications contributed the belief that the formula was ignored until Hadamard rediscovered it.

³⁹In this connection, see Wirtinger’s ICM 1904 address ([Wirtinger 1905](#)) for an account of just how many later discoveries were foreshadowed by Riemann’s work on the hypergeometric equation.

In WS 1858/59 he took up the subject (and monodromy) from another point of view. First he gave a treatment of P -functions in terms of the (Eulerian) integrals of the form

$$\int s^a(1-s)^b(1-xs)^c ds$$

between the limits $\{0, 1, \infty, x^{-1}\}$, then he considered the second order differential equation $a_0 y'' + a_1 y' + a_2 y = 0$. He denoted the ratio $\frac{Y_1}{Y_2}$ of two independent solutions Y_1 and Y_2 of this differential equation by z , so under analytic continuation along a closed path (“on which a_0, a_1, a_2 again take the original values”) z is transformed in $z' = \frac{az+b}{cz+d}$. The inverse function $x = f(z)$, Riemann remarked, has the property that $f(z) = f\left(\frac{az+b}{cz+d}\right)$. “Now, let us assume”, Riemann continued (1990, 674), “that we have a function which has the property of being unchanged under certain substitutions of this kind, and let us take up the task of deriving from this the differential equation upon which the function is connected”. Looking for the solution of his “task” Riemann was led to consider (a constant multiple of) what is today called the Schwarzian derivative. He applied this to the hypergeometric equation, considering $z = P/P'$ (where P and P' are two independent solutions of the same hypergeometric equation, and its inverse function $x = f(z)$, and he asked the question: “in which cases is there an algebraic relationship between two such inverse functions x and x_1 ?”. He related the answer to the problem of conformal mappings of triangles on the sphere onto the upper half-plane.

“In principle”—Narasimhan has remarked⁴⁰—“this includes many questions dealt with separately by others much later, in particular that of determining those cases of the hypergeometric equation for which the set of monodromy transformations is finite”. The ideas presented in these lectures were also applied by Riemann to his (unpublished) work on minimal surfaces and independently rediscovered and developed by Schwarz many years later. Furthermore, went on Narasimhan, “in the hands of Klein and Poincaré, these ideas were to develop into the imposing edifice of automorphic functions and uniformisation” (see also (Gray 1986 and 1994)). So we defer further consideration of these ideas of Riemann’s until this later work has also been discussed (see Sect. 8.2.4).

We also defer discussion of the note (Riemann 1863) that was only published posthumously, in which he investigated how the quotient of two P -functions could be written as a continued fraction. Gauss had considered this topic in the second part of his paper on the hypergeometric series (Gauss 1812a, 134–138). The method is connected with the so-called method of steepest descent later used by Debye in 1909, and so we discuss it in Sect. 7.5.1. It is related to another asymptotic approach, the method of stationary phase, which we shall not discuss.

⁴⁰See Riemann (1990, 19).

5.9 Extending the Riemann Mapping Theorem

Riemann made no distinction in kind between surfaces which were simply connected and those of multiple connectivity. On those grounds alone one would expect to find him trying to extend the Riemann mapping theorem to multiply connected domains, and indeed he did. The problem he now faced is that there will be inequivalent domains of the same topological type. These will have different moduli, but in the absence of any way of computing the moduli of a given surface talk of moduli is so many empty words. Nonetheless, it is possible to specify the right number of inequivalent domains of as simple as possible a form, and to formulate a theory of the conformal equivalence of any domain with exactly one of these canonical domains, and this is what Riemann did. His ideas were edited for publication after his death on the basis of a few manuscript notes and published for the first time as [Riemann \(1876b\)](#) in his *Werke*. They form the material in which Brill and Noether saw the germ of Riemann's later ideas (see [Brill and Noether 1894](#), 259), but in the absence of any dating supplied by the original source itself the sophistication of the treatment makes this seem unlikely. Certainly at one point in the published version the notes refer, parenthetically, to the paper on Abelian functions.

[Riemann \(1876b\)](#) started from a problem in physics: the equilibrium distribution of temperature on a domain S bounded by n disjoint circles. He argued that the temperature distribution would be known once one had a complex function $\zeta = \xi + i\eta$ taking every real value including ∞ exactly once on each boundary circle, and inside S it would be a complex function with positive imaginary part taking every value exactly n times. Thus S is represented conformally as an n -fold covering T of the upper half plane whose boundary curves are mapped to the real axis. It follows that T must have $2n - 2$ simple branch points in its interior, so that S and T have the same connectivity. So what Riemann set himself to establish was the existence of a function branched like T and whose real part was continuous in T and took arbitrarily real values on n boundary curves.

To find such a complex function, Riemann first extended the surface T by reflection to cover the whole plane of complex numbers. Then, by his theory of Abelian functions, there was a real function, g , defined by an integral of the third kind, with real moduli of periodicity, $4n - 4$ simple branch points, and infinities at conjugate points ε and $\bar{\varepsilon}$. The complex function of which ig was the imaginary part then solved the problem of finding ζ . He then defined functions y and y_1 by the equations

$$\frac{1}{\sqrt{\frac{dz}{d\zeta}}} = y, \quad \frac{z}{\sqrt{\frac{dz}{d\zeta}}} = y_1. \quad (5.35)$$

Straight-forward considerations of what happens at conjugate complex points then showed that the solution, s , of the equation $\frac{\partial^2 y}{\partial \zeta^2} = sy$ took conjugate complex values

at conjugate complex points. So s is a function of ζ branched like T and $z = \frac{y_1}{y}$ provides a conformal image of T bounded by circles.

5.10 Riemann's Zeta Function

If there is a single paper which provoked a general move to introduce complex function theory into number theory, it is Riemann's paper on the zeta function. Indeed, this paper (Riemann 1859) is one of most important papers in mathematics. Riemann's only contribution to number theory, the paper is an extraordinarily profound 9-page account of the prime number theorem. The theorem asserts that the ratio of the number of primes less than a real number x to the logarithmic integral $Li(x) = \int_2^x \frac{dt}{\log t}$ tends to unity as x tends to infinity. In other words, denoting by $\pi(x)$ the number of primes less than x , that $\pi(x) \sim Li(x)$. This estimate was first made by Gauss on the basis of a table of the first three million numbers.⁴¹ It was first conjectured in public by Legendre in his *Théorie des nombres* (1798). Chebyshev, in his (Chebyshev 1850), proved that for all sufficiently large x

$$0.89Li(x) < \pi(x) < 1.11Li(x). \quad (5.36)$$

Riemann may have learned of his work from Dirichlet, although he did not cite it.

What Riemann did was to offer not only a proof of the prime number theorem but a refinement of it, in the form of an infinite series for $\pi(x)$ in which $Li(x)$ was the leading term. The reception of the paper says a lot about what it contains. To quote from Edwards, the author of the standard full-length study of this paper of Riemann's:

Riemann's proof of this formula was inadequate; in particular, it is by no means clear from Riemann's arguments that the infinite series for $\pi(x)$ even *converges*, much less that its largest term $\int_2^x \frac{dt}{\log t}$ dominates it for large x . ((Edwards 1974, 4) emphasis in original.)

The importance of the paper is that, as Edwards says (1974, x), "Riemann was so far ahead of his time that it was 30 years before anyone else began really to grasp his ideas" and yet that, when they did "Riemann's methods . . . all have had important parts in the subsequent development of the theory"; indeed, they were crucial.

This is not the place for a detailed study of the paper. The careful account provided by Edwards takes 38 pages just to describe what Riemann did and to indicate how his claims might be proved. Almost every remark in the original 9-page paper called for comment. But we shall provide a capsule account of why

⁴¹See his letter to Encke in Gauss, *Werke* (2, 444–447) of 1849, where he recalls making this observation in 1792 or 1793.

Riemann's approach works, describe how Riemann's paper employed and extended the resources of complex function theory, and indicate the hints it left for others to take up. As Edwards remarked (1974, 4) "The real contribution of Riemann's 1859 paper lay not in its results but in its methods".

Given a Dirichlet series

$$\sum \frac{a_n}{n^s} \quad (5.37)$$

that converges for $\text{Re}(s) > 1$, knowledge about its singularities can be translated into knowledge about the related series $\sum_{n \leq x} a_n$, thought of as a function of x . In particular, if one wishes to study the function $\sum_{n \leq x} 1$, then one should start with the function $\log \zeta(s)$. The singularities of $\log \zeta(s)$ are at the zeros of ζ , and Riemann was able to provide an estimate of their number in the critical strip $0 \leq \text{Re}(s) \leq 1$. This estimate is enough to guarantee the convergence of a representation of another function, $\log \xi(t)$, as an infinite sum, from which Riemann was able to indicate how the prime number theorem could be obtained. The idea of passing from a Dirichlet series to another infinite series may be said to be the lesson Dirichlet took from his acquaintance with Fourier's ideas. The passage itself is a kind of Fourier transform. But the difficulties in making this rigorous in the complex setting are considerable, and it is with the zeros of $\zeta(s)$ that the greatest difficulties lie.

Riemann began with Euler's observation that

$$\prod \left(\frac{1}{1 - \frac{1}{p^s}} \right) = \sum \frac{1}{n^s}, \quad (5.38)$$

where the product on the left is taken over all primes p , and the sum on the right over all positive integers n . He at once let s be a complex variable, while, as we saw, Dirichlet had only considered real values of s . The expressions converge when $\text{Re}(s) > 1$ to a value Riemann denoted $\zeta(s)$. To obtain an expression which is always valid, Riemann introduced Gauss's factorial function, $\Pi(z)$, which we have replaced by the Gamma function $\Gamma(z)$ in use today; they are related by the equation $\Pi(z) = \Gamma(z+1)$. Riemann noted that

$$\int_0^\infty e^{-nx} x^{s-1} dx = \frac{\Gamma(s)}{n^s},$$

and summed it over n to obtain

$$\int_0^\infty \frac{x^{s-1} dx}{e^x - 1} = \Gamma(s) \zeta(s).$$

From this he soon deduced a single-valued expression for ζ that was, as he put it, finite for all $s \neq 1$:

$$2 \sin(\pi s) \Gamma(s) \zeta(s) = i \int_{\infty}^{\infty} \frac{(-x)^{s-1} dx}{e^x - 1}$$

where the integral is taken along the positive real axis from ∞ to 0, round 0, and back along the positive real axis to ∞ . The argument hinges on the fact that the integral converges everywhere and the use of the appropriate contour. It shows that the zeta function $\zeta(s)$ is analytic everywhere except at the point $s = 1$, where it has a simple infinity.

A second contour integral argument showed that the value of the expression

$$\Gamma\left(\frac{s}{2}\right) \pi^{-s/2} \zeta(s)$$

is unchanged when s is replaced by $1 - s$. Unlike the first example, this argument requires rather more care than Riemann was prepared to give it, which suggests that he was confident enough in such matters not to want to clutter the text with details. Be that as it may, Riemann's observation implies the functional equation for the zeta function, which we can write explicitly as

$$\pi^{-s/2} \Gamma(s/2) \zeta(s) = \pi^{(s-1)/2} \Gamma((1-s)/2) \zeta(1-s). \quad (5.39)$$

Using properties of the Gamma function, this becomes

$$\zeta(s) = 2^s \pi^{s-1} \sin(s\pi/2) \Gamma(1-s) \zeta(1-s). \quad (5.40)$$

This symmetry led Riemann to introduce a new function of the variable $t = \frac{1}{i}(s - \frac{1}{2})$, namely

$$\xi(t) = \Gamma\left(\frac{s}{2} - 1\right) (s-1) \pi^{-s/2} \zeta(s). \quad (5.41)$$

This function is analytic for all finite values of t . The symmetry of the function ζ gives a simple functional equation for the new function ξ :

$$\xi(t) = \xi(-t). \quad (5.42)$$

The symmetry property of the function ζ struck Riemann as so important that he gave a second proof of it. Noting that

$$\frac{1}{n^s} \Gamma\left(\frac{s}{2}\right) \pi^{-s/2} = \int_0^{\infty} e^{-n^2 \pi x} x^{(s/2)-1} dx,$$

he summed both sides over n and introduced into the integral the function $\psi(x) = \sum_{n=1}^{\infty} e^{-n^2 \pi x}$, which is defined for $x > 0$, to obtain

$$\Gamma\left(\frac{s}{2}\right)\pi^{-s/2}\zeta(s) = \int_0^{\infty} \psi(x)x^{(s/2)-1}dx.$$

This integral converges for all s in the complex plane and gives an analytical continuation of the zeta function to all $s \neq 1$ in the complex plane.

The function $\psi(x)$ is related to Jacobi's theta function $\theta(x) = \sum_{n=-\infty}^{\infty} e^{-n^2\pi x}$ by the equation $\theta(x) = 2\psi(x) + 1$, and Jacobi had shown that the theta function satisfies the inversion formula $\theta(1/x) = \sqrt{x}\theta(x)$, so $\psi(x)$ satisfies

$$2\psi(x) + 1 = x^{-1/2} \left(2\psi\left(\frac{1}{x}\right) + 1 \right),$$

and a little manipulation gave Riemann the equation

$$\Gamma\left(\frac{s}{2}\right)\pi^{-s/2}\zeta(s) = \frac{1}{s(s-1)} + \int_1^{\infty} \psi(x) \left(x^{(s/2)-1} + x^{-(1+s)/2} \right) dx$$

from which the symmetry of the zeta function is again evident. Riemann also gave an explicit expression for $\xi(t)$ that was essential to what followed:

$$\xi(t) = 4 \int_1^{\infty} \frac{d(x^{3/2}\psi'(x))}{dx} x^{-1/4} \cos\left(\frac{1}{2}t \log x\right) dx.$$

This function is finite for all finite values of t and can be developed, said Riemann without giving any evidence, as a rapidly converging power series in t^2 . The relevance of this observation to what followed was left undiscussed and could not adequately be brought to light until Hadamard created a theory about the how the behaviour of a function is determined by the coefficients of its Taylor series (see Sect. 8.5.1). He then argued, in a passage that [Edwards \(1974, 17\)](#) describes as “the most difficult portion of Riemann’s paper” that

$$\log \xi(t) = \sum \left(1 - \frac{t^2}{\alpha^2} \right) + \log \xi(0),$$

where the sum is taken over all the zeros of the function ξ .⁴² As to these zeroes, Riemann remarked that they must lie in the strip where the imaginary part of t lies between $-\frac{1}{2}$ and $\frac{1}{2}$. As for the number of roots of the equation $\xi(t) = 0$ whose real parts lie between 0 and T , Riemann estimated their number to be $\frac{T}{2\pi} \log \frac{T}{2\pi} - \frac{T}{2\pi}$ by a contour integral argument.⁴³

⁴²See [Edwards \(1974, 18\)](#). The necessary argument was first provided in [Hadamard \(1893\)](#).

⁴³This estimate was not to be proved until ([von Mangoldt 1905](#)).

Riemann (1859, 180) went on:

it is very likely that all of the roots are real. One would of course like to have a rigorous proof of this, but I have put aside the search for such a proof after some fleeting vain attempts because it is not necessary for the immediate objective of my investigation.

This, coupled with the observation that the zeta function has zeros at the negative even integers, where $\Gamma(s/2)$ has poles, is the famous Riemann hypothesis that is still resisting proof after more than 150 years.⁴⁴ It is clear that the ζ function cannot vanish in $\text{Re}(z) > 1$, because ζ satisfied the Euler product formula and is convergent in that domain, but no term in the product vanishes and so the product itself cannot vanish. By the functional equation the function cannot vanish in $\text{Re}(z) < 0$, and so all the zeros lie in the critical strip.

To investigate the number of primes less than a given real number x , Riemann defined $F(x)$ to be this number when x was not a prime, and to exceed this number by a half when x was prime. The connection with the zeta function was made by observing that

$$p^{-ns} = s \int_{p^n}^{\infty} x^{-s-1} dx,$$

so, on writing $f(x)$ for

$$\sum \frac{1}{n} F(x^{1/n})$$

one finds

$$\frac{\log \zeta(s)}{s} = \int_1^{\infty} f(x) x^{-s-1} dx.$$

Now, said Riemann, this is an example of a transformation of the form

$$g(s) = \int_0^{\infty} h(x) x^{-s} d \log x,$$

for which there is an inverse transformation expressing h in terms of g :

$$2\pi i h(y) = \int_{a-\infty i}^{a+\infty i} g(s) y^s ds.$$

So

$$f(y) = \frac{1}{2\pi i} \int_{a-\infty i}^{a+\infty i} \frac{\log \zeta(s)}{s} y^s ds.$$

⁴⁴It is one of the few Hilbert problems to resist solution, and is now one of the million dollar prizes offered by the Clay Mathematics Institute.

These are the first examples of complex Fourier transforms and require a straightforward convergence argument to be seen as rigorous.

The expansion of $\log$ which he had established earlier now allowed him to show, by means of a convergence argument that requires a lot of work to uphold, that $f(x) = Li(x) + \text{other terms}$. But Möbius inversion applied to the definition of the function f showed that

$$F(x) = \sum (-1)^\mu \frac{1}{m} f\left(x^{1/m}\right), \quad (5.43)$$

where m ranges over all positive square-free integers and μ denotes the number of prime divisors of m . It followed that $F(x)$, the number of primes less than a non-prime x , was the sum of a constant term (which Riemann estimated) a term that was bounded as x increased, and the oscillating term

$$\begin{aligned} Li(x) - \frac{1}{2}Li\left(x^{1/2}\right) - \frac{1}{3}Li\left(x^{1/3}\right) - \frac{1}{5}Li\left(x^{1/5}\right) \\ + \frac{1}{6}Li\left(x^{1/6}\right) - \frac{1}{7}Li\left(x^{1/7}\right) + \cdots. \end{aligned} \quad (5.44)$$

The old estimate $F(x) = Li(x)$ was thus refined and an observed “thickening and thinning of primes” explained for the first time.

The paper evidently falls far short of a proof of the prime number theorem, but just as clearly it provides it for the first time with a context in which it might be proved. Any one attempting to do so would find the idea that the zeta function is globally defined (except at $s = 1$) the least of their difficulties, although it is a bold extension of Dirichlet’s analysis. This way of thinking of a complex function as defined globally by some expression or other was not one that Weierstrass, for example, was happy with. The use of contour integration is not conceptually deep, but checking that it was correctly used was, as we noted, to be extremely hard. But it was in dealing with the zeros of the zeta function that Riemann was forced into his most obscure passages, for the deepest problems posed by Riemann’s paper all flow from the fact that $\zeta(s)$ gives up its secrets only slowly, and in finding ways through these difficulties that subsequent mathematicians were driven to their greatest novelties.

Riemann himself was aware of some of the biggest gaps in his brief, visionary paper. He wrote to Weierstrass that the estimate of the number of zeros was not completely proved and that the subsequent term by term evaluation of an integral also required justification. Much later, in 1932, Siegel showed that Riemann had even gone quite some way to give an alternative treatment, one that in some points surpassed work done by Hardy and Littlewood in 1920 in this connection (see [Siegel 1932](#)). Siegel’s analysis was based on a very patient reading of fragments in Riemann’s *Nachlass*, and it established what many had been prepared to doubt, that Riemann’s short paper rested not just on a profound but frequently a detailed grasp of the technicalities involved. Nonetheless, if the customary modern interpretation

of the estimate, somewhat vaguely stated by Riemann, is accepted, it still defies proof.⁴⁵

When the thirty-year period in which no-one had been able to proceed with Riemann's insights came to an end, it did because Hadamard was able to create a theory of entire functions and with it establish a key point in Riemann's paper. Within a few years the prime number theorem itself was proved. Other important developments lay in estimating the rate of growth of certain functions not by convergent but by asymptotic analysis, while the elusive nature of the zeros of the zeta function has continued to inspire generations of analytic number theorists. The last of these topics would take us too far afield, but we do look briefly at Hadamard's work (see p. 627) and at asymptotic analysis in Sect. 7.2.⁴⁶

5.11 Responses to Riemann's Work

It is often said, perhaps with the example of Riemann's paper on the zeta function in mind, that immediate responses to Riemann's work were few. Indeed, such as there were often begin with an ominous phrase to the effect that nothing seems to have been done for many years. Yet a brief but reasonably complete list of what was done would go along these lines:

1. [Clebsch \(1864\)](#), which discusses the concept of genus, Abel's theorem, and the use of Abelian functions in geometry—see Sect. 5.11.3.
2. [Roch \(1863b\)](#), refining the Riemann inequality to an equality—see Sect. 5.11.2.
3. [Neumann \(1865a\)](#), on the Riemannian approach to hyperelliptic integrals—see Sect. 7.7.2.
4. Durège's book of 1864, the first in German on complex function theory—see Sect. 5.11.3.
5. Work by many authors (Clebsch and Lüroth, Clifford) on the topology of algebraic curves—see Sect. 5.11.
6. [Clebsch and Gordan \(1866\)](#) on Abelian function theory—see Sect. 5.11.3.
7. Work done in the 1860s by Christoffel and Schwarz on the Riemann mapping theorem—see Sect. 7.7.1 and Sect. 7.9.1.
8. Work done in the early 1870s by Prym and Schwarz on the Dirichlet principle—see Sect. 5.11.1.
9. Work in the mid-1860s by Weierstrass and Schwarz on minimal surfaces—see Sect. 7.8.
10. Work by Betti on the topology of higher dimensional manifolds—see [Scholz \(1999\)](#).

⁴⁵The term by term evaluation was first shown to be permissible in [Landau \(1908\)](#).

⁴⁶See the books by Titchmarsh, Edwards, and Patterson, and the more popular accounts by Derbyshire and du Sautoy.

11. Casorati's explicitly Riemannian presentation of complex function theory in 1868—see Sect. 10.3.
12. Beltrami's papers in the late 1860s on spaces of constant curvature, with application to non-Euclidean geometry—see [Beltrami \(1868a,b\)](#).⁴⁷
13. Schottky's paper of 1877 extending the Riemann mapping theorem to non-simply connected domains—see Sect. 8.3.3.
14. Fuchs's work around 1870 on complex ordinary differential equations, generalising the hypergeometric equation to equations of the Fuchsian type—see Sect. 7.3.1.
15. Papers in the early 1870s by Thomae and Fuchs on the theta null values.⁴⁸
16. [Schläfli \(1870\)](#) gave an integral representation of the hypergeometric function very much in Riemann's spirit but in a form better adapted, he said, to study its analytic continuations.
17. [Brill and Noether \(1874\)](#) on algebraic curves in which the Riemann–Roch theorem is first so named—see Sect. 5.11.7.
18. Tonelli's paper of 1875 on defining the genus—see Sect. 5.11.
19. Work by Weierstrass on Riemann's example of a continuous nowhere differentiable function and its connection to elliptic modular functions by Dedekind in 1876—see Sect. 6.5.5 and Sect. 8.2.2.
20. Work in the later 1870s by Weierstrass on the definition of genus and the Weierstrass gap theorem—see Sect. 6.8.6.
21. Work by Lipschitz and Voss on Riemannian differential geometry, starting in the 1870s.⁴⁹
22. Clifford's theorem of 1878 on “special divisors”—see Sect. 5.11.8.
23. Work by Christoffel and Kraus independently in 1878–1879 on the idea of canonical differentials and canonical embeddings of curves.⁵⁰
24. The paper ([Dedekind and Weber 1882](#)) in which these authors sketched a remarkably modern algebraic version of the theory of functions on a Riemann surface, which is beyond the scope of our book and we shall not pursue it here.

Such a list leaves only three of Riemann's papers unattended: the one on Bessel's equation ([Riemann 1855](#)), the one on the prime number theorem ([Riemann 1859](#)), and the one on shock waves ([Riemann 1860](#)). Bearing in mind that Riemann died at the age of only 39, and that a number of his students, and Clebsch too, died young, the actual response is rather considerable. Taking notice of the fact that the number of mathematicians capable of responding adequately was also small, the response to Riemann's work can be seen to have been much better than is usually asserted. In addition, the importance that was attached to producing the first edition of his *Werke*, notwithstanding its many delays, and the concomitant work put in on editing

⁴⁷For a commentary, see [Gray \(2006\)](#).

⁴⁸See their papers listed in the Bibliography.

⁴⁹See their papers listed in the Bibliography.

⁵⁰See [Gray \(1998\)](#).

up unpublished papers, lecture notes, and fragments, suggests a real willingness to keep his ideas alive after his premature death. Moreover, the academic world had recognised Riemann's talent swiftly and generously. Weierstrass had proposed him in strong terms to the Berlin Academy of Sciences in 1859, and Riemann went with Dedekind to accept the nomination. There he met Weierstrass, and the two apparently got on well together. In later years Weierstrass encouraged his best students to look at Riemann's papers for good topics to work on, which argues for a high opinion of their importance but not for their rigour. Faced with all this work, the question becomes: why did Riemann's work draw the responses that it did?

As far as the reception of his theory of complex functions is concerned, any one reading that theory around 1860 would see that it rested on a few key points. It emphasised the importance of considering the global nature of the domain of a complex variable and gave a topological analysis of the domain in terms of cuts in the Riemann surface. It appealed to Dirichlet's principle to establish the existence of complex functions on various domains and having prescribed singularities, and it offered a proof of that principle. It created a theory of algebraic functions on a Riemann surface in terms of spaces of functions with prescribed types of infinities. It refined that analysis in terms of a theory of theta functions of several variables. Finally, it parameterised the family of different Riemann surfaces of a given genus by means of moduli and gave a hint of how they too could be further analysed by means of theta functions. We shall look at the first four of these aspects in turn, leaving the fifth to the next chapter, and then consider our question again.

Each of these points raised problems for even the best of Riemann's contemporaries. It was by no means a trivial matter to decide what it meant for a curve to have complex points.⁵¹ Setting that difficulty on one side, we may begin by investigating how the topology of an algebraic curve came to be understood. This was the work of many authors, all of whom concurred in finding it not at all easy to pass from an equation to the form of the corresponding Riemann surface. Thus Clebsch's seven distinguished obituarists listed this as one of the three hardest problems confronting anyone who approached Riemann's work (see Von der Mühl 1875, 25). Clebsch and Lüroth had been prominent in this endeavour and in the 1860s showed how a Riemann surface may be cut up along loops that encircle the branch points in particularly simple ways. Other contributions were made by Clifford in (Clifford 1877), so that one can say that fairly soon this difficulty was resolved. In 1875, Tonelli pointed out that Riemann's discussion of connectivity was imprecise and rectified it (see Tonelli 1875).

⁵¹Plücker, von Staudt and other geometers dealt with them in terms of automorphisms of the surface of period 2 with no real fixed points, see Gray (1994) and below, p. 332.

5.11.1 Prym and the Dirichlet Principle

What of Riemann's reliance on the contentious Dirichlet principle? In the 1880s Klein tried to argue that Riemann, who had immersed himself deeply in potential theory, had advocated Dirichlet's principle on physical grounds, but Prym, a former student of Riemann's, held a more nuanced position. Prym thought that Riemann had indeed, as a student of Dirichlet, based his ideas in the potential-theoretic way of thinking, and "to that extent it was true that Riemann had come to his discoveries through mathematical physics" (see Bottazzini 1977b, 32). But, he went on, he could not see that Riemann's work on Abelian functions owed anything to "physical or geometrical representations". What is clear is that the principle was felt to need proof, on this every one including Riemann was agreed—only actual physicists like Helmholtz and Maxwell were satisfied.⁵² The problem was that Riemann had offered a proof, and it was this that collapsed. Accordingly, when in 1900 Hilbert succeeded in giving a direct proof of the validity of Dirichlet's principle for a wide class of domains, he restored Riemann's original line of approach only under mildly restricted circumstances (see Sect. 7.7.5).

The first to refute it was Riemann's former student Prym.⁵³ His work (Prym 1871) was superseded by the much more extensive treatments of Schwarz, who went on to recover the Dirichlet's principle for a large if ill-defined class of boundaries, but Prym's example is interesting. He took the case of a disc and an arbitrary continuous function, u , defined on the boundary, for which Dirichlet's principle implied that there was an extension to a finite and continuous harmonic function defined on the whole of the disc. He pointed out that all known proofs of this result relied on the claim that the function u was identical with its Fourier series, but that this result was only known for functions having only finitely many maxima and minima. In particular, he said, it was a misapprehension to think, as Hankel (another of Riemann's students) did, that Riemann had proved that an arbitrary function is representable by its Fourier series. But in any case, he said, even when Dirichlet's principle was true, Riemann's approach to it might be in error.

To show this, Prym (1871, 361–364) considered a branch of the complex function $u + iv = i\sqrt{-\log(R+x+iy)}$ defined on a disc of radius $R < \frac{1}{2}$, and introduced polar coordinates ρ and τ centred on the point $(-R, 0)$. In the disc ρ took every value from 0 to $2R < 1$ and τ every value from $-\pi$ to π . The branch of logarithm taken was to satisfy $-\log(R+x+iy) = -\log\rho - i\tau$. From the explicit form for

⁵²Thus Klein wrote in his *Entwicklung*, (1926–1927, 1, 264), that Helmholtz once said to him "For us physicists, Dirichlet's principle remains a proof". On Maxwell, see his reliance on Green's work in his *A Treatise on Electricity and Magnetism*, Chapter IV.

⁵³Friedrich Prym took his doctorate in 1863 officially from the University of Berlin although he is best regarded as a student of Riemann's. He became a professor for some years in Zurich before becoming a professor at Würzburg in 1869, where he remained until 1909. He worked for most of his life on the theory of theta functions of several variables and a related class of functions today called Prym functions in his honour.

u and v in terms of polar coordinates it then followed that the functions u and v are everywhere defined and single-valued, even on the boundary of the disc. Since the function u is the real part of a complex function it is certainly harmonic. A closer examination showed that the function u was zero at the point $\rho = 0$. Prym then considered the Dirichlet's integral $L(u, 0)$ and showed that it was infinite. The reason, as his formulae make clear, is that the function u oscillates infinitely often in any neighbourhood of the point $\rho = 0$. Consequently there is no hope that step 2 of Riemann's argument ($L(\mu) < \infty$) could be made to work.⁵⁴

The usual story, that Riemann relied uncritically on Dirichlet's principle and thereby impeded the acceptance of his own work, must be modified in three ways. First, Riemann's approach was not uncritical, but his own use of it was shown to be fundamentally flawed. Second, the generality of the principle was simply too novel: its use in defining functions was unacceptable to a mathematician such as Weierstrass who wanted explicit expressions. Third, it was the ubiquity of it in Riemann's work which meant that once it was called into question much else of what he did was likewise put profoundly in doubt. Brill and Noether's view (1894, 265) from outside the Riemannian circle was that "In this generality the function idea became unintelligible and so volatile that it no longer led to secure conclusions. In this way the central Riemannian existence theorem for algebraic functions on a given Riemann surface lapsed and in its place came a vacuum".

5.11.2 *Gustav Roch*

Readers who accepted Dirichlet's principle, or who were willing to assume that it could either be put right or circumvented were free to proceed to the Riemann inequality. Most famous of all of these was Riemann's student Gustav Roch. Roch was born on 9 December 1839 in Dresden, and at his father's wish studied chemistry for two years at the Polytechnische Institut Neustadt before coming under the influence of Schlömilch and switching over to mathematics and practical physics. His first paper, on the mathematical theory of magnetism and electricity, appeared in Schlömilch's *Zeitschrift* in 1859 while he was still at school.⁵⁵ In Easter of that year he went to Leipzig, where he studied mathematics under Möbius and physics under Hankel. In April 1861, on the advice of Hankel, he migrated to the university of Göttingen, where he studied under Wilhelm Weber, Schering, Lotze, Enneper, and Riemann. He was so impressed by Riemann's brilliance that he resolved to follow in his footsteps, and after spending a term in Berlin, he graduated

⁵⁴For an instructive comparison of Prym's counter-example and the later and much better-known work of Hadamard on this topic, see Maz'ya and Shaposhnikova (1998, 373–377).

⁵⁵Schlömilch, ever the active editor, eventually published half of Roch's 18 papers. Schlömilch had administrative responsibility for instruction at schools and colleges in Saxony and was an advisor to the publishing house of Teubner, Leipzig; see Stubhaug (2002, 333).

from Leipzig⁵⁶ in 1862, and a year later he became a Privatdozent at Halle, where he habilitated in 1863 with a thesis on Abelian functions (Roch 1863b). Heine was one of the examiners on that occasion, and he had a high opinion of Roch. In 1866 he successfully recommended him for promotion to an extra-ordinary professorship at Halle, but the appointment, in August, was followed in October by the university granting Roch leave of absence over the winter semester, so that his health might recover. It was not to be, and Roch died in Venice of tuberculosis on 21 November 1866, a few days before his 27th birthday.⁵⁷

Like Riemann, Roch worked both on potential theory and complex function theory. His short but profound contribution (Roch 1865a) to Riemann's work was to convert the Riemann inequality into a meaningful equality by interpreting the difference between the dimension of the space of meromorphic functions on a given Riemann surface of genus p with m given singular points and the number $m - p + 1$. He began by reminding his readers that the two numbers can genuinely differ, giving the Riemannian example of the functions whose square roots Riemann had called Abelian functions. He then took the most general expression for an integral of the second kind (equivalently, a meromorphic function), which Riemann had given as

$$v = \beta_1 t_1 + \cdots + \beta_m t_m + \alpha_1 w_1 + \cdots + \alpha_p w_p + c, \quad (5.45)$$

where c is a constant. Here the function t_i has a simple infinity at the point σ_i (for simplicity, Roch supposed that all the points σ_i are finite). For v to be a meromorphic function, all its periods must vanish, which impose $2p$ equations on the $m + p + 1$ constants, leaving $m - p + 1$ free when those equations are linearly independent.

To investigate when the equations are, instead, linearly dependent, Roch wrote each function w_i in the special form Riemann had also used: $u_i = \int \frac{\phi_i(s, z) dz}{\frac{\partial F}{\partial s}}$. Each of these functions has a simple set of periodicity moduli on the a and b cuts, which we omit here. The periodicity moduli of the functions t_i can also be given a simple form. This produces a simple form for the periodicity moduli of the function

$$s' = \beta_1 t_1 + \cdots + \beta_m t_m + \alpha_1 u_1 + \cdots + \alpha_p u_p + c. \quad (5.46)$$

The upshot is that the equations for the vanishing of the periodicity moduli of the putative function s' take the form

$$\sum_{k=1}^m \beta_k \phi_i(s_k, z_k) \frac{\partial F(s_k, z_k)}{\partial s_k} = 0, \quad i = 1, \dots, p. \quad (5.47)$$

⁵⁶With a thesis entitled *Über die Darstellung von Functionen dreier Variablen durch Potentialausdrücke [etc]*.

⁵⁷Some of this information comes from the web-site <http://www.mathematik.uni-halle.de/history/roch/index.html>.

It follows that one or more can be a consequence of the others if and only if the p coefficients $c_1, \dots, c_p$ in $\phi = c_1\phi_1(s, z) + \dots + c_p\phi_p(s, z)$ can be so chosen that $\frac{\phi_i(s, z)dz}{\frac{\partial F}{\partial s}}$ vanishes for each of the m values of k . But that is to say that the m points $\sigma_1, \dots, \sigma_m$ are points where the function ϕ_i vanishes. Roch therefore deduced that if q of the functions $\frac{\phi_i(s, z)dz}{\frac{\partial F}{\partial s}}$ vanish at the m points $\sigma_1, \dots, \sigma_m$, then the function s' depends on $m - p + 1 + q$ arbitrary constants. This result was shortly called the Riemann–Roch theorem by Brill and Noether in their (1874, 280), and the name persists to this day.

To give an example, Roch let $F(s, z)$ be a non-singular curve of degree 5, and therefore genus $p = 6$. Every everywhere finite integrand has the form

$$\frac{(as^2 + bsz + cz^2 + ds + ez + f)dz}{\frac{\partial F}{\partial s}},$$

so every polynomial function of degree 2 is a function ϕ . He then considered the function $s' = \frac{as+bz+c}{a_1s+b_1z+c_1}$. This function vanishes at the five points where the line $a_1s + b_1z + c_1 = 0$ meets the curve. However, three functions ϕ vanish at those points, namely $a_1s + b_1z + c_1$, $(a_1s + b_1z + c_1)s$, and $(a_1s + b_1z + c_1)z$. So the function s' depends on only $5 - 6 + 1 + 3 = 3$ arbitrary constants, which are, of course, a, b , and c .

5.11.3 Clebsch and Gordan's Algebra

The most thorough-going of Riemann's immediate successors was Rudolf Clebsch. He graduated from Jacobi's school in Königsberg and first worked on elasticity theory and hydrodynamics. But in the 1860s, after becoming Riemann's successor at Göttingen, he decided to master Riemann's work. There he emerged as the leader of a strong group of algebraic geometers who saw themselves as leading a wave of new mathematics in just those areas neglected by Gauss. This school sought to reformulate Riemann's ideas in terms of their own theory of the projective geometry of algebraic curves and to cross from there to analysis by the bridge, as they saw it, provided by Abel's theorem. This was not welcome news to Riemann's surviving pupils. Prym wrote to Casorati in 1866 of the book by Clebsch and Gordan, in which their approach was set out in detail, that "The attempt to base function theory on algebra is completely useless" (Neuenschwander 1978b, 61) and said that they would never have dared to publish the foreword in Riemann's lifetime. But even Clebsch was not long enlisted in Riemann's cause. He was to have edited Riemann's *Werke*, but he died unexpectedly of diphtheria in 1872 when only 39.

Clebsch's first major paper in this connection is his (1864). He pointed out that in the six years since Riemann had published his paper on Abelian functions not enough had been done by the younger generation of German mathematicians to

catch up with this work. But it did show a good grasp of several of the main ideas Riemann had presented, notably the theory of theta functions with characteristics and their applications to geometry, as we shall describe below. In one respect indeed Clebsch surpassed the master, for he was the first to establish the converse of Abel's theorem.

Clebsch and his younger colleague Paul Gordan then wrote what they intended to be the definitive book on Abelian function theory from the standpoint of algebraic geometry, their *Theorie der Abelschen Functionen* of 1866. They departed from Riemann's treatment, arguing that⁵⁸

The difficulties that lie on Riemann's path into the theory of Abelian integrals, belong to exactly the same circumstances that are mostly responsible for bewildering the researcher. We mean, on the one hand, the highly general and at the same time carefully restricted idea of a function that is taken as foundational, and, on the other hand, the indirect path by which the functions being studied are described more synthetically than analytically. . . .

As the authors occupied themselves with the thought of grounding this theory in a new way, it seemed to them that it would be worthwhile to pay renewed attention to the original source of the entire discipline, the Jacobi inversion problem that Herr Weierstrass has successfully based the theory of hyperelliptic functions upon. . . . In fact, it needs only a few geometrical considerations and some theorems about integration in the imaginary to carry out this direct approach completely. In particular, in this way one avoids in particular all considerations of functions in general, which are always awkward because they are tangled up with completely indefinite and unknown possibilities. The theory of Abelian functions must be developed through theorems that can be established that draw only on what is known, in particular the algebraic functions.

The main technical improvement as they saw it over the position taken by Clebsch in 1864 was the definition of the genus of a curve. Earlier, Clebsch had accepted the definition as the number of linearly independent holomorphic integrands. Now, the genus was defined in terms of the degree of the equation defining the curve and the number of its double points and cusps. Curves having worse singularities were for the time being excluded.

What excited the ire of Prym was the emphasis on algebra over topology. However imperfectly, Riemann had attempted to get away from defining a Riemann surface by an equation and had attempted to prove that any finite Riemann surface could be described by (an equivalence class of) equations. This separated the surface, thought of abstractly, from any embedding it might have in some complex projective space; the Clebsch–Gordan treatment attempted to collapse that distinction. Then again, the definition of genus was turned away from cuts and back to properties of the defining equation. Even though they could show that birational transformations of the surface did not change the value of the genus (at least for curves that were not too singular) their definition was evidently not intrinsic.

The two men did, however, do better when it came to the function theoretic side of their work. One of the complaints that Riemann's followers had was that his theory of theta functions was not intimately connected to the rest of the theory,

⁵⁸See [Clebsch and Gordan \(1866, v–vi\)](#).

and the book presents a much improved version of that, spelling out at length the connection between theta functions and integrals of the third kind, which Riemann had only sketched.

As we discuss in more detail in Chap. 10, the first book in German on complex function theory is the one by Durège, first edition 1864. Durège was a former colleague of Riemann's, and as one would expect, his book is along the lines of Riemann's lectures. In 1865 the first edition of Neumann's *Vorlesungen über Riemann's Theorie der Abel'schen Integrale* came out. This edition just treated the hyperelliptic case, and with that restriction aimed to make Riemann readable, but it was not popular with Riemann's surviving students. Prym wrote to Casorati (Neuenschwander 1978b, 61) that "it is a watery stew of Riemann's theory, as clear and easy to see through as thin gruel". He went on to complain that the main ideas were completely buried under related ideas, Dirichlet's principle—"Riemann's true method for creating functions"—was missing and so Neumann worked too much with expressions and not enough with functions. "With Neumann we lost a school teacher" was his contemptuous opinion, although he conceded that the book was worth reading. Roch, in a lengthy review of it in Schlömilch's *Zeitschrift* found the individual sections too long to be lectures (as Neumann had styled them) and although he found things to praise in it such as the appeal to geometric intuition he still concluded⁵⁹:

Should a book of this kind really be appropriate for making substantial propaganda for Riemann's theories and so win many pupils to this great master, it should at least show how the content of Jacobi's *Fundamenta nova* can be developed with obvious brevity from these theorems about functions. To write a book of this kind would seem to the reviewer to be exceptionally useful, but the first rule besides clarity would be brevity.

The point of contention for both Prym and Roch was that Neumann, by avoiding Dirichlet's principle, had not got to the difficult ideas. There was no discussion of the Riemann inequality, of moduli, or of Abel's addition theorem. For his part Neumann was at pains to distinguish his work from that of Durège or Roch. However, it cannot be said that he really understood what was wrong with the Dirichlet principle, as his little book on it (also written in 1865) makes clear.⁶⁰ He left it out only on pedagogic grounds.

In the second edition, 1884, Neumann treated the general case of Abelian functions, but backed off from his earlier flirtation with Dirichlet's principle. The second edition, when it was re-read in Göttingen later, was said by Klein to have made every thing look so easy it was insulting, which shows how far things had swung Riemann's way (Klein 1926–1927, 1, 273).

⁵⁹Roch (1866b, 39).

⁶⁰See Roch's review (Roch 1866c).

5.11.4 *The Broader Response*

Of the textbooks, one of the best is Casorati's, whose remarkable book of 1868 begins with almost 150 pages of careful historical writing before giving over 250 pages to an account of the present state of the art. His book, which is quite a mixture of the techniques and ideas of Cauchy, Riemann, and Weierstrass, did a lot to establish Riemannian complex function theory in Italy. Klein (1926–1927, 1, 274) praised it highly and placed it first among the books inspired by Riemann's work. Curiously, the second part is more elementary than the first, covering the ground that Riemann himself covered in his lectures, while the first part has more to say about Riemann surfaces and Abelian functions.

The centre of mathematics in the second half of the nineteenth century was undoubtedly Berlin, where Weierstrass, Kummer, and Kronecker reigned for almost 40 years. Throughout the 1860s these three were engaged on a profound attempt to sort out the bases of real and complex analysis. What is a singular point, what is a natural boundary? What is the connection between continuity and differentiability? As we shall describe in more detail in the next chapter, this work was to push Weierstrass more and more towards algebra, but another way in which Weierstrass in particular dealt with the Riemannian legacy was to encourage his best students to take it up and rework it. This is most clear with his favourite pupil, Schwarz, much of whose career can be seen as an attempt to rescue Riemann from himself. Apart from the Schwarz–Christoffel approach to the Riemann mapping theorem and the alternating method for proving a modified form of Dirichlet's principle, Schwarz also worked extensively on the theory of minimal surfaces. Indeed, he literally got his hands dirty and made them himself. The subject was one that Weierstrass liked as well, and he came to his realisation of the close connection between minimal surfaces and complex function theory at the same time as Riemann but independently. So here too Riemann's ideas fell upon appreciative, if critical, ears.

Riemann also had students of his own at Göttingen. There were a number of these: Abbe, Roch, Hattendorff, Hankel, Prym, in addition to the valuable Italian contact of Betti. But of these Abbe left mathematics to work in optics for the firm of Zeiss, Roch died in 1866, Hankel in 1872, and Hattendorff in 1882. One can only guess what would have happened had Riemann himself lived, or if Dirichlet for that matter had stayed at Göttingen for twenty years. History does not possess Riemann as a healthy 45-year-old taking the young Klein under his wing in 1872

Until the 1880s there was little understanding of his work in France. Matters then changed. On 23 May 1882 Simart submitted his thesis⁶¹ for the degree of Docteur ès sciences mathématiques to the Faculté des sciences in Paris. The examiners were Hermite (as President of the jury), Bouquet, and Darboux, and they pronounced

⁶¹See Simart (1882).

themselves happy to award him the title of doctor. Of more interest than that, however, was what was they said about the current state of Riemann's theory of functions in Germany and in France.

Simart had begun by saying that Riemann's methods, which "had perhaps been expounded too succinctly", were little known in France. They were, however, well studied in Germany—he named several authors—and more was at stake than the mere concept of a Riemann surface. It seemed therefore to be useful to survey all of Riemann's methods, concentrating on Karl Newmann's [sic, for Neumann's] accounts both of the theory and of the Dirichlet principle, and the "well-known" work of Königsberger on elliptic functions. This he then did, deliberately sticking to the order in which Riemann had presented his ideas and almost literally to his definitions and the statements of results. So he began with the topology, and showed, more precisely than had been done before, how to reduce a Riemann surface of order of connectivity $2p + 1$ to a simply connected surface. Then he discussed harmonic functions and complex functions, then the use of the Dirichlet principle to obtain such functions, and finally he surveyed Riemann's theory of Abelian functions. Here he used the methods of Puiseux and Elliot, as described in Briot's book on Abelian functions (Briot 1879), to go beyond Riemann's own discussion, which had been limited to simple branch points. His thesis ended with a proof of the Riemann–Roch theorem and the deduction that every algebraic function of z branched like a function s defined by an algebraic equation $F(s, z) = 0$ is a rational function of s and z . He also noted that as he finished his thesis he came across Klein's little book *Ueber Riemanns Theorie der algebraischen Funktionen und ihrer Integrale* (On Riemann's theory of algebraic functions and their integrals) (1882), and he hoped his "preliminary study" would make it easy to read Klein's "beautiful memoir".

The thesis is one of a number reporting, accurately but not always with any originality, on work being done in Germany on Riemannian themes; others include Tannery's on Fuchs (1874), Niewenglowski on minimal surfaces (1880), and Goursat on the hypergeometric equation (published as Goursat 1881). When he sent an offprint of the latter to Mittag-Leffler, Hermite claimed that "it is one of the most remarkable in recent years" (in (Dugac 1984, 141)); in his review of it Hamburger (*Fortschritte* 13.0267.01) noted the many recapitulation of known results but still found it "a very interesting work because of its great breadth". Some more theses were written on other subjects in which the German lead was pronounced, although it should be said that some young mathematicians, Picard and Poincaré among them, were more obviously original. Picard's thesis dealt with the theory of surfaces and is not of our concern here. As for Poincaré, his thesis (Poincaré 1879) dealt with the problem of the solution by series of partial differential equations. It is interesting to remark in passing that in their report Darboux and Bonnet recognised that one of Poincaré's theorem constituted "a first, really remarkable progress", and also that there were "some introductory lemmas worth of interest", but "the rest of the thesis is a bit confused, and shows that the author did not succeed yet in expressing his ideas in a clear and simple manner". Nonetheless, taking into account "the great difficulty of the subject and Poincaré's skill" the Faculty awarded him the title of doctor (with three blank votes!).

In Niewenglowski's case Hermite gave him credit because he had "made Riemann's memoir perfectly clear, which because of both the peculiarity of the analysis of great geometer and the conciseness of his style was almost unintelligible for most of those who wanted to study it". August, in his review (*Fortschritte* 12.0619.01), merely noted that the thesis also reminded readers of the contribution of Bonnet to the subject, but contained nothing new.

In Simart's case, with almost the same words than in Niewenglowski's Hermite reported that Riemann's original presentation was "far from providing the required developments", and "most geometers had experienced the greatest difficulties in following the new principles he introduced in science". That said, the theory of Abelian functions displayed the power of the new methods so well that several analysts had begun to explore them. Among these Klein had been able to illuminate many points, although access was still difficult and not without some obscurity, and more recently Schwarz had done important work on the Dirichlet principle. All this work by German mathematicians made French mathematicians long for a simple and elementary exposition of the main results, and this difficult task had now been accomplished "with talent and success" by Simart, "whose thesis—according to Hermite—renders a signal service to mathematical studies in our country".⁶² It is a striking testimony to the intellectual insecurity of France that a competent but barely original survey can be so warmly received.

Within Germany the response was increasingly along lines inimical to the deepest tendencies in Riemann's thought. The Berlin school opposed the topological emphasis and it was Weierstrass's habit to absorb and rework other ideas completely, even to the point of giving few references, so the large audiences drawn to Berlin would not necessarily know when he was entering Riemannian territory. Nor could they pick up a lead to the original sources. Now Weierstrass increasingly hardened his views, and Schwarz had his own reasons for going along. In Weierstrass's case, the discoveries mentioned above had the effect of calling all of Riemann's insights into question, until they could be proved. He placed more and more emphasis on power series methods, and even eliminated the Cauchy integral formula from his presentation of complex function theory. The fully developed algebraic approach to complex analysis obliterates Riemann's—at the price, a later generation came to feel, of making it impossible to think for oneself.

The school around Clebsch likewise preferred an algebraic geometry based on a study of equations, and while they named theorems after Riemann, it was only after giving them entirely different proofs. They did this quite self-consciously, seeing in algebra both the novelty of their approach to geometry and its rigour. The result is a theory of algebraic curves that regards them as plane curves, defined by an equation; thus, as embedded curves. The theory of space curves is different again on this approach, and the birational theory developed by Clebsch's successors Brill and Max Noether relied more and more on algebra. The approach that reads everything

⁶²This information comes from [Gispert \(1991, 325–338\)](#), the report on Poincaré's thesis is on p. 331, the report on Simart's thesis is on p. 335.

off the equation, the degree, the singularities, the genus, is far from being Riemann's. For them, it was Riemann's reliance on the "misleading" Dirichlet's principle that was the problem. In their history of algebraic function theory, written in 1894, this distrust comes across quite clearly. Definitions become indefinite, they wrote (1894, 265), and

At such a level of generality the idea of a function, incomprehensible and evaporating, no longer delivers reliable conclusions. In order to delineate precisely the domain of validity of the theorems under consideration, people have recently abandoned Riemann's path completely.

This school did not intend to be faithful to what Riemann had tried to do.

Apparently, it was the Italians who proved most receptive to what Riemann had been trying to say. One might argue that in the absence of a strong national mathematical tradition such as Cauchy's in France they were more open to new results from abroad, especially from Germany. Apart from Casorati, who met Riemann only once in 1858 in Göttingen, there was also Riemann's friend Betti, who is famous for his attempts to produce a theory of higher dimensional manifolds. Betti was influential in another way. For 30 years he ran the Scuola Normale in Pisa, a small but high quality institute that trained most of the next generation of Italian mathematicians, and that is a lot of very good people.⁶³ Betti was faithful to the breadth of Riemann's interests and that shows in the work that came out of that school, and in due course trickled back to Klein when he was trying to outflank the *Journal für reine und angewandte Mathematik*, edited in Berlin, with the more recent *Mathematische Annalen*, founded by Clebsch and now edited by Klein. So important is Betti's response to Riemann that it is discussed at length in the next section.

The result of Riemann's influence being most marked in Italy, negligible in France for a generation, and overlaid with alien techniques in Germany was that there was justifiably a profound sense of rediscovery that attended the rehabilitation of his ideas in Göttingen in the 1880s. This process was begun by Klein when he arrived in Göttingen, taken further by Hilbert, and perhaps consummated by Weyl. In Klein's case there was to be a profound effect. Elevated to a professorship at the remarkably early age of 23 he attached himself to Clebsch, whom he later described as a divinely inspired teacher. By 1880 he had begun to read Riemann, and to think what he should do to realise his own potential and fulfill his own obvious gifts for leadership. He was at once caught up in the intense burst of energy that attended Poincaré's discoveries of non-Euclidean geometry and automorphic functions (see Sect. 8.2.4) and, as he was later to say, this cost him his health.

When he recovered from what seems to have been a nervous collapse he channeled his energies not exactly away from research but into building up Göttingen. He energetically sought money for the department as an alternative to Berlin. He directed numerous students. He helped edit the many unpublished papers of Gauss.

⁶³Dini, Arzelà, Pincherle, Bianchi, Ricci Curbastro, Pieri, Volterra and Enriques were among them.

And he wrote a remarkable history of nineteenth century mathematics. In this personal journey he broke with the Clebsch school, and when he gives his popular lectures in Evanston in 1893 (see (Klein 1894b)) Clebsch is the only mathematician whom he singles out for criticism. Not surprisingly, Brill and Noether repaid him by slighting his contributions in their own history. Now it was Klein who set about restoring Riemann's reputation, and he did so partly by establishing a great tradition at Göttingen. This tradition down-played the other responses, and for their own reasons neither Berlin nor Clebsch's followers could talk up their connection to Riemann. Klein's line had a clear run into the future.

Klein shrewdly placed emphasis on just the intuitive aspects of Riemann's ideas that his own gifts as an expositor favoured, even though in so doing he was criticised by those who remembered Riemann personally. One casualty of this is the theory of theta functions, which disappeared from his book on Riemann's theory of algebraic functions and their integrals, and on some views this is to present Christmas dinner without the turkey. Perhaps inadvertently this also diminishes the extent to which Riemann's ideas had an immediate reception. On the other hand, in his lectures in the 1890s Klein did return to that part of the Riemannian legacy, as we shall see in Sect. 7.9.3.

5.11.5 Betti–Riemann's “Most Faithful Friend”

Betti became so closely associated with the work and ideas of Riemann on complex function theory that after Riemann's death he became the obvious and most authoritative reference for scholars hoping to learn Riemann's last and “true” thoughts on various topics. For example, Schering in Göttingen wrote to him when planning to publish an Italian edition of Riemann's collected works,⁶⁴ and later Klein asked Betti's advice about his physical interpretation of Riemann surfaces in terms of electrostatic currents on a surface (Klein 1882a). So it worthwhile tracing the intertwined paths of Riemann and Betti in the late 1850s and early 1860s.

Betti did not start his career as a function theorist, rather, his early papers were devoted to the theory of algebraic equations, and to Galois theory in particular. But the influence of his meeting with Riemann in Göttingen in 1858 was dramatic. Impressed by Riemann's results, Betti changed his field of research, turning first to complex analysis and elliptic function theory, then to mathematical physics. Back in Italy, Betti translated Riemann's 1851 dissertation into Italian, which was published in the newly founded journal *Annali di matematica pura ed applicata* (Betti 1859), and devoted his 1859/60 lectures at the university of Pisa to elliptic function theory. The lecture notes of that course formed the basis of a very long article, actually a monograph on the subject, which then appeared in the *Annali* (Betti 1860–1861).

⁶⁴The relevant letters are kept in Betti's *Nachlass* in Pisa. See Bottazzini (1977a).

Betti's article was an original attempt to combine different approaches to the subject. Jacobi's approach to elliptic functions via theta series was based by Betti on preliminary results which in turn were embedded in Cauchy's theory of complex functions as expounded in the newly published book by Briot and Bouquet, their (1859). This was the object of a careful, thoughtful study by Betti who, in his letters to Italian colleagues, criticised many of their theorems. All this was combined with the developments in [Riemann's \(1851\)](#).

Betti began with the definition of a monodromic function $w(z)$ and its derivative, then, following Riemann, he stated that the Cauchy–Riemann equations give a necessary and sufficient condition for the derivative $\frac{dw}{dz}$ to be unique, i.e. independent of z . Adopting Cauchy's language, he called the functions satisfying this condition monogenic. An analytic function was defined by Betti as a function whose values can be expressed by a (finite or infinite) number of elementary operations. Thus, he easily proved that all analytic functions are monogenic functions. Entire functions are defined as “the functions whose values can be expressed by convergent power series for any real or complex value of z ” ([Betti 1860–1861](#), 229).

Having defined what he called the residue of $w(z)$ as the value of the integral $\int wdz$ along a (closed) path, he proved the Cauchy integral theorem (without giving it a name). His proof, inspired by Riemann, was a direct application of a Green's function argument that was known also to [Gauss \(1840\)](#). He supposed the path is smooth, without knots, and inscribed in a rectangle with edges given by the tangents to the path parallel to the axes, and assumed that $w(z)$ is “finite and continuous” within the rectangle, and deduced that

$$\int wdz = \int (u + iv)(dx + idy) = \int \left(u \frac{\partial x}{\partial s} - v \frac{\partial y}{\partial s} \right) ds + i \int \left(v \frac{\partial x}{\partial s} + u \frac{\partial y}{\partial s} \right) ds,$$

the line integrals being taken over the boundary of the rectangle. On the other hand

$$\iint dxdy = \iint \left(u \frac{\partial x}{\partial s} - v \frac{\partial y}{\partial s} \right) ds, \quad \iint dxdy = \iint \left(v \frac{\partial x}{\partial s} + u \frac{\partial y}{\partial s} \right) ds, \quad (5.48)$$

which allows the line integrals to be replaced by surface integrals of a zero-integrand once the Cauchy–Riemann equations are satisfied.

If instead the function has a singular point D in the domain, then the residue of $w(z)$ taken along the curve C equals the residue of the integral taken along the (closed) boundary of an arbitrarily small domain around D . In particular, this domain could be a circle. Betti did not say that explicitly but used it to obtain Cauchy's formula for the residues ([Betti 1860–1861](#), 231). Then, by resorting to the Cauchy integral formula (and again without mentioning him by name) Betti showed that a function can be expanded in power series of z for all values of z lying within a disk where the function is finite and continuous. However, having no clear idea about (possibly essential) singularities of entire functions—as nobody did at that time—Betti, like Briot and Bouquet, was then led into a series of sometimes

mistaken arguments that led him close to discovering the Weierstrass factorisation theorem some years before Weierstrass.

Betti claimed that the properties of polynomial functions could be “easily extended” to entire functions, and he mistakenly stated that “an entire function always becomes infinite for $z = \infty$ ” (Betti 1860–1861, 234). He claimed that this result was a consequence of Liouville’s theorem (without naming him, either) and he went on to argue that therefore “an entire function has always at least one (finite or infinite) root” (zero would be the modern term), before concluding (correctly) that an entire function having no finite roots has the form e^w , w being an entire function. Then he proved that if α is a root of an entire function w , then w can be divided by $(1 - z/\alpha)$ and also that a function having only a finite number of finite roots, and no infinite root, is a rational entire function, i.e. a polynomial.

Betti then reformulated a theorem from Briot and Bouquet (1859, 39) which asserted that two monodromic and monogenic functions with the same zeros and infinities differ from one another only by a constant factor. This false “result” is the source of many of the errors in the book of Briot and Bouquet. According to Casorati, it originated in their failing to appreciate the difference between a function “ceasing to be finite” and “ceasing to be continuous and monodromic”. Betti, however, deduced that: two entire functions having the same roots can only differ from each other by a factor of the form e^w , w being an entire function. He then used an argument about line integrals to show that entire functions which are not the product of a polynomial and an exponential function of the form e^w , w being an entire function, have an infinite number of roots which cannot form a continuous line in the complex plane. He then concluded (1860–1861, 239) that the roots of entire functions satisfying the above condition “could never be included in a finite (i.e. bounded) domain of the plane”.

Then he referred again to Briot and Bouquet (1859, 135) to state the theorem that if the infinite product $\prod(1 - z/\alpha)$, α being the roots of an entire function W , is convergent for every finite value of z , it represents an entire function that has all and only the values α for its roots. Therefore W can be expressed as $W = e^w \prod(1 - z/\alpha)$, w being an entire function. As Weierstrass had done in his 1856 paper on analytic faculties in Crelle’s *Journal*, the proof of the convergence of the infinite product was reduced by Betti to the proof of the convergence of an infinite series by taking its logarithm. Betti then showed that the infinite series is absolutely convergent by comparing it with $\sum \frac{1}{\alpha_i^\mu}$ for $\mu > 2$ (where the α ’s are a finite distance from each other—that is, they do not get arbitrarily close together).

Conversely, given an infinite system of complex numbers α “which do not form a continuous curve” one can always build an entire function having the numbers α , and only them, as its roots. To this end Betti first proved partial results by distinguishing between entire functions of the first species (those having their roots on a straight line) and functions of the second species (whose roots lay in the plane at a finite distance from each other) and eventually he stated the general result that for any given (finite or infinite) system of complex numbers α such that $|\alpha_i - \alpha_j| < \infty$ for $i \neq j$ one can form an infinite product of linear factors like $(1 - z/\alpha_i)$ and of exponential factors like $(1 + a)^z$ or $(1 + a)^{z^2}$, where $a = 2\alpha_i^2$,

such that it represents an entire function whose roots are all the values α_i alone (Betti 1860–1861, 246). Although flawed by the inaccuracies that the Casorati–Weierstrass theorem was to expose very soon, this theorem was a significant step toward Weierstrass's factorisation theorem, which, however, went almost unnoticed.

The origins of the mistakes in Betti's argument can be traced back to the book by Briot and Bouquet. Dugac undoubtedly went too far by stating that Briot and Bouquet “were among the early ones to study essential singular points”, and by attributing them the (admittedly still incorrect) theorem that “a holomorphic function in the plane, having an essential singular point at infinity, takes all possible values” (Dugac 1973, 58). In fact, Briot and Bouquet remarked that “it can happen that the same value of z simultaneously makes a function zero and infinite”, as is shown by $e^{1/x}$ according to whether one approaches zero from the right or the left half plane with respect to y . The same behaviour is exhibited by e^x for infinite values of x . As a consequence of this remark they stated that “a function monogenic and monodromic in the whole plane takes all possible values” (Briot and Bouquet 1859, 38).

Betti then considered fractional, i.e. meromorphic functions. Following Riemann (1851), he proved the removable singularity theorem and obtained the Laurent tail of a function at a pole of order μ . The fact that the poles of a monogenic and monodromic function w cannot form a continuous curve in the complex plane was an obvious consequence of the analogous theorem concerning the zeros of $1/w$ from which he derived the quotient representation of meromorphic functions. Thus, Betti concluded, monogenic and monodromic functions can be either entire or fractional, and he developed their theory in Parts I and II of his treatise accordingly.

The study of entire functions and their representation in infinite products provided Betti with the necessary foundation for introducing Jacobi's theta series in a more direct manner. In Part I he did it “step by step” beginning with the new entire function he defined:

$$es(z) = z \prod_1^{\infty} \left(\frac{m}{m+1} \right)^z \left(1 + \frac{z}{m} \right),$$

whose zeros lay on a straight line, and which is characterised by the functional equations

$$es(0) = 0, es(z+1) = \frac{1}{z} es(z), \lim_{w \rightarrow \infty} \frac{es(z+w)}{es(w)} w^z = 1, \lim_{z \rightarrow 0} \frac{es(z)}{z} = 1. \quad (5.49)$$

Betti pointed out that $\Gamma(z) = 1/es(z)$. As we shall see below (Sect. 7.2.4) the study of the Γ -function was also Weierstrass's starting point for his discovery of prime functions and the factorisation theorem.

Then Betti introduced entire functions whose zeros lay on a lattice $m\omega + n\omega' + \alpha$, ($m, n \in \mathbb{Z}$, $\omega, \omega' \in \mathbb{C}$, $\text{Im}(\omega'/\omega) > 0$), thus obtaining Jacobi's theta series. In fact, the functions satisfying the required conditions are of the form $\phi(z)\theta(z-\alpha)(z)$, $\phi(z)$ being an entire function without finite roots. He developed the theory of “Jacobian functions” in great detail, obtaining in passing Weierstrass's AI -functions. In Part II he developed the theory of elliptic functions considered as a quotients

of two theta series, including their transformations and a study of the modular equations as well.

Perhaps because the *Annali* had not yet reached the rank of an international journal, Betti's paper apparently escaped the attention of foreign readers including Weierstrass, who did not mention it in his influential (1876b) paper on infinite-product expansions of single-valued complex functions, where the factorisation theorem was established in its full generality independently of Betti's work (see Sect. 6.7). It seems that Weierstrass discovered this as a matter of "curiosity", according to what he wrote to Sonya Kovalevskaya on June 9, 1881: "I have found a curious thing. Herr Betti should have asserted that [the theorem about] an entire transcendental function with given zeros, of which only finitely many occur in any finite domain, is properly due to him; for he had, in order to prove it, only to have had the idea that the entire function which enters in the exponential factor associated to each linear factor, cannot have the same degree in each factor. How sad that the right ideas do not always arise at the right time".⁶⁵

While publishing his lectures on elliptic functions, Betti also published another paper (Betti 1862) where he gave a purely algebraic treatment of a subject largely inspired by Riemann (1857c).⁶⁶ Given an algebraic function $z(t)$ defined by the (irreducible) equation

$$F(z, t) = \sum_{r=0}^{\mu} \sum_{s=0}^{\nu} A_{r,s} t^s z^r.$$

Betti studied the behaviour of the μ branches of the function z as Puiseux had done, by means of suitable paths and the group of substitutions acting on them, including a detour on their "infinities and infinitesimals" (i.e. poles and zeros) and the relationships on their number. Then he determined the branch points as the zeros of the discriminant

$$D(t) = A_0^{2(\mu-1)} \prod (z_m - z_n) = A_0^{\mu-2} \prod_{r=1}^{\mu} \left(\frac{\partial F}{\partial z} \right)_{z=z_r}$$

⁶⁵See Bölling (1993, 253). This letter and the letters of Schwarz to Weierstrass we will quote below are kept in the library of the Mittag-Leffler Institute. We would like to thank the former Director of the Institute, Dr. Laksov, for his kind permission to publish excerpts of them.

⁶⁶This paper originated in a course on this subject Betti gave in 1861/62. He lectured on this subject several more times in the 1860s. Lecture notes for his 1862/63 course, taken by Ulisse Dini and held in the library of the Department of Mathematics of the University of Florence, have been published in Petti (2002). The same library also holds Antonio Roiti's notes of Betti's 1867/68 course, published in Petti (2003).

and, resorting to geometrical arguments, he determined the order of connection of an algebraic equation, i.e. the number

$$p = (\mu - 1)(\nu - 1) - \tau,$$

where τ is the number of double points on the curve.

Eventually he referred to Riemann's (1857c) paper to state that, contrary to what Riemann had done, the characteristic properties of algebraic functions—that is, the necessary and sufficient conditions for determining them—can be obtained directly by algebraic and geometric reasoning without resorting to integrals. This was a consequence of the following theorem, whose proof was given by an algebraic counting argument:

It is always possible to determine a finite number of algebraic equations which have μ branches, order of connection $2p + 1$, their branch-points arbitrarily given in the plane, ν infinities of first order and $\mu - p$ infinitesimals of first order arbitrarily given, and which assume an arbitrarily given value for a given value of the variable, and two of such functions which have the same branching are identical (Betti 1862, 39).

In the last paragraph he tackled the problem of classifying algebraic functions. Using purely algebraic reasoning he proved that “algebraic equations that define algebraic functions which have the same order of connections and, considered as functions of any of them, have the same branching, belong all to the same class; and vice versa” (Betti 1862, 43).

Irreducible algebraic equations, whose coefficients are rational entire functions of one variable, were said by Betti to be of “order” p if the order of connection of the algebraic functions they define is $2p + 1$. Each order is divided into an infinite number of distinct classes, such that all the equations contained in each of them can be rationally transformed into each other. These classes differ from each other by a finite number of constants (the moduli belonging to a given order). Eventually Betti concluded by stating the theorem that “classes of algebraic equations of first order differ from each other for the value of one modulus. Classes of algebraic equations of order p differ from each other according to the values of $3p - 3$ moduli” (Betti 1862, 45).

Betti gave an offprint of his paper to Riemann, who was visiting Pisa on his way back to Göttingen. Riemann replied that he had appreciated the paper very much and wished to have it translated into German, adding significantly:

My representation of functions by means of surfaces, in the form in which I have presented in Borchardt,⁶⁷ has raised difficulties even for German readers. In my lectures I was able to make this way of representing clear and familiar to my listeners by means of suitably chosen examples. But in any case your derivation of the algebraic results without this help and without resorting to integrals along a purely algebraic road is very desirable (quoted in Bottazzini 1983, 255).

⁶⁷With this Riemann referred to his (1857c) appeared in Crelle's *Journal* whose editor-in-chief was Borchardt.

This was the beginning of a friendship which strengthened in the last years of Riemann's life. In July 1863 Betti offered Riemann a chair in Pisa, but he refused the offer of his "most faithful friend" for "probably as a consequence of a too-hurried trip" to Göttingen he felt "a deterioration in my health which, while scarcely dangerous, makes it impossible. I am only able to speak with a great deal of effort", and this had become, he said, a continuing inconvenience (quoted in [Bottazzini 1983](#), 256). Indeed, on his doctor's advice he again left Göttingen for Italy in the autumn of 1863. Afterwards, Betti wrote to Tardi on 6 October 1863 (in [Cerroni and Martini 2009](#), 82) that

Kronecker once said to his brother [Karl Hugo] that he would have liked to spend one year together with Riemann to venture into his speculations, and we have the chance that [Riemann] himself came to visit us. We must take advantage of this, and not miss this opportunity.

So Riemann was staying in Pisa when in the opening lecture of the academic year 1863/64 Betti explained that in his view Riemann's

method has the advantages over the others of its immense generality and of completely satisfying the principal tendencies of modern analysis, since the mechanism of the calculus hardly enters at all and it is almost entirely a magnificent work of pure thought. But so great is the force of the mind, so great is the concision and obscurity of the style of this eminent geometer, that at the moment it is as if his work did not exist in the scientific world (quoted in [Bottazzini 1983](#), 250).

Betti was not a blind follower of Riemann's, as his papers on elliptic and algebraic functions show. Nonetheless, they can be seen as his own contribution to bringing Riemann's essential ideas into the mathematical milieu. However, they too went almost unnoticed, and very soon thereafter Betti left complex analysis for mathematical physics.

In the early years Riemann spent in Pisa Betti took the opportunity of discussing various ideas with him. These included topological issues such as the connectivity of spaces⁶⁸ which he published well after Riemann's death ([Betti 1870](#)) and the origin of the idea of cross-cuts "which came to Riemann's mind because of a definition Gauss gave him of them during a conversation on another subject", as well as subtleties about continuity and differentiability of functions (perhaps Riemann's counterexample, see Sect. 6.10) which, however, were misunderstood by his Italian listeners (see [Bottazzini 1983](#), 257). But very soon Riemann's health deteriorated to the point that he was almost unable to speak, as Betti recorded in his letters to Casorati. We quote one moving passage.⁶⁹

⁶⁸"I've spoken with Riemann once again about the connection of spaces and I've got an exact idea of it", Betti wrote to Tardy on Oct, 6th 1863 (quoted in [Bottazzini 1983](#), 256). Most of the content of [Betti \(1870\)](#) is included in a letter to Tardy on October 16th 1863. For an English translation see Weil (1979b).

⁶⁹Betti to Casorati on January 26th, 1866. The letters from Betti to Casorati are kept in Casorati's *Nachlass* in Pavia. We would like to thank Ing. A. Gabba for his kind permission to publish excerpts of them.

I have no news of our Riemann for a month, but I almost do not want to have any for the last letters that I got from his doctor in Göttingen were very pessimistic. Unfortunately it seems that there is no hope left of saving him. It is very painful to have to lose such a strong mind, such a dear person so early. Many things that he has already done in his mind will be lost. Talking was so tiring and harmful to him that little could be gained from the talks one had with him. When Prym was here and forced him to speak slightly more than usual suddenly his condition very seriously worsened. Most recently he has dealt with the applications of Abelian functions to geometry and number theory. As for geometry he told me that Clebsch still had a long way to go. In former times when he was here he could speak much more; but it was tiring following his speech because he wanted to speak in Italian and could find the words only with difficulty and one had to make guesses in order to suggest the missing words to him. But I hope you will visit me soon, and then we will talk about Riemann and about your travel in Germany, about which I am curious to have news.

5.11.6 *Algebraic Geometry*

A geometer today naturally thinks of the equation $f(x,y) = 0$, where f is a polynomial in x and y , as defining a curve in the Cartesian plane with coordinates x and y . One reason for the introduction of algebra proved successful is that it is a flexible and powerful technique for defining curves and proving things about them. For example, polynomial equations have as many solutions as their degree indicates, so it is possible to count the points of intersection of a curve with a line. More generally, it is possible to count the common points of two curves: Bezout's theorem says that two curves of degrees m and n , respectively, have mn points in common. The price is that the solutions are generally complex but, strangely, the implications for geometry were but slowly picked up by mathematicians. Who, one might ask, was the first to endow a curve with all its complex points?

It might seem that the answer to this question should have been given during the eighteenth century, by Euler, for example. In 1750 he published an attractive paper (1748b) on Bezout's theorem. He showed that one must add points at infinity and that one must define the multiplicity of an intersection. Finally, and for our present purposes most interestingly, the necessary intersections may not exist; a line may fail to meet a conic at all. The points of intersection are then complex; Euler talked of imaginary intersections. But the clear implication was that a curve is a real thing, while algebra may give you intersection points that are not there, they are fictitious imaginary ones. Euler's view was not to be contested in that "most algebraic of centuries". After the revival of interest in the nature of complex numbers, and throughout the first half of the nineteenth century, geometers remained ambiguous about the meaning of complex quantities in geometry. Plücker, a leading exponent in the revival of algebraic geometry that then got underway, announced in 1834 a result that is still attractive, and serves as a good peg for this discussion: a plane algebraic curve of degree 4 has 28 bitangents. In his book of 1839 he gave a detailed account of these bitangents, showing that all 28 could be real, but that they need not be. He enumerated all the cases that can arise of N real bitangents and $28 - N$ imaginary ones. His methods were entirely algebraic, and when imaginary

tangents and their imaginary points of contact were to be investigated, Plücker wrote expressions of this form $p' + p''\sqrt{-1}$.

All this suggests that he considered as imaginary points (and lines) those whose coordinates (or coefficients) are imaginary. And indeed he spoke of not merely the contact points of the bitangents but whole branches of a curve being imaginary (see (Plücker 1839, 231 and 233)). But this impression does not exactly fit the case. For Plücker the original curve was always a real curve. He therefore felt he had to confront the question: what is the real (i.e. true) interpretation of an imaginary tangent?

The answer he gave in 1828 was in terms of involutions. An involution on the real projective line is a map of the line to itself of period 2. As such it has fixed points; two real distinct ones, or a real repeated one, or two conjugate imaginary ones. In this last case the involution is said to be elliptic. Plücker interpreted pairs of conjugate imaginary points (say, the imaginary contact points of a bitangent) as meaning that there was an elliptic involution with those points as its fixed points. In 1847 he had shifted his position and said that to speak of imaginary points was “to give geometrical clothing to an algebraic fact” (Plücker 1847, 412). Even the later position falls short of putting complex points on a par with real ones; complex points are still to be explained geometrically in terms of something real. This theory was vital in its day. It was taken up by von Staudt in the 1850s, and it is usually he who is credited with the creation of complex geometry as a result. In his *Beiträge zur Geometrie der Lage* (1856–1860) he developed an intricate theory of complex lines and planes in what has been called complex projective three-space, all in terms of involutions of various pencils (of points, lines, or planes). But is not a theory of complex projective space in the modern sense of the term.⁷⁰

Von Staudt’s approach was extended by many writers.⁷¹ Klein, a former student of Plücker’s, used it to draw what he called new Riemann surfaces associated with real algebraic curves, in this way.⁷² Count the number of imaginary tangents to the curve from each point of the plane; for example, it is 2 for points inside an ellipse, 0 for points outside. Associate with each point as many points of the new surface as there are imaginary tangents to the curve. This produces a real surface which you can think of as an ellipsoid, and Klein claimed that in this way you will always get a real surface equivalent to the Riemann surface of a curve in Riemann’s sense. Plainly this is not the modern point of view, and that tells us that in its day this theory, so closely tied to one’s sense of geometry as being about real things, was a solution to the question “what are the complex points of a curve?”. The problem was solved in such a way that the modern solution did not get a look-in.

To see where complex points come from, we might return to elliptic function theory, and Cauchy’s paper of 1846 described above. There he spoke of the

⁷⁰For an account of the many and various interpretations of the imaginary in geometry, see Coolidge (1924).

⁷¹See Lüroth (1875, 1877), Stolz (1871), and Nabonnand (2008).

⁷²See Klein (1874), recapitulated it in his 1926–1927 *Entwicklung*.

“complex variable y (that) satisfies the finite equation $y^2 = (1 - x^2)(1 - k^2x^2)$ (and) varies with x by insensible degrees” (Cauchy 1846i, 168). Then in 1850 Puiseux, largely inspired by Cauchy's work, wrote a long paper of his own in which—after Riemann's work, we would say—it is quite clear that a function $f(u, z) = 0$, as he wrote it, of degree m in u , can be thought of as a m -sheeted covering of the z -plane branched over a number of points (see above, Sect. 3.6.1). But analysis is not geometry. The main proponent of a complex theory of geometrical curves seems to have been Riemann. Riemann's idea that an algebraic curve with equation $f(s, z) = 0$ of degree n in s and m in z , where s and z are complex variables, can be thought of as an n -fold covering of the complex z -plane (or, better, z -sphere) made available for algebraic functions all the resources of complex function theory. As we discussed in Sect. 5.2.2, Riemann surfaces are a good example of Riemann's philosophy of magnitudes and quantities at work. Moreover, from Riemann's standpoint there was no need for a real interpretation of complex points on a curve. For the first time, a curve may simply have complex points on it.

Historians rightly point out that Riemann's ideas took time to get across. Even geometers found the idea of a Riemann surface difficult, not least because of the difficulty in accepting that the real and complex points satisfying a given equation be accepted equally as points of a surface.⁷³ Such an idea is implicit in the work of Abel and Jacobi on elliptic functions, but it was never interpreted geometrically. The geometrical theory of complex quantities sought to give a real interpretation to complex points as involutions, so while it was entirely acceptable by mid-century to regard a complex quantity as a point in a real plane (often called the Gaussian plane) the acceptance of complex points on a curve, and their presentation as a surface, seems to have been difficult to accept. Difficult to a trained geometer, because there was a theory in place that had the virtue of starting with real points. This was a theory people had invested heavily in, as we saw, and would not lightly be abandoned. Difficult because it broke entirely with the ontology that started from real objects. And difficult because, indeed, it is hard, given an equation for a complex curve, to see and get to grips with the associated Riemann surface.

Riemann's bold approach of seeing an equation of the form $f(z, w) = 0$ as defining a surface spread out somewhere and lying over the complex sphere was an extra barrier to the ready reception of his ideas. When in 1878 Cayley explained how a curve can be thought of as a set of points in $\mathbb{C} \times \mathbb{C}$, he explicitly remarked that “I was under the impression that the theory was a known one; but I have not found it anywhere set out in detail” (Cayley 1878, 317). But Cayley distinguished sharply between Riemann's approach and the naive idea that a complex curve was a set of points in $\mathbb{C} \times \mathbb{C}$. This gives a further point to the question of how did Riemann's ideas reach the mainstream. The answer, so far as algebraic geometry is concerned, seems to have been Clebsch, who took them up quite deliberately in 1862. From then until his death ten years later at the age of 39 Clebsch was the leader of a growing

⁷³For the response of the analysts see Bottazzini, *The Higher Calculus*. For the geometers' response, see Gray (1989).

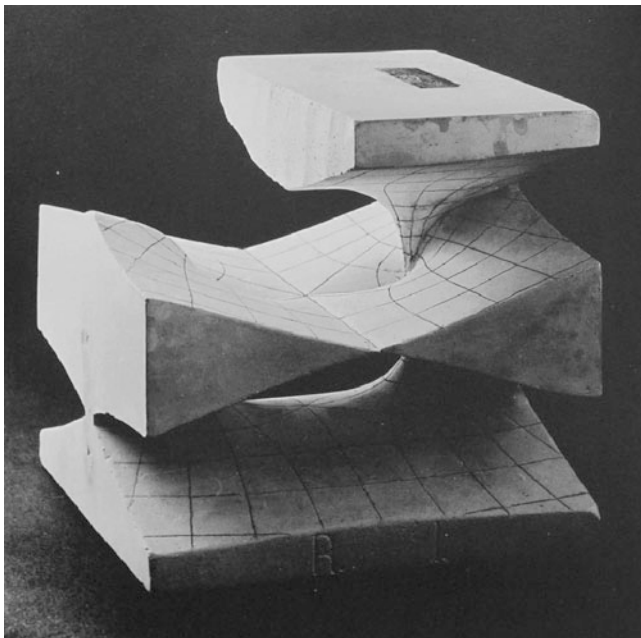


Fig. 5.5 The Riemann surface $w^4 = 1 - z^2$ (Fischer, *Mathematische Modelle – Mathematical Models*, Vieweg © 1986, number 125)

group of German geometers, and he and his colleague Carl Neumann founded the *Mathematische Annalen* in 1869 as a forum for their views.

We note here that Riemann surfaces became a natural topic for the commercial makers of mathematical models (see [Fischer 1986](#)), as this example marketed from 1880 by Kuen and Wolff, Munich, shows (Fig. 5.5).

5.11.6.1 Quartic Curves

Riemann had outlined a theory of complex curves, and it was taken up by Clebsch, Weber, and others. In February 1862 Riemann had lectured on the 28 bitangents to a plane quartic, and Roch's notes were later published.⁷⁴ His paper on Abelian functions of 1857 underlay his account of algebraic curves. He let X be a non-singular plane curve given by an equation $f(x, y) = 0$ of degree n . A holomorphic 1-form on it will be of the form $\frac{adx}{(\partial f / \partial y)}$ where a is of degree $n - 3$. If $n = 4$, then the genus is 3 and $n - 3 = 1$, so a basis for these things will be found on successively letting $a = x, a = y$, and $a = 1$. The general form for a will therefore be $mx + ny + k$,

⁷⁴To be found in Riemann (1990, 519–536).

which equated to zero is the equation of a line. Such a line meets a quartic in four points, illustrating the theorem that a holomorphic 1-form on a curve of genus g has $2g - 2$ zeros.

Riemann's general theory gave him $2^{2g} = 64$ cases where something strange occurs. Riemann showed that what happened could be described by his theory of theta functions with characteristic. This is a generalisation of Jacobi's theory in the elliptic case to the general setting. Riemann's thinking was somewhat along these lines. The quotient of two 1-forms will be a/a' , say, a function on X whose zeros are along the line with equation $a = 0$. If a and a' are chosen carefully, these zeros can be made to occur in pairs, and then the line is a bitangent. The careful choice is made (in a way not to be described here) by the theory of characteristics. But there was more. In homogeneous coordinates x, y, z the original curve may have an awful equation. By letting a, a' , and $a'' = 0$ be coordinate axes it is possible to simplify the equation, indeed to a form originally given by Plücker, but in a more reliable way. Moreover, the labelling on the bitangents now enables one to deduce theorems about the families they occur in,⁷⁵ while the transition to Plücker's form enabled Riemann to write down their equations explicitly. All told, a rich geometrical harvest.

5.11.7 Clebsch's Work

The decisive influence on spreading this way of thinking was that of Clebsch. As we noted above (see p. 317) in his important paper (1864) he first observed that elliptic functions can be used to parameterise points on a cubic curve, and in this way obtained results that had been earlier claimed, without proof, by Steiner, remarking that "many of the problems solved [by Steiner] first find their true analytic expression in the theory of elliptic functions" (Clebsch 1864, 189). Then he generalised and extended what Riemann had done in lectures and dealt with a wide variety of topics in the theory of curves by means of Riemann's theory of theta functions.

Clebsch first reviewed Riemann's proof of Abel's theorem for a curve of order n having at worst double points, whose equation is $f(x_1, x_2, x_3) = 0$. Abel's theorem says that if the end points of a sum of integrals are algebraically related then a certain sum of integrals vanishes, the converse asserts that if the sum of integrals vanishes then the end points are algebraically related.⁷⁶ Clebsch gave a geometric interpretation of Abel's theorem. He interpreted it as being about the sums of

⁷⁵They occur in families or groupings of six pairs, any two of which reduce the equation to Plücker's form; they are recognised as the sets of characteristics all pairs of which have the same sum. Sets of 4 bitangents determine 8 points on both the curve and a conic, sets of 6 are connected with cubics, and so forth, according to a rich geometric theory developed in Hesse (1855).

⁷⁶Clebsch, more precisely, considered the sums of integrals to be zero modulo the period lattice.

integrals taken along between a fixed lower end point and set of points cut out on the curve by another curve, $\phi = 0$, of order m which depends linearly on some parameters. The integrands are of the form of a product of a rational function of order $n - 3$ with an expression of the form

$$\frac{\sum c_1 x_2 dx_3}{c_1 \frac{\partial f}{\partial x_1} + c_2 \frac{\partial f}{\partial x_2} + c_3 \frac{\partial f}{\partial x_3}}$$

(the sum is over all suitable permutations of the indices); these are the most general holomorphic integrands. The sums of the integrals then depend on the variable coefficients of ϕ . Since all the upper end points enter symmetrically, this dependence is determined by the rational function, and so gives rise to only rational, logarithmic and trigonometric functions of the coefficients. This is the geometric interpretation of Abel's theorem. In particular, if the rational function is a polynomial, then the sum of the integrals vanishes and is therefore independent of the variable coefficients of the curve ϕ .

To prove the converse Clebsch restricted his attention to the case of non-singular curves—claiming rather optimistically that all other cases were just special cases of this one. He then tightened the claim made in Abel's theorem to highlight the way the end points depend on the coefficients of the curve $\phi = 0$. The fixed curve has degree n and genus $p = \frac{(n-1)(n-2)}{2}$ and Clebsch denoted the everywhere finite integrals on it, as Riemann had done, $u_1, u_2, \dots, u_p$. He denoted the value of u_i at a point P_j by $u_i^{(j)}$. As the points P_j vary, the possible values of the $u_i^{(j)}$ can only change as points cross the cuts, in the way that Riemann had described. If each sum $u_i^{(1)} + u_i^{(2)} + \dots + u_i^{(mn)}$ is a constant, say γ_i , then by considering the special case where the variable curve of degree m consists of m lines, Clebsch deduced that $\gamma_i = c_i \gamma$, where the c_i are independent of not only the coefficients of ϕ but also of its degree. He could therefore suppose that the degree of ϕ was $m = n - 3$. But in this case Riemann had shown, apart from some small print about double points that was moot in this case by assumption, that if the P_j depend linearly on the coefficients in the numerators of the integrands, then the sum of the integrals was congruent to zero modulo periods.

Now, conversely, if the sum of the integrals is zero, the differential equations

$$du_j^{(1)} + du_j^{(2)} + \dots + du_j^{(mn)} = 0, \quad 1 \leq j \leq p \quad (5.50)$$

are solved by p algebraic equations provided by Abel's theorem. These algebraic equations show that the mn points lie on a curve of order m , because the solutions are determined precisely by, at most, the mn points. They therefore coincide with the (possibly transcendental) solutions Riemann had shown to exist. Moreover, because these equations depend on p arbitrary parameters, the mn points must be determined by a suitable choice of these arbitrary constants. For such a suitable choice, the algebraic solutions must each have the form which ensures that the mn points are the

intersection with a curve of the m th order and that depend linearly on the coefficients in the integrands.

In their extensive history Brill and Noether (1894, 324) commented that this does not show that the algebraic function just determined, which may be taken to have simple poles at one set of mn points and simple zeros at another set of mn points, is of the form C'_m/C_m where C_m and C'_m are of degree m in x_1, x_2, x_3 . This follows, as Clebsch had argued, by counting constants, but only in the case where the curve has no singular points. Otherwise, one is forced into the sorts of considerations Roch had introduced in proving his part of the Riemann–Roch theorem. Even so, Clebsch's argument still rests on a transcendental argument at one crucial stage.

Clebsch also pushed the theory of the bitangents to a plane quartic further, re-derived results of Hesse's, and found results about curves of higher degree. He was followed almost immediately by Roch (1866a) and in 1876 by Weber, who, however, confined their attention to the quartic and its bitangents. The paper began a whole school of work in algebraic geometry, developed by Brill and Noether after Clebsch's death in 1872, in which the functions $a(x, y, z)$ that enter expressions like $\frac{a(x, y, z)dx}{\partial f/\partial y}$ are interpreted as curves of certain degrees, and which, as occasion demanded, be subject to such conditions as to pass $k - 1$ times through each k -fold singular point of the fixed curve. In this way analysis and geometry were firmly brought together, and the concept of a complex curve was created in something like its modern form. But it was progress bought at a price, because the abstract formulation of Riemann, which in principle separated questions about curves from questions about curves embedded in the plane or space, was reworked by them firmly as a topic in the theory of plane curves, thus confusing geometric questions of two kinds and forcing analysts and geometers into an uneasy collusion over what was truly known.

5.11.8 The Riemann–Roch Theorem

Brill and Noether followed Clebsch in treating the subject geometrically, that is, in terms of a linear family of adjoint curves to the given “ground” curve, which is a possibly singular plane curve of degree n (a curve is an adjoint curve if it passes $k - 1$ times through every k -fold point of the ground curve). They generalised the Clebsch–Gordan definition of the genus g of a curve to a curve having arbitrary ordinary singularities, and then proved that the number of free coefficients in the equation for an adjoint curve of degree $n - 3$ to a curve of degree n is $g - 1$. They also showed that the total number of intersection points of the curve and its adjoint apart from the multiple points is $2g - 2$, so at most $g - 1$ of these are determined by the rest. In other words at least $2g - 2 - (g - 1) = g - 1$ can be chosen arbitrarily. Therefore q , the dimension of the space of adjoint curves of order $n - 3$ that cut out a set of Q points satisfies the inequality $q \geq Q - g + 1$.

Brill and Noether next proved the converse by using induction on q and Q . They showed that there is a q -dimensional family of adjoint curves of degree $n - 3$ that cut out a set of Q points on the ground curve, provided $q \geq Q - g + 1$. This is their version of the Riemann inequality. They stated their version of the Riemann–Roch theorem in terms of what they called special families: a special family satisfies the strict inequality $q > Q - g + 1$. The Brill–Noether version of the Riemann–Roch theorem then says: If an adjoint curve of order $n - 3$ is drawn through a special set of Q points in a q -dimensional family of points, for which $q = Q - g + 1 + r$ (where $0 < r < g - 1$), then this curve meets the given curve in $2g - 2 - Q = R$ further points that themselves belong to a special set of R points in an r -dimensional family, where $r = R - g + 1 + q$.

Klein, when he lectured on this material almost 20 years later, in (Klein 1892), was rightly critical of Brill and Noether’s strong preference for algebra and geometry over function theory. He observed that the connection to Riemannian function theory is made by observing that if ϕ_0 and ϕ_∞ are two adjoint curves of any order, then their quotient $\frac{\phi_0}{\phi_\infty}$ is a meromorphic function on the algebraic curve having prescribed zeros where the curve $\phi_0 = 0$ meets the ground curve and prescribed poles where the curve $\phi_\infty = 0$ meets the ground curve. As Klein commented, this is a theorem “which Brill and Noether everywhere suppose but never properly derive” (Klein, 1892, 189). Relations between Klein and these followers of Clebsch had become strained as Klein had moved in the late 1880s to adopt the mantle of Riemann and become his true successor at Göttingen. His enthusiasm for intuitive geometry grated with their preference for the certainties of algebra.

For Brill and Noether, the Riemann–Roch theorem was a theorem about families of plane curves. The first to use higher-dimensional geometry in this context was the English mathematician and acolyte of Riemann, William Kingdon Clifford. Clifford’s paper on the embedding of algebraic curves in projective space is regarded these days as one of the first significant applications of the Riemann–Roch theorem. In (Clifford 1878) he argued as follows. If a curve of order n and genus g lies in $\mathbb{CP}^k$, then a hyperplane meets it in n points. But the hyperplane is determined by k points. Abel’s theorem gives the condition that once k arbitrary points are chosen on the curve a further $n - k$ may be chosen so that the sum of each g Abelian integrals taken from a fixed starting point to these n points vanishes. This gives g equations for the $n - k$ points in terms of the arbitrary k points. If $g \leq n - k$ there are solutions for these equations, but if $g > n - k$ then there can only be solutions if points satisfy a further $r = g - (n - k)$ conditions. This condition of linear dependence is a concealed reference to the Riemann–Roch theorem. It reduces to the statement that if $g > n - k$ then $k \leq n/2$.⁷⁷ Or, in more overtly geometrical terms: a curve of order n and genus $g \leq n/2$ can exist in at most $n - g$ dimensions.⁷⁸

⁷⁷This is an improvement on the Weierstrass gap theorem; see Sect. 6.8.6.

⁷⁸Modern interpretations of Clifford’s theorem deduce from it results about the canonical forms of curves of low genus, such as every non-hyperelliptic curve of genus 3 embeds in $\mathbb{CP}^2$ as a smooth

Clifford's work was not initially noticed, and he was followed by L. Kraus (who had studied under Klein and Weierstrass and died at the age of 27) and E.B. Christoffel, although credit has usually been given to Noether. All started from the observation that an algebraic curve of genus $g > 1$ has a g -dimensional space of holomorphic 1-forms. While (Christoffel 1879) and Noether pursued the analytic implications of taking a basis for these 1-forms, say $(\omega_1, \dots, \omega_p)$, Kraus (1880) thought of the p -tuple $(\omega_1, \dots, \omega_p) = (f_1(z)dz, \dots, f_p(z)dz)$, as giving a map from the curve to a projective space of dimension $p - 1 : z \mapsto [f_1(z), \dots, f_p(z)]$. Kraus fudged the proof that this map is well defined (independent of the coordinate system used) and is a map into projective space (the functions f_i do not simultaneously vanish); a modern simple proof uses the Riemann–Roch theorem. Riemann's work had already made it explicit that the degree of the map is $2g - 2$. But Kraus did see that the result is false for hyperelliptic curves: instead one gets a $2 - 1$ map from the curve to the Riemann sphere. The novelty of Kraus's insight, which Klein appreciated, is the emphasis on higher-dimensional geometry, which ensures that for all but the hyperelliptic curves questions about the curve, or whole families of curves, can be reduced to questions in projective geometry.

A history of algebraic geometry in the nineteenth century would fill a book.⁷⁹ Of the many topics that might be discussed, some spring to the fore. The genus of a curve; how should it be defined, what does it mean? Is it a projective or a topological invariant? Because a complex curve has two real dimensions, one may also ask for the first appreciation that a complex curve has an intrinsic geometry in the sense of Gauss. Finally, one might ask about the study of complex surfaces. The magnitude of each of these topics that we must set aside illustrates well how the introduction of complex variables changed and enriched the practice of geometry.

5.12 Cryptic Messages

Riemann never published a truly elementary account of his complex function theory. His contemporaries and immediate successors shared his naiveté in topological matters and found that they could agree with many things that he had taken for granted, but later generations often found them in serious need of reformulation. It is necessary to hold these perceptions apart if the nature of Riemann's gift to his fellow mathematicians is to be properly appreciated.

Central to Riemann's idea of a complex function of a complex variable was that the derivative existed and was independent of direction. This of course implied that

quartic, every non-hyperelliptic curve of genus 4 embeds in $\mathbb{CP}^3$ as a curve of degree 6 which is the intersection of a quadratic and a cubic surface, and all but an identifiable class of non-hyperelliptic curves of genus 5 embed in $\mathbb{CP}^4$ as a smooth curve of degree 8 that is the intersection of three quadrics. See Miranda (1995, Chap. VII).

⁷⁹Gray (1989, 1998) are a start, see the references there, notably (Dieudonné 1974).

the Cauchy–Riemann equations held and that the function was conformal everywhere that the derivative did not vanish, but much more important in Riemann’s eyes was that this licensed complex function theory on, if not *any* surface, then at least on all the ones you could think of, including all the ones defined by a polynomial equation in two variables. He was clear early on that these latter kinds of surface were classified purely topologically and soon discovered that there was a finer classification by their moduli, which parameterised the different complex structures (to use a modern term) that a surface could admit. He extended this analysis to include transcendental functions given as the solutions of a differential equation, and in his lectures began to outline the implications for the geometry of algebraic curves. His profound paper on Abelian functions extended these ideas to the theory of theta functions of several variables, but here even his geometric insight left much for others to do.

The paper on the distribution of prime numbers that spawned the Riemann hypothesis is much less geometric. It illuminates instead the way in which Riemann appreciated how complex analysis can play a major role in number theory. The elementary, but profound and unstated, insight is that the zeros and poles of a function can be made to yield numerical information via the Cauchy integral theorem. Nonetheless, the bold way that Riemann asserted properties of some of the functions that enter his account was to baffle those who followed him for thirty years, upon whom the burden of proof fell once the role of the visionary had been taken. The paper hints tantalisingly at what other insights Riemann might have had into complex function theory, but since no-one in the intervening years has turned his short paper into a convincing argument it might be wise to conclude that he, like Fermat on another famous occasion, may not have seen all the way to a proof.

Riemann’s work on complex function theory did not so much establish the field, in the sense of setting out the basic ideas in an orderly fashion, as show what new domains of mathematics it opened up, and what new questions could profitably be asked.

His work often has a paradoxical character: it is both profound and naïve. As a result it was easy to grasp the general message, but much harder to make it precise, and we shall see that significant advances were made in sharpening and making rigorous his intuitive presentations. This gives rise to the further paradox that the idea of a Riemann surface and a complex one-dimensional manifold are by no means the most difficult ideas in modern mathematics, yet the reception of Riemann’s ideas about what we now present in that way was slow and convoluted and proceeded along a route now marked with many abandoned formulations.

To take but one example, Riemann’s way of thinking was topological, except that topology did not exist as a subject in his day other than for a few insights. He presented an abstract surface and a theory of integrals on such a surface, but what an abstract surface not embedded in any ambient space might be was not made clear. The eponymous Riemann mapping theorem indicates that he thought of both the topological classification (there is only one surface of a given genus, all discs are topologically equivalent) and the kinds of complex structure the surface can have—all discs have the same structure. A great deal of work had to be done

to make Riemann's ideas make sense: Riemann had, for example, no idea what a disc might look like, or any idea of how to define a surface properly. It is not surprising, therefore, that even some of Riemann's admirers dropped some of his more profound ideas, and that others, less favourably disposed, sought to re-write his work in a way that also would have effaced it had they completely succeeded.

But Riemann was not solely a great conceptual thinker. He brought to his vision of what several branches of mathematics involving complex function theory should be like, the technical skill to realise many parts of that vision. Not only had he been able to master the existing, and inevitably technical literature, he could never have got to the position where he could contemplate letting the zeta function become complex, or the hypergeometric equation generate a Riemann surface with infinitely many leaves had he not possessed the ability to deliver the details. Later writers were often to find many of these details inadequate; they would not have bothered had not enough been securely in place. In his hands complex functions and their integrals, singular points and analytic continuation demonstrated the value of thinking about the largely unexplored class of complex differentiable functions. Indeed, his work is not so much a contribution to complex function theory *per se* but to showing that the new concepts and methods can very profitably be put to use in several areas of mathematics.

The challenge, one taken up more often than has been realised, was to do more in his geometric mode, one that balanced the qualitative and the quantitative sides of the new function theory. Just how naïve he had been, despite the profundity of his ideas, was to be left for the mathematicians of a later generation to discover. What he played down was the more algebraic aspects of the new subject; the immediately next generation was to turn in that direction, often in a spirit of more and more open rivalry.

Chapter 6

Weierstrass's Analytic Function Theory

The ultimate aim is always the representation of a function.

Weierstrass

6.1 Weierstrass's Early Years

Mathematical creativity is often said to be peculiar to young people. Counterexamples to this widespread opinion are indeed quite rare in the history of mathematics, but Weierstrass is one of them, and possibly the most remarkable. Even when he was 39, the age at which Riemann died, Weierstrass was still an unknown school teacher. Admittedly, by that time he had already written some five papers. Most of them, however, had remained in manuscript or had been printed in virtually unknown school programs. He was 40 when he published his first paper in Crelle's *Journal*, in which he solved the Jacobi inversion problem for hyperelliptic integrals (see Sects. 4.3 and 6.2.5). This paper announced the entry into the mathematical world of a major figure who was to dominate the scene for the next forty years.

Karl Weierstrass was born on October 31, 1815 in Ostenfelde near Münster. After going to primary school there he went to Paderborn where he attended the local catholic Gymnasium. Weierstrass's father had served as secretary to the mayor of Ostenfelde, then as a revenue official in the Prussian services and later as an administrator of the salt works at Westernkotten. Following his father's wishes, in 1834 the young Karl enrolled at the University of Bonn to study jurisprudence and administrative matters (*Kameralwissenschaft*), which prepared students for the higher public services. He spent four years there without getting a degree or even trying to pass any examination. According to a story first told by Mittag-Leffler (1923a, 14–15) who claimed he had it from Karl's brother Peter, the young Weierstrass was involved in *Jugendabenteuer* in Bonn, joined the *Korps Saxonia* and spent his time more in cellars and student fights than in studying.

This fascinating, romantic picture contrasts strikingly with the severe image of the master of absolute rigour commonly associated with Weierstrass's name and fame. However, it seems to match only part of the truth. According to Lorey (1915, 597) even at secondary school Weierstrass used to read Crelle's *Journal* and Steiner's papers in particular. (After Steiner's death in 1863 Weierstrass became the editor of Steiner's *Gesammelte Werke*, which were published in two volumes in 1881 and 1882.) In 1840 Weierstrass was required to write a report on his own training (*Studiengang*), and in it he said that in Bonn he had fully immersed himself in the study of mathematics following the suggestions of the late professor von Münchow, a professor of astronomy who also taught mathematical physics. In Weierstrass's recollections, von Münchow was "an insightful adviser". When von Münchow died in 1836 Plücker succeeded him as the professor Ordinarius of mathematical physics in Bonn, and according to Mittag-Leffler Weierstrass attended a semester's lectures by Plücker, but apparently the latter exerted no influence on him. Instead, and on his own, Weierstrass independently tackled the study of such difficult works as Laplace's *Mécanique céleste* and Jacobi's *Fundamenta nova*. In addition, he obtained from one of the Gudermann's students a copy of the lecture notes of Gudermann's course in Münster on elliptic functions (Mittag-Leffler 1923a, 14–15).

When one thinks of the impressive mathematical achievements Weierstrass was to accomplish in the early 1840s, one is led to believe that Weierstrass may have spent much more time in Bonn studying mathematics and less in cellars and student fights than Mittag-Leffler seems to have suggested. At all events, by the time he left Weierstrass realised that he had been mistaken in his choice of university studies, and after what must have been six difficult months at home he decided to devote himself completely to the study of mathematics.

6.1.1 A Student of Gudermann

Weierstrass left Bonn and returned to Münster in 1839, where he enrolled at the local Theological and Philosophical Academy in order to obtain a teacher's diploma. There he attended Christoph Gudermann's lectures on the theory of elliptic functions for a semester. Gudermann had just that year become a full professor of mathematics at Münster, where he worked on spherical geometry and special functions. He was one of the first to take up and develop Jacobi's theory of elliptic functions, and according to Lorey (1915, 600) Gudermann was only the second professor to teach this subject at German universities, the first being Jacobi himself. For whatever reason, according to Mittag-Leffler (1923a, 18), after the very first lecture the audience reduced to Weierstrass alone. Gudermann then decided to lecture on "analytical spherics" (i.e. the goniometry of circular and hyperbolic functions) *privatissime* for this single student.

Some ten years earlier Gudermann had published a book on spherical geometry, his *Grundriss der analytischen Sphärik* (1830). He then made the circular and

hyperbolic functions the subject of a series of papers that appeared in Crelle's *Journal* in 1830 and 1831, which he collected into a book in 1833 (*Theorie der Potenzial- oder cyklisch-hyperbolischen Functionen*, Berlin 1833). Both circular and hyperbolic functions were defined by Gudermann in terms of a general power function u^x and its reciprocal u^{-x} "which was then expanded into a power series to obtain the fundamental formulas in this trigonometry" (Manning 1974–1975, 315).

Gudermann showed no particular concern for the convergence of the series he was dealing with in these papers, which might be due to the fact that most of them are convergent everywhere. Many of his results on the expansion of hyperbolic functions were already known to Lambert, L'Huilier and Cauchy, not to mention Gauss, and had their counterpart in Euler's expansion of the circular functions published in his *Introductio* of 1748. However, as Manning has pointed out (1974–1975, 317), "Gudermann's work on series formed a directed line of inquiry and compilation of mathematical results that give a new meaning to the use of series in a time when the theory of complex variable had hardly been developed". Moreover, he made extensive use of transformations of the infinite series and products that he found to enable the computation of tables of values for the functions.

By the time Weierstrass arrived in Münster, Gudermann's interests had shifted firmly to the theory of elliptic functions ("modular functions", as he called them). He published a series of papers on them in Crelle's *Journal* in the years 1838 to 1843 under the common title *Theorie der Modular-Functionen und der Modular-Integrale*. As a friend of Jacobi, Gudermann expounded the theory of elliptic function following the *Fundamenta nova*. He began, as Jacobi had done, by considering the elliptic integral of the first kind

$$u = \int_0^t \frac{dt}{\sqrt{(1-t^2)(1-k^2t^2)}}, \quad (6.1)$$

where k is the modulus. Gudermann first analysed the particular cases $k = 0$ and $k = 1$ where the integral degenerates and defines the circular functions. This allowed him to point out clearly the analogy between the trigonometric and elliptic functions. He then turned to the standard case $0 < k < 1$. He introduced the notation snu , cnu , and dnu for Jacobi's $\sin am\ u$, $\cos am\ u$, and $\Delta am\ u$, respectively, and following the analogy with the circular and hyperbolic functions established such relations for elliptic functions as

$$\operatorname{sn}^2 u + \operatorname{cn}^2 u = 1, \quad (6.2)$$

and the like. Then he established the formulae for differentiation:

$$\frac{d\operatorname{snu}}{du} = \operatorname{cnu}\operatorname{dnu}; \quad \frac{d\operatorname{cnu}}{du} = -\operatorname{snu}\operatorname{dnu}; \quad \frac{d\operatorname{dnu}}{du} = -k^2\operatorname{snu}\operatorname{cnu}, \quad (6.3)$$

and, by solving a system of differential equations, he obtained the addition (and subtraction) formulae for snu , cnu , and dnu . Next he established their periodicity,

writing “the cyclic modular [i.e. elliptic] functions are doubly periodic functions—real and imaginary at the very same time” (Gudermann 1838, 32).

Gudermann then tackled the problem of expanding the elliptic function in series. He relied on repeated differentiation, which, as Manning has remarked, is “more difficult than in the cases of circular and hyperbolic functions, since the change is induced on three functions rather than two”, as formulae (6.2) and (6.3) show. According to Manning “this fact perhaps discouraged Gudermann from a discussion of the Taylor series of elliptic functions in the early part of his work”,¹ and indeed, Gudermann postponed his treatment of the Taylor series expansion of snu , cnu , and dnu until he had established the additions theorems for these functions. A more convincing explanation, however, seems to be that Gudermann now had at hand a system of differential equations which he could use to obtain the sought-for expansions with a method which was very much in the spirit of the German “combinatorial school” in which he had been trained.

Beginning with snu Gudermann took up the expansion

$$\operatorname{snu} = u + bu^3 + cu^5 + \cdots \quad (6.4)$$

and observed that snu satisfies the differential equation

$$\frac{d^2 \operatorname{snu}}{du^2} = -(1+k^2)\operatorname{snu} + 2k^2 \operatorname{sn}^3 u. \quad (6.5)$$

This provided him with a recursive method for obtaining the coefficients. As Manning summarised it, “he assumed that the differential equation had a power series solution and consequently assumed a canonical form for that series. From these assumptions he arrived at recurrence formulas to determine the unknown coefficients” (1974–1975, 351). Gudermann obtained the expansions of cnu and dnu similarly.

6.1.2 Weierstrass’s First Essay

Gudermann’s technique was adopted by Weierstrass in his very first paper (Weierstrass 1840), an essay on elliptic functions that he wrote in the summer of 1840 and submitted in the autumn so that he could obtain the *venia docendi* (i.e. the permission to teach). His starting point was a remark made by Abel in a letter to Legendre,² which Weierstrass had read in his student days in Bonn. Abel had also mentioned the same question in his *Précis* (see Sect. 1.3), where he said that the elliptic function $\lambda(u)$ (snu in the symbolism Weierstrass took from Gudermann)

¹For this and the previous quote, see Manning (1974–1975, 347).

²Published in Crelle’s *Journal* 6, (1830), 76.

defined by (6.1) could be expressed as the ratio of two power series in u whose coefficients are entire functions of the modulus k .

Abel had claimed (without proof) that

$$\lambda(u) = \frac{u + A_1 u^3 + A_2 u^5 + \cdots}{1 + B_2 u^4 + B_3 u^6 + \cdots}, \quad (6.6)$$

where the numerator and the denominator are series that converge for all (real or complex) values of u and k . Weierstrass's aim was to prove this claim by providing the required expansions of this and the other elliptic functions, and also to show how the formulae could be extended to complex values. According to what Weierstrass wrote to Lie on April 10th, 1882, the first serious mathematical problem that he had tackled when he was in Bonn was that of obtaining the function $\lambda(u)$ directly from the differential equation that defines it. His success in solving this problem in the seventh semester of his study in Bonn played a decisive role in his decision to “devote himself completely to mathematics”.³ Apparently Abel exerted a decisive, lasting influence on Weierstrass. According to Mittag-Leffler (1923a, 25), “Read Abel” was the first and last suggestion Weierstrass used to give his students in the early years of his teaching in Berlin.

As had Gudermann, Weierstrass took the differential equations (6.3) as the definition of the elliptic functions snu , cnu , and dnu , which can also be expressed in series

$$\operatorname{snu} = u + a_1 u^3 + \cdots + a_r u^{2r+1} + \cdots \quad (6.7)$$

$$\operatorname{cnu} = 1 + b_1 u^2 + \cdots + b_r u^{2r} + \cdots \quad (6.8)$$

$$\operatorname{dnu} = 1 + c_1 u^2 + \cdots + c_r u^{2r} + \cdots, \quad (6.9)$$

where the coefficients $a_r, b_r, c_r, \dots$ are entire functions of the modulus k^2 .

Weierstrass considered the following four (developments of) functions:

$$\phi(u) = -\log u + \frac{u^2}{2} + \sum_{r=1} a_r \frac{u^{2r+2}}{(2r+1)(2r+2)} \quad (6.10)$$

$$\psi(u) = \frac{u^2}{2} + \sum_{r=1} b_r \frac{u^{2r+2}}{(2r+1)(2r+2)} \quad (6.11)$$

$$\chi(u) = \frac{k^2 u^2}{2} + \sum_{r=1} c_r \frac{u^{2r+2}}{(2r+1)(2r+2)} \quad (6.12)$$

$$\theta(u) = \frac{k^2 u^4}{3.4} + \sum_{r=2} d_r \frac{u^{2r+2}}{(2r+1)(2r+2)} \quad (6.13)$$

³In (Abel 1902, 108).

and showed that they are related to Gudermann's elliptic functions as follows:

$$\frac{d^2\phi(u)}{du^2} = \frac{1}{\operatorname{sn}u^2}; \quad (6.14)$$

$$\frac{d^2\psi(u)}{du^2} = \frac{\operatorname{dn}u^2}{\operatorname{cn}u^2}; \quad (6.15)$$

$$\frac{d^2\chi(u)}{du^2} = \frac{k^2\operatorname{cn}u^2}{\operatorname{dn}u^2}; \quad (6.16)$$

$$\frac{d^2\vartheta(u)}{du^2} = k^2\operatorname{sn}u^2 \quad (6.17)$$

Then Weierstrass introduced four new functions $Al(u)$ (named in honour of Abel) that were defined as follows:

$$Al_1(u) = e^{-\phi(u)}, \quad Al_2(u) = e^{-\psi(u)}, \quad Al_3(u) = e^{-\chi(u)}, \quad Al(u) = e^{-\vartheta(u)}, \quad (6.18)$$

which enabled him to represent the elliptic functions $\operatorname{sn}u$, $\operatorname{cn}u$, $\operatorname{dn}u$ as quotients

$$\operatorname{sn}u = Al_1(u)/Al(u); \quad \operatorname{cn}u = Al_2(u)/Al(u); \quad \operatorname{dn}u = Al_3(u)/Al(u), \quad (6.19)$$

as required. In fact, as Klein (1926–1927, 1, 280) remarked, the Al functions differ from the corresponding four Jacobian theta functions by a factor of the form $ce^{\lambda u^2}$. Klein also remarked that the Al functions were in fact a step towards the σ -functions that Weierstrass was to introduce some twenty years later. The corresponding σ and the σ_1 , σ_2 , and σ_3 functions are symmetric with respect to the periods. In addition, σ is invariant under linear transformations of the periods.

Weierstrass next wrote the series (6.10) in a canonical form

$$Al_1(u) = u + \sum_{r=1} A_r u^{2r+1}; \quad (6.20)$$

$$Al_2(u) = 1 + \sum_{r=1} B_r u^{2r+1}; \quad (6.21)$$

$$Al_3(u) = 1 + \sum_{r=1} C_r u^{2r+1}; \quad (6.22)$$

$$Al(u) = 1 + \sum_{r=2} D_r u^{2r}. \quad (6.23)$$

In order to determine the coefficients in the above series he showed that under certain conditions, each of them is a solution of the second order linear partial differential equation

$$\frac{\partial^2 \phi}{\partial u^2} + 2k^2 u \frac{\partial \phi}{\partial u} + 2k(1-k^2) \frac{\partial \phi}{\partial k} + k^2 u^2 \phi = 0. \quad (6.24)$$

Following Gudermann's recurrence method, he used this equation to derive a recursion formula for the coefficients of the series (6.20). He proved that the A/k functions are given by convergent series whose coefficients are entire functions of k . Eventually, he developed the A/k s in convergent Fourier series. This completed Weierstrass's treatment of the proposed question.

Gudermann judged this paper very highly and did not hesitate to state that "the candidate enters hereby as an equal in the ranks of celebrated discoverers" (in Lorey 1915, 601; Mittag-Leffler 1923a, 21). However, for some reason which Weierstrass (1894, 50) "could not go into closely", the paper was not published. One can only speculate about the relationship between the teacher and his most gifted student, but Weierstrass came to know his teacher's judgement only in 1853, and—he wrote to Schwarz on July 4th, 1884—he regretted that ignorance of Gudermann's statement had perhaps prevented him seeking to have his paper published and so to embark upon an academic career at that time. In fact, Weierstrass began to publish his work in appropriate journals only after Gudermann's death in 1852. An excerpt of this paper was published in Weierstrass (1856b, 346–379), but the full paper appeared in print only 54 years later, in vol. I of Weierstrass's *Mathematische Werke* (see (Weierstrass 1840)).

6.2 Weierstrass's Early Papers

Having passed the examination, Weierstrass spent the following year as a *Probekandidat* at the Gymnasium Paulinum in Münster. Here he wrote his second paper (Weierstrass 1841a), which was also only published for the first time in his *Werke*. As Mittag-Leffler (1923a, 27) observed, in this paper

one can find the first known rigorous proof of Cauchy's theorem on the integration between two given complex limits without any application of double integrals or surface integrals; this is given in such a way that one obtains Laurent's theorem at the same time, which became known only two years later through a note by Cauchy.

This is all the more remarkable, Mittag-Leffler added, because "Weierstrass first became acquainted with Cauchy's works in 1842".

Even more remarkable is the fact that Weierstrass made an essential use of integrals in his proof. Apparently Weierstrass avoided mentioning the existence of this paper to his colleagues until the publication of the first volume of his *Werke*. Thus Pringsheim (1896, 123) commented: "In his fundamental work *Zur Theorie der eindeutigen analytischen Functionen* and, as far as I could determine, even in his lectures, Herr Weierstrass neither explicitly proved nor directly used Laurent's theorem". In this respect, Mittag-Leffler (1923a, 35) observed that "In Weierstrass's function theory the theorem of Laurent does not have an elementary place and does not play the fundamental role that it does in Cauchy's".

6.2.1 The Proof of Laurent's Theorem

The aim of Weierstrass's paper is to prove the following statement: let $F(x)$ be a single-valued, finite, and continuous function of a complex variable x in the annulus $A < |x| < B$ and let it also satisfy the further, essential condition that for any "infinitely small" k , any x in the annulus, and any (bounded) h , the difference between

$$\frac{F(x+k) - F(x)}{k} - \frac{F(x+hk) - F(x)}{hk} \quad (6.25)$$

is "infinitely small", i.e. the difference (6.25) tends to 0 uniformly with respect to both h and x , then $F(x)$ can be represented in an absolutely convergent series $\sum_{n=-\infty}^{+\infty} A_n x^n$ whose coefficients A_n are independent of x .

Weierstrass began by proving that the integral $J = \int_C x^n dx$, where n is an integer and C is a circle of radius r , with $A < r < B$, equals zero for $n \neq -1$. To this end he remarked that such a circle can be parameterised by⁴

$$r \frac{1 + i\lambda}{\lambda + i}, \quad -\infty < \lambda < +\infty.$$

This turns an integral along a circle in the complex plane into an improper integral along the real axis, for, putting $x = rw$ one has

$$J = \int_C x^n dx = \int_{-\infty}^{+\infty} (rw)^n r \frac{dw}{d\lambda} d\lambda.$$

It is an easy calculation to show that $J = 0$, for $n \neq -1$ and $J = 2\pi i$ when $n = -1$. Indeed, one has

$$\int_{-\infty}^{+\infty} \frac{dw}{w} = 2i \int_{-\infty}^{+\infty} \frac{d\lambda}{1 + \lambda^2} = 4i \int_0^{+\infty} \frac{d\lambda}{1 + \lambda^2} \quad (6.26)$$

but $\int_0^{+\infty} \frac{d\lambda}{1 + \lambda^2}$ is "well known to" equal $\pi/2$. Weierstrass remarked, however, that all he needed for convergence is to prove that this integral has a finite value.

Then he considered the integral $\int_{-\infty}^{+\infty} \frac{F(x_0 w)}{w} dw$ where $|x_0| = r$ and, by making a decisive use of condition (6.25), Weierstrass (1841a, 57) showed that its value "is the same for all values of x_0 whose modulus is contained between the limits A, B ". This result is sometimes referred to as Weierstrass's mean value theorem.

⁴This is the point where the straight line passing through $(-r, 0)$ with slope λ intersects the circle centred at the origin and with radius r .

As [Remmert \(1991, 351\)](#) has written, “the independence of the integral from the radius r is the heart of Weierstrass's work”. In fact, by transforming an integral along a circle in the complex plane into an improper real integral and resorting to mean values instead of integrals he was able to prove the Cauchy integral theorem for an annulus.⁵

Assuming that $F(x)$ can be expanded in an absolutely convergent Laurent series $\sum_{n=-\infty}^{+\infty} A_n x^n$, on the basis of his previous results Weierstrass determined the coefficients A_n and A_{-n} as

$$A_n = \frac{1}{2\pi i} \int_{-\infty}^{+\infty} \frac{F(x_0 w)}{w} (x_0 w)^{-n} dw, \quad A_{-n} = \frac{1}{2\pi i} \int_{-\infty}^{+\infty} \frac{F(x_0 w)}{w} (x_0 w)^n dw, \quad (6.27)$$

the integral being independent of x within the annulus, because if $F(x)$ satisfies the required conditions so does $x^n F(x)$.⁶ Weierstrass then proved that the series is unconditionally (i.e., absolutely) convergent, and that it actually converges to $F(x)$. In order to do so he took an indirect and troublesome route by introducing a new variable $\varepsilon < 1$ and resorting to the method of Abel summation⁷ to prove that for ε tending monotonically upwards to 1

$$\lim_{\varepsilon \uparrow 1} \sum_{n=1}^{\infty} A_n \varepsilon^n x^n = \sum_{n=1}^{\infty} A_n x^n$$

and

$$\lim_{\varepsilon \uparrow 1} \sum_{n=1}^{\infty} A_{-n} \varepsilon^n x^{-n} = \sum_{n=1}^{\infty} A_{-n} x^{-n},$$

where the coefficients A_n and A_{-n} are given by (6.27). In the next paragraph Weierstrass proved the so-called Riemann removable singularity theorem by using Cauchy's estimates for the coefficients⁸ and eventually (1841a, 64–66) he established the formulae for the derivatives of $F(x)$.

Apparently Weierstrass never mentioned either this result or his “mean value theorem” in his Berlin lectures. Commenting on this [Pringsheim \(1896, 123\)](#)

⁵It is worth noting here that, without mentioning it, Weierstrass resorted to a mean value argument just as ([Cauchy 1840c](#)) had done for proving his integral representation theorem on the expansion of functions in power series.

⁶Here Weierstrass interchanged integration and summation without establishing the necessary uniform convergence of the series; see the discussion below.

⁷Abel summation is the method that deduces the convergence of $\sum_k a_k b_k$ from the facts that $|\sum_k a_k|$ is bounded as $n \rightarrow \infty$ and (b_k) is a sequence of real numbers tending monotonically to 0. It can be traced back to Theorem IV in [Abel \(1826b\)](#): If the series $f(x) = \sum_k v_k x^k$ converges for $x = \delta$, then it converges for all $x < \delta$ [in absolute value] and if β continually decreases to 0 the limiting value of $f(\alpha - \beta)$ is $f(\alpha)$ for all $x < \alpha$ [in absolute value].

⁸For a historical account of this theorem, see ([Ullrich 1994](#)).

observed that Weierstrass “neither explicitly proved nor directly applied” Laurent’s theorem, neither in his fundamental paper (Weierstrass 1876b) nor in his lectures. After lamenting that this theorem had not secured the place it deserves in elementary function theory—i.e. Weierstrass’s theory based on power series without any use of integrals—he acutely observed: “it seems to me that elementary function theory without Laurent’s theorem possesses no means whatsoever of inferring the regular behaviour of $f(x)$ at $x = a$, e.g. the existence of a power series $P(x|a)$ that represents $f(x)$ and is convergent in a certain neighbourhood of a ” (Pringsheim 1896, 123).

The fact that this paper became known to the mathematical community only after its publication in 1894 in Weierstrass’s *Werke* raises an interesting question: Why did Weierstrass keep this paper “hidden”, and avoid mentioning it in his later lectures at the Berlin university? This is all the more intriguing, for this paper provided a proof that a complex differentiable function can be locally expanded in a power series, a theorem that—as Pringsheim suggested and Ullrich (1989, 152–159) has convincingly shown—cannot be found anywhere in his lectures on function theory.

Admittedly, Weierstrass was notoriously reluctant to resort to “transcendental methods”. As he explained more than forty years later, in a talk delivered in 1884 at the Mathematical Seminar in Berlin,

However great the importance of the notion of the integral for all of analysis, I nevertheless wish to found the theory of functions solely with the help of the elementary theorems about the basic operations. I do not say that one should give, or can give such a direct proof in each case; I leave this question undecided. But I try to give direct proofs as far as possible, and I want to use this method particularly with the foundation of the theory of functions.⁹

It is worth mentioning here that an elementary proof of Laurent’s theorem was provided by Mittag-Leffler (1884b) that very same year. Even though his “new” proof of Laurent’s theorem was “elementary” in the sense that it did not use Cauchy’s integral theorem, however, as (Pringsheim, 1896, 124) commented acutely, it was really entangled in nature, and “it provides the reader with ever greater difficulties than the usual Cauchy–Laurent proof” to the effect that in this case “one could not incorrectly say that the consequence of the method seems to be that one must pay all too high a cost in simplicity”. Pringsheim was similarly critical of the proof published by Scheeffer (1884) in the same volume of the *Acta*, which was based on the mapping of an annulus onto a simply connected surface. Then he referred to a proof he himself had recently produced in (1895b). This proof was inspired by Cauchy (1840c) and was based on the mean values of a function “which in the end—commented Mittag-Leffler (1923a, 35) in turn—is the same as the application of Cauchy’s integral theorem”.

⁹Weierstrass’s talk was posthumously published by Mittag-Leffler in 1925, see (Weierstrass 1925, 8).

6.2.2 Theorems on Power Series

Also in 1841 Weierstrass wrote another paper (1841b), where he stated and proved three theorems related to power series expansions (in his own terminology this term included both “ordinary” power series and Laurent expansions). Theorems (A) and (B) evaluate the coefficients of a Laurent series in one and several complex variables, respectively, without any use of integrals. In fact, Weierstrass obtained the Taylor series inequalities that Cauchy had presented in his Turin 1831 *Mémoire* (see Sect. 3.4.4) on the “calculus of limits” independently of Cauchy. Weierstrass’s theorem (C) was the double series theorem nowadays named after him.

More precisely, Weierstrass’s theorem (A) stated: Let $F(x) = \sum_{v=-\infty}^{v=+\infty} A_v x^v$ be a power series in the complex variable x with given coefficients and r any determinate, positive magnitude which lies within the domain of convergence of the series. Let $\sup |F(x)| = g$ for $|x| = r$. Then, one has $|A_\mu| \leq g r^{-\mu}$ for every integer value of μ . (Weierstrass 1841b, 67).

Weierstrass made an essential use of his assumption that the series was uniformly convergent for $|x| = r$. Weierstrass had learnt about the uniform convergence of series from Gudermann, who apparently was the first mathematician to express this concept in a definite way. A propos some series for the elliptic functions he wrote “It is a fact worth noting that ... the series just found have all the same convergence rate” (Gudermann 1838, 251–252). Weierstrass’s words in his (1841b, 68–69) were “Because the power series under consideration ... converges uniformly, given an arbitrary positive quantity δ , a finite number of terms of the series can be discarded so that the sum of all the remaining terms is, for every value in the specified domain ... in absolute value $< \delta$ ”.¹⁰

Theorem (B) extends this result to functions $F(x_1, \dots, x_\rho)$ of several complex variables. It says:

Let $F(x_1, \dots, x_\rho) = \sum_v A_{v_1, v_2, \dots, v_\rho} x_1^{v_1} x_2^{v_2} \dots x_\rho^{v_\rho}$ be a convergent series, where $x_1, x_2, \dots, x_\rho$ are complex variables, $A_{v_1, v_2, \dots, v_\rho}$ are given constants and $v_i \in \mathbb{Z}$, and let $r_i (1 \leq i \leq \rho)$ be a system of positive quantities such that the point $(x_1, x_2, \dots, x_\rho) = (r_1, r_2, \dots, r_\rho)$ belongs to the domain of convergence. In addition, let $\sup |F(x_1, \dots, x_\rho)| = g$ for $|x_i| = r_i$ for any i . Then

$$|A_{v_1, v_2, \dots, v_\rho}| \leq g r_1^{-\mu_1} r_2^{-\mu_2} \dots r_\rho^{-\mu_\rho}$$

for every system of integer values of μ_i .

The proof proceeded along the same lines as the one variable case, an essential role being played by the uniform convergence of the series for values x_i such that $|x_i| = r_i$ for every i .

Finally, Weierstrass established theorem (C):

¹⁰Uniform convergence was only slowly coming in in the 1840s, see Bottazzini (1986, Sect. 5.4).

Let $F_\mu(x_1, \dots, x_\rho)$ be an infinite sequence of functions for integers μ defined by “ordinary” power series $F_\mu(x_1, \dots, x_\rho) = \sum_v A_{v_1, v_2, \dots, v_\rho}^{(\mu)} x_1^{v_1} x_2^{v_2} \dots x_\rho^{v_\rho}$. Let each of the functions (and their sum) be “unconditionally [i.e., absolutely] and uniformly” convergent in a determinate neighbourhood G of the origin $(0, 0, \dots, 0)$. Then

$$\sum_{\mu=0}^{+\infty} F_\mu(x_1, \dots, x_\rho) = \sum_v A_{v_1, v_2, \dots, v_\rho} x_1^{v_1} x_2^{v_2} \dots x_\rho^{v_\rho},$$

where $\sum_{(\mu)} A_{v_1, v_2, \dots, v_\rho}^{(\mu)} = A_{v_1, v_2, \dots, v_\rho} < +\infty$ (Weierstrass 1841b, 70–71).

In other words, under the given hypotheses about convergence he proved that the sum of the series of functions F_μ can be expressed by a power series in $x_1, \dots, x_\rho$ whose coefficients are given by the sum of the corresponding coefficients of the power series that represent the functions F_μ .

He proved this theorem by using Cauchy’s inequalities for Taylor coefficients, which he had already obtained in theorem (B). A comparison with his previous paper on Laurent expansions shows that in the meantime Weierstrass had obtained a clearer understanding of the phenomenon of the uniform convergence of series, as far as the interchanging of two infinite series was concerned. (That said, besides uniform convergence Weierstrass stipulated the additional hypothesis that the series converges absolutely, which is not necessary. When he reformulated this theorem in (1880c) Weierstrass no longer required the absolute convergence of the series.)

As Remmert (1991, 251) has remarked, “for Weierstrass the double series theorem was the key to convergence theory”. In particular, from this theorem Weierstrass obtained the theorem that the derivative of a uniformly convergent series representing an analytic function is obtained by differentiating the series term by term. To establish this he developed each F_μ in its Taylor series around an arbitrary point $a_1, \dots, a_\rho$ of G and observed that, as a consequence of theorem (C), for fixed $a_1, \dots, a_\rho$ these series converge in some fixed disk centred at $a_1, \dots, a_\rho$ and lying in G . Thus the series

$$\sum_{\mu=0}^{+\infty} F_\mu(x_1, \dots, x_\rho)$$

represents an analytic function there and has derivatives of any order which are given by term-by-term differentiation:

$$\frac{\partial}{\partial x_\lambda} \sum_{\mu=0}^{+\infty} F_\mu(x_1, \dots, x_\rho) = \sum_{\mu=0}^{+\infty} \frac{\partial}{\partial x_\lambda} F_\mu(x_1, \dots, x_\rho), \quad \lambda = 1, \dots, \rho \quad (6.28)$$

As with his previous papers, even this one remained unpublished until 1894. However, Weierstrass used to expound his theorem (A), and sometimes also theorems (B) and (C), in his Berlin lectures on the introduction to analytic function theory (see Sect. 6.5) and in this way they were published informally.¹¹

¹¹Various versions may be consulted, see, e.g., Weierstrass (1968, 66–68), Weierstrass (1874, 278–294), Weierstrass (1988a, 99–100, 122–123) and Pincherle (1880, 327–342).

Weierstrass's papers (1841a, 1841b) suggest a natural question: Why did he avoid integrals when looking for an estimate of the coefficients of the Laurent series whereas he used them in getting the Laurent expansion? Theorem (B) suggests a tentative answer. Weierstrass could indeed have proved theorem (A)—the one variable case—by using an integral taken over the boundary of a disk in $\mathbb{C}$. But in the several variables case he would have encountered serious difficulties in following the same route. How is one to deal with an integral of a function of several complex variables taken over the boundary of a polydisk in $\mathbb{C}^p$? And passing from one to several variables was not a generalisation for generalisation's sake: the study of Abelian integrals naturally leads to functions of several complex variables. As this was Weierstrass's main aim from the very beginning of his mathematical career, he surely realised very soon that the method used in his paper (1841a) would have been ineffective. On the other hand, the method of power series seemed to apply to one, two, and several complex variables equally well. For this reason, Weierstrass seems to have decided as early as 1841 to give up integrals (or “transcendental” methods, as he later called them) once and for all in favour of power series. In this respect, this paper marked a turning point in Weierstrass's use of analytic methods.

6.2.3 Functions Defined by Differential Equations

His early work was completed by the paper that Weierstrass wrote in the spring of 1842. There he began by proving that the system of n differential equations

$$\frac{dx_i}{dt} = G_i(x_1, \dots, x_n), \quad i = 1, \dots, n, \quad (6.29)$$

where the $G_i(x_1, \dots, x_n)$ are given entire functions, could be solved by determining n power series

$$x_i = P_i(t), \quad i = 1, \dots, n, \quad (6.30)$$

which are convergent in a neighbourhood of the point $t = 0$ where they take arbitrarily prescribed values $a_1, \dots, a_n$. “From the determination of the series” $P_i(t)$, Weierstrass (1842, 80) remarked, one deduces the uniqueness of the solutions, or in his words, that “there is only one such system of power series”.

As Weierstrass himself recognised in 1894 (in *Math. Werke* 1, 85), when he wrote the paper he had “no knowledge” of the fact that this theorem had already been found (and published) by Cauchy in a series of papers he presented to the French Académie in July 1842 in the context of the application of his *calcul des limites* to differential equations (see (Cauchy 1842a, b, c)).¹²

His own paper, however, Weierstrass continued, included “important theorems that cannot be found in Cauchy”. Indeed, in Sect. 2 he proved that, under the condi-

¹²“It is also not unlikely”—Mittag-Leffler (1923a, 38) remarked—that Weierstrass, who became acquainted with Cauchy in 1842, has been influenced by the *calcul des limites*”.

tion that both the coefficients of the functions $G_i(x_1, \dots, x_n)$ and the $a_1, \dots, a_n$ are single-valued analytic functions of arbitrarily many variables $u_1, u_2, \dots$ belonging to a connected domain where they are all less than some finite bound, the series $x_i = P_i(t)$ ($i = 1, \dots, n$) are unconditionally and uniformly convergent, and form a system of single-valued analytic functions of the variables $t, u_1, u_2, \dots$.

Mittag-Leffler (1923a, 39) aptly remarked that a thoughtful study of such a system $x_i = P_i(t)$ ($i = 1, \dots, n$) had been first tackled by Poincaré in his prize-winning memoir (1890a) on the three-body problem, and then by Painlevé (1897) in his Stockholm lectures on the analytic theory of differential equations.

Then, in Sect. 3 of his paper Weierstrass introduced for the first time the concept of “analytical continuation”, and consequently the concept of “analytical function”. He considered the system of power series

$$x_i = P_i(t - t_0, a_1, \dots, a_n), \quad i = 1, \dots, n; \quad t_0, a_1, \dots, a_n \text{ fixed,}$$

that satisfy the given differential equations, assume arbitrarily prescribed values $a_1, \dots, a_n$ for $t = t_0$, and which are all convergent in a domain T_0 with $t, t_0 \in T_0$.

Given any $t_1 \in T_0, t_1 \neq t_0$, the series can be rewritten as

$$P_i(t - t_0, a_1, \dots, a_n) = P_i(t - t_1, a'_1, \dots, a'_n) \quad i = 1, \dots, n,$$

where $a'_i = P_i(t_1 - t_0, a_1, \dots, a_n), i = 1, \dots, n$.

Their convergence disk T_1 can lie outside T_0 . In this case T_1 splits into two connected parts, and the series coincide for $t \in T_0 \cap T_1$ while outside T_0 the series $P_i(t - t_1, a'_1, \dots, a'_n); i = 1, \dots, n$ are a “continuation” of $P_i(t - t_0, a_1, \dots, a_n); i = 1, \dots, n$.

Remarkably enough, Weierstrass also observed that it might be impossible for (some of) the $P_i(t - t_0, a_1, \dots, a_n)$ to be continued outside T_0 or, in later terms, the possibility that some of the given series might have the boundary of T_0 as their natural boundary. By repeating this reasoning one obtains by analytical continuation a sequence of series that taken together give the definition of a (single-valued or many-valued) system of analytic functions of the variable t in the most general terms.

According to Mittag-Leffler (1923a, 41), there is hardly any doubt that by this time Weierstrass had the general concept of an analytic function, and apparently this was confirmed by Weierstrass himself. Mittag-Leffler added that in later years Weierstrass used to expound the content of his 1842 paper at the mathematical Seminar in Berlin when adding a couple of related theorems to it. In Mittag-Leffler’s view they had been found by Weierstrass at the same time and in the same context as his (1842), and therefore could be considered as Sects. 4 and 5 of it. Of particular interest is the following: “If, when moving from the interior of the convergence disk of the integral (6.30) one approaches a singular point on the boundary of the disk, then $\sup |P_i(t, a_1, \dots, a_n)|$ increases beyond any limit”. Mittag-Leffler added that this theorem was also stated and proved in a letter he had received from Weierstrass on 7 August 1885 (quoted in (Mittag-Leffler 1912, 41)). In that letter Weierstrass

discussed an important application of this to the n -body problem (in [Mittag-Leffler 1912](#), 44–45), and then he referred to it when formulating the question for King Oscar's Prize.

Therefore, it is not surprising that the importance of this paper in Weierstrass's work was also emphasised by [Poincaré \(1899b\)](#), 9–10) when he said that in order to represent

all the functions by means of (power) series and to be able to use this representation without fear in all questions of the integral calculus, it is necessary to show that one can equate to a power series every implicit function drawn from a system of equations whose first terms are power series or the integral of a differential equation whose coefficients are power series. This important theorem became for Weierstrass one of the foundation stones of his systems.

Thus, working in complete isolation Weierstrass established the foundations of the theory of analytic functions in these three papers, which he kept unpublished and later occasionally used in his lectures in Berlin.

6.2.4 Analytische Fakultäten

In autumn 1842 Weierstrass obtained a position as teacher at the Katholisches Progymnasium in Deutsch-Krone in West Prussia. He had to teach the junior classes, and, in addition to mathematics and physics, his teaching included such matters as botany, geography, history, and even calligraphy and physical exercise. In spite of this, he was able to continue his mathematical research and in the scientific appendix to the annual report of the Progymnasium for the year 1842/43 he published the paper “Bemerkungen über die analytische Fakultäten” (in *Math. Werke* 1, 87–103). This was his first printed paper, and in fact part of a more extended one that Weierstrass completed in 1854, and published two years later in Crelle's *Journal* ([Weierstrass 1856a](#)).

In the early decades of the nineteenth century the theory of the so-called analytical powers (*analytische Fakultäten*) was a very popular subject in Germany, cultivated by a number of second rank, but very influential and academically powerful mathematicians such as Kramp, Crelle, and Martin Ohm.

Indeed, in introducing Weierstrass's paper Crelle was pleased to state in a footnote that “the very important theory of analytical powers” attracted the attention of the analysts more and more, as was now the case of “such an outstanding and acute mathematician as Herr Weierstrass”. As for the latter's criticism of the standard approach to the theory, including his own, Crelle remarked he had “something to say” but he was forced to give it up because of his poor health and lack of strength, being content that, if not he himself, someone else could get “closer to the truth”.¹³

¹³In ([Weierstrass 1856a](#), 1). Crelle's footnote is not included in the version published in Weierstrass's *Math. Werke* (1, 153–221).

According to Mittag-Leffler (1923a, 45), what was needed was “first of all to create a true theory of functions from scratch, and it was only this aim that led Weierstrass to the goal”. He went on by claiming that the essential step was taken by Weierstrass in 1854 with a sharp distinction between “analytic expression” and “analytic function”. It is difficult to evaluate the correctness of Mittag-Leffler’s claim, for this distinction is only implicit in (1856a, 4) as it was published in Crelle’s *Journal*, and Weierstrass reworked the relevant passage when re-publishing the paper in (1886, 183–260). (This version of the paper was eventually included in his *Math. Werke*.)

There Weierstrass began by considering the function

$$f(u, x, y) = \prod_{v=0}^{y-1} (u + vx),$$

where u and x are (real) variables and y a positive integer. The function $f(u, x, y)$ satisfies functional equations such as

1. $f(u, x, y + y') = f(u, x, y)f(u + yx, x, y')$
2. $f(u, x, 1) = u$
3. $f(ku, kx, y) = k^y f(u, x, y)$ (k constant)

that suggest an analogy with the properties of the power u^y to which $f(u, x, y)$ reduces for $x = 0$. Thus, Kramp and other mathematicians, including Crelle, were led to think that u^y always had a meaning for arbitrary values of y . They called this hypothetical function an *analytische Fakultät* and denoted it by $u^{x|y}$.

Bessel tried to avoid the contradictions in which Kramp’s theory was involved by adding to the functional equations above the further condition

$$\lim_{u \rightarrow +\infty} \frac{u^{x|y}}{u^y} = 1.$$

Commenting on Bessel’s definition, Weierstrass observed that it implied that the same *Fakultät* $u^{x|y}$ was given by the two analytical expressions:

$$u^{x|y} = \lim_{n \rightarrow +\infty} (nx)^y \prod_{v=0}^{y-1} \frac{u + vx}{u + yx + vx} \quad (x > 0), \text{ and}$$

$$u^{x|y} = \lim_{n \rightarrow +\infty} (-nx)^y \prod_{v=0}^{y-1} \frac{u + yx - vx}{u - vx} \quad (x < 0).$$

In 1856 Weierstrass limited himself to remarking that this showed that it was impossible to offer a representation of the same $u^{x|y}$ that remained unchanged for all the values of u, x, y . Then he raised the question about how to extend the given definition to complex values of the variables and stated that “the true concept of an analytic function must be such that it extends uniformly for *all* the values of

the argument” [his emphasis] (1856a, 4). In 1886 he clarified this point by stating that for every (real or complex) value of u, x, y each of Bessel’s expressions for $u^{x|y}$ defines an analytic function, but in fact they are two different functions according to whether x is positive or negative. “It looks e.g. as though one would define $\log x$ in the usual way for positive values of x , and $\log x = \log(-x)$ for negative values of x ”, Weierstrass (1856a, 155) explained, and in a footnote he pointed out that according to the wording he himself had introduced in (1880c) Bessel’s *Fakultät* $u^{x|y}$ “is not a monogenic function”. Instead, the expressions for $x > 0$, and resp. $x < 0$, “are branches of two different analytic functions”.

As a matter of fact, this distinction appeared in print for the first time only in Weierstrass (1880c) (see Sect. 6.9). It took a long time until it was accepted; Mittag-Leffler (1923a, 46) commented that “Hermite never understood it”, and “as important person as Christoffel explained to me with great excitement his definitive break with Weierstrass’s point of view some years after the publication of Weierstrass’s paper (1880c)”. (Perhaps more significantly, Mittag-Leffler omitted to remind his readers that Weierstrass had openly criticised Riemann’s point of view there.)

When he reprinted the paper (1856a) in his *Werke* Weierstrass admitted that “in my eyes the theory of analytical powers [*Fakultäten*] no longer has the importance which mathematicians used to attribute to it in earlier times” (1856a, 158). Nevertheless, he decided to reprint the paper because “even today” it contained “some things” which could be of “some use” to mathematicians. Actually, as we have mentioned, Weierstrass should have attributed more importance to it than he claimed, for he had previously let that paper be reprinted in his (1886). Indeed, of particular interest is a result that he proved in Sect. 1 where, starting from Crelle’s functional equations listed above as defining an *analytische Fakultät* he came across the relation $F(u+1) = uF(u)$. He remarked that Legendre’s $\Gamma(u)$ satisfies this relation, and after some formal manipulations performed without assuming the theory of Γ -function he eventually obtained the remarkable expression

$$F_c(u) = u \prod_{n=1}^{+\infty} \left(\frac{n}{n+1} \right)^u \left(1 + \frac{u}{n} \right)$$

that he called the “factorial of u ”, and which provides a glimpse of his later theory of prime functions. He stated that $F_c(u)$ presents “no break of continuity” for any value of x , and above all it has the character of rational, entire function like the elementary, transcendental functions e^u , $\sin u$, $\cos u$ and so forth. Following this, he proved that $F_c(u) = uF_c(u+1)$ and used the function $F_c(u)$ to establish the theory of the *analytische Fakultäten* on sound foundations.

Then, in Sect. 5 he established some theorems on the convergence of infinite products, including a proof of the simultaneous convergence (and divergence) of the series $\sum_{n=0}^{+\infty} |x_n|$ and the infinite product $\prod_{n=0}^{+\infty} (1 + |x_n|)$. Eventually, he applied this result to prove the convergence for every (real or complex) value of u of the infinite product defining $F_c(u)$ as well as of the power series in which it can be expanded.

An important step in Weierstrass's teaching career came in 1848, when he obtained a position at the Gymnasium in Braunschweig. The following year he published a draft of a theory of Abelian functions in the *Programmheft* of the Gymnasium. The basic ideas set out in this paper were later developed by Weierstrass in two papers that appeared in 1854 and 1856 respectively in Crelle's *Journal*.

In the first paper Weierstrass presented a solution of the Jacobi inversion problem in the hyperelliptic case (see Sect. 4.5). Weierstrass solved the problem for arbitrary $p > 1$ by following a completely new route. His 1854 paper presented "a short overview" of the work on Abelian functions which he had developed "several years ago" and summarised in the annual report of the Braunschweig Gymnasium for 1848–49.

The impact of Weierstrass's 1854 paper was immense. It "caused a surprise all over the mathematical world, which remained almost unique in the history of our science" (Killing 1897, 706), and Borchardt went to Braunschweig in order to meet the author of the paper. Liouville wrote a complimentary letter to Weierstrass, and the very same year a French translation was published in the *Journal de mathématiques* that he edited. Soon thereafter Weierstrass was awarded a doctoral degree *honoris causa* from Königsberg University. According to Killing (1897, 718), a delegation led by Richelot went to Braunschweig and greeted Weierstrass as "our teacher".¹⁴

6.2.5 Hyperelliptic Functions

As we saw in Sect. 4.5, Göpel and Rosenhain started with theta functions and by means of formal calculations eventually obtained the differential equations of Jacobi inversion problem in the particular case $p = 2$. Weierstrass in his 1854 paper took the opposite route from the equations of the Jacobi problem to the theta series, thus proceeding in accordance with "the nature of the problem" as Krazer and Wirtinger (1920, 622) remarked. He began with the polynomial $R(x) = (x - a_0)(x - a_1) \dots (x - a_{2n})$, where the a_i are real numbers satisfying the inequalities $a_i > a_{i+1}$. He decomposed $R(x)$ into the factors $P(x) = (x - a_1)(x - a_3) \dots (x - a_{2n-1})$ and $Q(x) = (x - a_0)(x - a_2) \dots (x - a_{2n})$ and considered the equations

$$u_j = \int_{a_1}^{x_1} \frac{P(x)}{(x - a_{2j-1})} \frac{dx}{2\sqrt{R(x)}} + \int_{a_3}^{x_2} \frac{P(x)}{(x - a_{2j-1})} \frac{dx}{2\sqrt{R(x)}} + \dots$$

$$+ \int_{a_{2j-1}}^{x_j} \frac{P(x)}{(x - a_{2j-1})} \frac{dx}{2\sqrt{R(x)}}, \quad j = 1, \dots, n. \quad (6.31)$$

¹⁴This was also recorded by Kiepert (1926, 58), and repeated many times since.

Weierstrass's aim was to "establish in detail" Jacobi's theorem, which he considered "the foundations of the whole theory". As Jacobi had remarked, for given values of $x_1, x_2, \dots, x_n$ the quantities $u_1, u_2, \dots, u_n$ have infinitely many different values. "Conversely, if the values of $u_1, u_2, \dots, u_n$ are given, then the values of $x_1, x_2, \dots, x_n$ as well as the corresponding values of $\sqrt{R(x_1)}, \sqrt{R(x_2)}, \dots, \sqrt{R(x_n)}$ are uniquely determined". Moreover, " $x_1, x_2, \dots, x_n$ are roots of a (polynomial) equation of degree n whose coefficients are completely determined, single-valued functions of the variables $u_1, u_2, \dots, u_n$ ". Analogously, Weierstrass added, there exists a polynomial function of x , whose coefficients are also single-valued functions of $u_1, u_2, \dots, u_n$ which gives the corresponding values of $\sqrt{R(x_1)}, \sqrt{R(x_2)}, \dots, \sqrt{R(x_n)}$ for $x = x_1, x_2, \dots, x_n$. Every rational symmetric function of $x_1, x_2, \dots, x_n$ could consequently be regarded as a single-valued function of $u_1, u_2, \dots, u_n$.

Weierstrass then supported the essential content of his results with a hint of the mathematical developments needed to support his claims. Most of them, however, were simply stated without proof. Weierstrass considered the function $L(x) = (x - x_1)(x - x_2) \dots (x - x_n)$ and the $2n + 1$ single-valued functions $al_\alpha(u_1, u_2, \dots, u_n) = \sqrt[h_\alpha]{L(a_\alpha)}$, where $(\alpha = 1, 2, \dots, 2n + 1)$ and h_α are suitable constants given by the formula $h_\alpha = \frac{(-1)^{\tilde{\alpha}}}{\sqrt{(-1)^{\alpha} R'(a_\alpha)}}$, where $\tilde{\alpha} = [\alpha/2]$. He called the al_α -functions Abelian functions "since they are the ones which completely correspond to the elliptic functions" to which they reduce when $n = 1$.

The al -functions can be expanded in series that are all convergent for suitable values of $u_1, u_2, \dots, u_n$:

$$al_{2\alpha-1}(u_1, u_2, \dots, u_n) = \sqrt[4]{\frac{-Q(a_{2\alpha-1})}{P'(a_{2\alpha-1})}} \{u_\alpha + \sum_{k=1} (u_1, u_2, \dots, u_n)_{2k+1}\};$$

$$al_{2\beta}(u_1, u_2, \dots, u_n) = \sqrt[4]{\frac{P(a_{2\beta})}{Q'(a_{2\beta})}} \{1 + \sum_{k=1} (u_1, u_2, \dots, u_n)_{2k}\}, \quad (6.32)$$

where $\alpha = 1, 2, \dots, n$, $\beta = 0, 1, 2, \dots, n$ and $(u_1, u_2, \dots, u_n)_m$ denotes an entire, homogeneous function of degree m in the variables $(u_1, u_2, \dots, u_n)$.

Then, on the basis of Abel's theorem, he stated the "principal property" of such functions: an addition theorem according to which $al_\alpha(u_1 + v_1, u_2 + v_2, \dots, u_n + v_n)$ are rationally expressed in terms of $al_\alpha(u_1, u_2, \dots, u_n)$, $al_\alpha(v_1, v_2, \dots, v_n)$ and their first-order partial derivatives. Following this, he wrote down (again without proof) the algebraic equation

$$\sum_{k=1}^n e_{2k-1}^2 \frac{al_{2k-1}^2(u_1, u_2, \dots, u_n)}{a_{2k-1} - x} - 1 = 0, \quad (6.33)$$

where $k = 1, 2, \dots, n$ and $e_{2k-1} = \sqrt{\frac{-Q(a_{2k-1})}{P'(a_{2k-1})}}$, whose roots are the quantities $x_1, x_2, \dots, x_n$ satisfying equations (6.31) for arbitrary $u_1, u_2, \dots, u_n$.

After writing $\sqrt{R(x_\alpha)}$ as a sum of terms involving $al_{2k-1}(u_1, u_2, \dots, u_n)$ and their first partial derivative with respect to u_k , ($k = 1, 2, \dots, n$) Weierstrass observed that there exist other equations similar to (6.33), and involving any n of the al -functions (6.32).

The functions $al(u_1, u_2, \dots, u_n)_\alpha$ become infinite for the same values of the variables $u_1, u_2, \dots, u_n$ and can be represented as the quotient of two power series in $u_1, u_2, \dots, u_n$ that are convergent for $|u_i| < U_i$ for finite, arbitrarily given U_i . This led to the conjecture, Weierstrass (1854, 140) added, that the $al(u_1, u_2, \dots, u_n)_\alpha$ can be represented as fractions with a common denominator such that both the numerator and the denominator are functions of $u_1, u_2, \dots, u_n$ which can be expanded in convergent power series. If this is the case, $al(u_1, u_2, \dots, u_n)_\alpha = p_\alpha/p$, say, then $d \log al(u_1, u_2, \dots, u_n)_\alpha$ must split into two parts, the first one becoming infinite for all the values $u_1, u_2, \dots, u_n$ such that $al(u_1, u_2, \dots, u_n)_\alpha = 0$, the second one becoming infinite for all the values for which $al(u_1, u_2, \dots, u_n)_\alpha = \infty$. The same holds true for higher logarithmic differentials, so that

$$d^\mu \log al(u_1, u_2, \dots, u_n)_\alpha = P_\alpha - P, \quad (6.34)$$

where $P_\alpha = \infty$ for $al(u_1, u_2, \dots, u_n)_\alpha = 0$ and $P = \infty$ for $al(u_1, u_2, \dots, u_n)_\alpha = \infty$. Therefore, $d^\mu \log p_\alpha = P_\alpha$ and $d^\mu \log p = P$. As P_α and P can be expressed by means of $al(u_1, u_2, \dots, u_n)_0, al(u_1, u_2, \dots, u_n)_1, \dots$ so can the functions $p_0, p_1, \dots, p_{2n}, p$ defined by $2n + 2$ differential equations. In 1894, when editing this paper in his *Werke*, Weierstrass added a footnote here to point out that his statement needed a more precise formulation, and to this end he referred to his (1879) where he stated and proved a necessary and sufficient condition in order that

$$d \log f(x_1, x_2, \dots, x_n) = \sum_{\alpha=1}^n f_\alpha(x_1, x_2, \dots, x_n) dx_\alpha, \quad (6.35)$$

where $f_\alpha(x_1, x_2, \dots, x_n)$ are single-valued functions that have no essential singularities and can be represented a quotient of convergent power series (Weierstrass 1879, 165–166).

In his (1854, 140–141) Weierstrass limited himself to showing that in the elliptic case one could easily verify the correctness of his statement and obtain the functions p_1 and p by following the same route that he had followed in his (still unpublished) 1840 paper, to which he referred. From the functions p_1 and p one could eventually get Jacobi's θ -functions. Thus, the differential equations for p_1 and p provide the foundations for the whole theory of elliptic functions. By analogy with the elliptic case, Weierstrass claimed to have tried the same calculations for the $al(u_1, u_2, \dots, u_n)_\alpha$ as he had done for p_1 and p , to have succeeded in finding the relevant second-order differential equations, and eventually to have obtained the expressions for the al -functions as quotients of convergent power series. In this way, he added (1854, 142) that towards the end of 1847 he had been able to establish what he considered the “most important” theorem needed for finding a valid representation of the Abelian functions for all values $u_1, u_2, \dots, u_n$: the al -functions

could be represented as quotients of entire functions that can be expanded in convergent power series for all (real and complex) values of $u_1, u_2, \dots, u_n$. In (1854) he presented an “easier way” for obtaining this result. He succeeded in doing this by resorting to the Abelian integrals of the second and third kind.

Weierstrass introduced the expression

$$Sl(u_1, u_2, \dots, u_n) = \sum_{i=1}^n \int_{a_{2i-1}}^{x_i} \frac{1}{2} \frac{\sqrt{R(a)}}{P(a)} \frac{P(x)}{x-a} \frac{dx}{\sqrt{R(x)}}, \quad (6.36)$$

where $x_1, x_2, \dots, x_n$ are expressed in terms of $u_1, u_2, \dots, u_n$, and in each integration x takes the same values as in (6.31). He observed that the function $Sl(u_1, u_2, \dots, u_n)$ has the form

$$A \log \varphi(u_1, u_2, \dots, u_n) + \varphi_1(u_1, u_2, \dots, u_n),$$

where φ and φ_1 are single-valued functions of $u_1, u_2, \dots, u_n$. After putting $\frac{P(a)}{\sqrt{R(a)}} = b$, he stated that when a is close to one of the values $a_1, a_3, \dots, a_{2n-1}$ the function $Sl(u_1, u_2, \dots, u_n)$ can be expanded in power series of b whose coefficients, when a is close to a_{2i-1} , he denoted by $Sl(u_1, u_2, \dots, u_n)_i$ where

$$dSl(u_1, u_2, \dots, u_n)_k = \sum_{i=1}^n \frac{1}{2} \frac{Q(a_{2k-1})}{P'(a_{2k-1})} \frac{P(x_i dx_i)}{(x_i - a_{2k-1})^2 \sqrt{R(x_i)}}. \quad (6.37)$$

After remarking that the n functions $Sl(u_1, u_2, \dots, u_n)_i$ are single-valued functions like the $al(u_1, u_2, \dots, u_n)_0, al(u_1, u_2, \dots, u_n)_1, \dots$, he added that all Abelian integrals of the second kind can be reduced to them and all Abelian integrals of the third kind to the function (6.36). Then Weierstrass stated that a function $Al(u_1, u_2, \dots, u_n)$ can be defined such that

$$\begin{aligned} & d \log Al(u_1, u_2, \dots, u_n) \\ &= - \sum_{i=1}^n (J_i^{2i-1} + Sl(u_1 - K_1^{2i-1}, u_2 - K_2^{2i-1}, \dots, u_n - K_n^{2i-1}) du_i \end{aligned} \quad (6.38)$$

is a complete differential (J_i^{2i-1} and K_i^{2i-1} are suitable, definite integrals of the second, resp. first kind). The Al -function thus defined can be expanded in a convergent power series for all (finite) values of u whose coefficients are rational functions of a_i . The functions $Sl(u_1, u_2, \dots, u_n)$ and $Sl(u_1, u_2, \dots, u_n)_i$ can be expressed in terms of the Al -function. In addition, even the functions $al(u_1, u_2, \dots, u_n)_\alpha$ can be expressed as the quotient of two Al -functions. These functions represent the true goal of Weierstrass's research, as Brill and Noether (1894, 244) remarked.

In order to obtain an “effective” representation of the Al -functions, Weierstrass began by observing that

$$Al(u_1 + 2K_1^\alpha, \dots) = (-1)^\alpha e^{-2 \sum_i J_i^\alpha (u_i + K_i^\alpha)} Al(u_1, \dots), \quad (6.39)$$

where the periods of the first, resp. second kind K^α and J^α satisfy the $n(2n-1)$ bilinear relations that Weierstrass had published in 1849 in his Braunsberg Program, and are analogous to Legendre's equation between the complete elliptic integrals of the first and second kind. After introducing new variables $v_1, v_2, \dots, v_n$ defined as linear combinations of the $u_1, u_2, \dots, u_n$ whose coefficients are expressed in terms of K^α , Weierstrass obtained the generalised Jacobi theta function expressed as

$$Jc(v_1, v_2, \dots, v_n) = g e^{E(u_1, u_2, \dots, u_n)} Al(u_1, \dots, u_n), \quad (6.40)$$

where g is a constant that can be determined by putting $Al(0, 0, \dots, 0) = 1$, and $E(u_1, u_2, \dots, u_n)$ is a homogeneous, entire, second degree function of $u_1, \dots, u_n$. As [Weierstrass \(1854, 149\)](#) observed, for $n = 1$ the function $Jc(v_1, v_2, \dots, v_n)$ reduces to the theta function “introduced in analysis by Jacobi”.

The function $Jc(v_1, v_2, \dots, v_n)$ is an entire, periodic function that can be expanded in a convergent series (for all finite values of v) of the form

$$Jc(v_1, v_2, \dots, v_n) = S e^{-2 \sum_{i,j}^n m_i m_j \delta_{i,j}} \cos(m_1 v_1 + m_2 v_2 + \dots + m_n v_n), \quad (6.41)$$

where m_i, m_j are integers such that $-\infty < m_i, m_j < +\infty$ and S depends on them. Having obtained the function $Jc(v_1, v_2, \dots, v_n)$, in terms of which the Al -functions could be represented, the Jacobi inversion problem in the hyperelliptic case was completely solved.

The $\frac{1}{2}n(n+1)$ parameters $\delta_{i,j}$ are such that $\delta_{i,j} = \delta_{j,i}$. Being expressed in terms of the integrals of the first kind K^α , they depend only on $2n-1$ constants, namely, as [Brill and Noether \(1894, 245\)](#) remarked, the $2n+2$ roots of $R(x) = 0$ minus 3 constants that vanish because of linear substitutions. Weierstrass stated that when $n = 2$ his results coincided with Göpel's and Rosenhain's—he knew of Rosenhain's work only through the correspondence ([Jacobi 1850](#)) published in Crelle's *Journal*. “Jacobi had already observed that for $n = 3$ the generalised theta-functions contain more constants than the Abelian functions of 3 variables”, [Weierstrass \(1854, 143\)](#) added. “Therefore, determined relations must hold among these constants in order that the relevant series lead to Abelian functions”. Weierstrass was happy to state that he had found the relevant relations for any n . However, he added, he doubted whether this was possible a priori starting from the series without having independently developed the theory of Abelian functions “with another method”.

As we remarked at the end of Sect. 4.5, this is a difficult problem which Weierstrass merely mentioned in this paper. Nor did he tackle it two years later, when he resumed this work and published the first part of an expanded and detailed version of it in Crelle's *Journal*.

As he had done in his 1854 paper, in his (1856) Weierstrass considered the polynomial $R(x) = A_0(x-a_1)(x-a_2)\dots(x-a_{2\rho+1})$ and the analogous polynomials $P(x) = (x-a_1)(x-a_2)\dots(x-a_\rho)$ and $Q(x) = \frac{R(x)}{P(x)} = A_0(x-a_{\rho+1})\dots(x-a_{2\rho+1})$,

where this time the a_j are any complex numbers such that $a_j \neq a_k$ for $j \neq k$. Instead of equations (6.31) he considered the corresponding system of differential equations

$$du_m = \sum_{j=1}^{\rho} \frac{1}{2} \frac{P(x_j)}{x_j - a_m} \frac{dx_j}{\sqrt{R(x_j)}}, \quad m = 1, \dots, \rho, \quad (6.42)$$

and formulated the Jacobi inversion problem as the problem of finding solutions $x_j = x_j(u_1, \dots, u_\rho)$ to the system that satisfy the initial conditions

$$x_j(0, \dots, 0) = a_j, \quad (j = 1, \dots, \rho).$$

Weierstrass succeeded in proving that the solutions $x_j = x_j(u_1, \dots, u_\rho)$ are single-valued functions of $u_1, \dots, u_\rho$ in a neighbourhood of the origin. They could be considered as the roots of a polynomial equation of degree ρ , whose coefficients were given in terms of al -functions which, for any bounded value of $(u_1, \dots, u_\rho)$, are single-valued functions expressed as quotient of power series.

To this end he showed that for suitable small $|u_j| < U_j$ the expressions

$$s_j = \sqrt{\frac{P'(a_j)}{Q(a_j)}(x_j - a_j)}, \quad j = 1, \dots, \rho \quad (6.43)$$

can be expanded in convergent series as

$$s_j = u_j + (u_1, u_2, \dots, u_\rho)_3 + (u_1, u_2, \dots, u_\rho)_5 + \dots, \quad (6.44)$$

where $(u_1, u_2, \dots, u_\rho)_{2k+1}$, $k = 1, 2, \dots$ denotes an entire, homogeneous function of $u_1, u_2, \dots, u_\rho$ of degree $2k+1$. The same holds true for $\frac{P(x_j)}{\sqrt{R(x_j)}} = \frac{\sqrt{R(x_j)}}{Q(x_j)}$ as $\frac{P(x_j)}{\sqrt{R(x_j)}}$ can be expressed in (odd) power series of s_j .

In addition, Weierstrass observed that

$$x_1, x_2, \dots, x_\rho, \text{ and } \sqrt{R(x_1)}, \sqrt{R(x_2)}, \dots, \sqrt{R(x_\rho)}$$

are single-valued functions of $u_1, u_2, \dots, u_\rho$ for $|u_j| < U_j$. However, one could get this for arbitrary values of u_j by resorting to Abel's theorem. To prove this claim, instead of the ρ variables u_j , Weierstrass introduced $2\mu\rho$ new variables $u_j', u_j'', \dots, u_j^{(2\mu)}$, $j = 1, 2, \dots, \rho$ and μ any arbitrary integer, such that $|u_j^{(m)}| < U_j$, $m = 1, 2, \dots, 2\mu$. Now, let $s_j^{(m)}$, $x_j^{(m)}$, $\sqrt{R(x_j^{(m)})}$ be the expressions corresponding to s_j , x_j , $\sqrt{R(x_j)}$ ($j = 1, 2, \dots, \rho$) after substituting $u_1^{(m)}, u_2^{(m)}, \dots, u_\rho^{(m)}$ in place of $u_1, u_2, \dots, u_\rho$. Then one can determine a polynomial equation of degree $2\mu\rho + \rho$ that links the x_j with the $x_j^{(m)}$. This equation has the form

$$P(x)M^2(x) - Q(x)N^2(x) = \Pi(x)\varphi(x) = 0, \quad (6.45)$$

where $M(x)$ and $N(x)$ are completely determined by the $2\mu\rho$ roots $x_j^{(m)}$ of

$$\begin{aligned} \Pi(x) &= (x - x'_1) \dots (x - x'_\rho)(x - x''_1) \dots (x - x''_\rho) \dots \\ &\quad (x - x_1^{(2\mu)}) \dots (x - x_\rho^{(2\mu)}) = 0. \end{aligned} \quad (6.46)$$

Accordingly, $P(x)M^2(x) - Q(x)N^2(x)$ is divisible by $\Pi(x)$ and $\varphi(x)$ is an entire function of the form

$$\varphi(x) = x^\rho + P_1x^{\rho-1} + P_2x^{\rho-2} + \dots + P_\rho, \quad (6.47)$$

where each P_j is rationally expressed in terms of $x_j^{(m)}$ and $\sqrt{R(x_j^{(m)})}$ ($j = 1, 2, \dots, \rho$ and $m = 1, 2, \dots, 2\mu$). Denoting the roots of the equation $\varphi(x) = 0$ by $x_1, x_2, \dots, x_\rho$, from Abel's theorem one has

$$\begin{aligned} &\sum_{j=1}^{\rho} \frac{1}{2} \left(\frac{P(x'_j)}{x'_j - a_m} \frac{dx'_j}{\sqrt{R(x'_j)}} + \frac{P(x''_j)}{x''_j - a_m} \frac{dx''_j}{\sqrt{R(x''_j)}} + \dots \right. \\ &\quad \left. + \frac{P(x_j^{(2\mu)})}{x_j^{(2\mu)} - a_m} \frac{dx_j^{(2\mu)}}{\sqrt{R(x_j^{(2\mu)})}} \right) = \sum_{j=1}^{\rho} \frac{1}{2} \frac{P(x_j)}{x_j - a_m} \frac{dx_j}{\sqrt{R(x_j)}}, \quad m = 1, \dots, \rho. \end{aligned} \quad (6.48)$$

Then, from the condition that $P(x)M^2(x) - Q(x)N^2(x)$ is divisible by $\Pi(x)$ Weierstrass deduced that the functions $M(x)$ and $N(x)$ can be expressed as fractions whose numerator and denominator can be expanded in power series of the $2\mu\rho$ variables $u_j^{(2\mu)}$ and the function $\varphi(x)$ has the form

$$\varphi(x) = P(x) - \sum_{j=1}^{\rho} \frac{Q(a_j)}{P'(a_j)} \frac{P(x)}{(x - a_j)} \psi_j^2, \quad (6.49)$$

where

$$\psi_j = \sqrt{\frac{\varphi(a_j)}{-Q(a_j)}} = \frac{U_1^j + U_3^j + U_5^j + \dots}{1 + U_2^{(0)} + U_4^{(0)} + U_6^{(0)} + \dots} \quad (6.50)$$

the $U_m^{(k)}$ being homogeneous, entire functions of degree m of the variables $2\mu\rho$ variables $u_j^{(2\mu)}$. The relevant series are convergent for $u_j^{(2\mu)} < U_j$ as were the above series in u_j .

Assuming arbitrary large values T_j , one can chose μ large enough so that for any j , $T_j < 2\mu U_j$. Then, one can put $u'_j = u''_j = \dots = u_j^{(2\mu)} = u_j/2\mu$, ($j = 1, 2, \dots, \rho$)

so that the homogeneous entire functions $U_m^{(k)}$ transform into homogeneous entire functions of u_j , and the numerator and denominator of ψ_j become series of u_j that are convergent for $u_j < T_j$. Following this, Weierstrass proved that the values of the ψ_j , and consequently the values of the coefficients of $\varphi(x)$ as well as $x_1, x_2, \dots, x_\rho, \sqrt{R(x_1)}, \sqrt{R(x_2)}, \dots, \sqrt{R(x_\rho)}$ are independent of μ . After observing that the functions ψ_j can be defined for any value of $u_1, u_2, \dots, u_\rho$ via a suitable choice of μ , he called them “hyperelliptic functions” and denoted them as $\psi_j = al(u_1, u_2, \dots, u_\rho)_j$.

These developments, Weierstrass concluded, provide the rigorous proof that the solutions $x_1, x_2, \dots, x_\rho$ of the system (6.42) that satisfy the initial conditions can be expressed as the roots of an equation of degree ρ of the form

$$\sum_{j=1}^{\rho} \frac{Q(a_j)}{P'(a_j)} \frac{al^2(u_1, u_2, \dots, u_\rho)_j}{(x - a_j)} = 1, \quad (6.51)$$

where the al -functions are expressed as the ratio of two power series that are convergent in any bounded domain of $(u_1, u_2, \dots, u_\rho)$.

Next Weierstrass considered the integral

$$\int \left(\frac{F(x_1)dx_1}{\sqrt{R(x_1)}} + \frac{F(x_2)dx_2}{\sqrt{R(x_2)}} + \dots + \frac{F(x_\rho)dx_\rho}{\sqrt{R(x_\rho)}} \right), \quad (6.52)$$

where $F(x)$ is any rational function.

Following the same route as before, the

$$x_1, x_2, \dots, x_\rho, \text{ and } \sqrt{R(x_1)}, \sqrt{R(x_2)}, \dots, \sqrt{R(x_\rho)}$$

can be expressed in terms of $u_1, u_2, \dots, u_\rho$. Under this assumption he proved that there is a function $Al(u_1, u_2, \dots, u_\rho)$ defined by the equation

$$dAl(u_1, u_2, \dots, u_\rho) = \sum_{j=1}^{\rho} \frac{1}{2} \frac{\sqrt{R(a)}}{P(a)} \frac{P(x_j)}{x_j - a} \frac{dx_j}{\sqrt{R(x_j)}} \quad (6.53)$$

and satisfying the condition $Al(0, 0, \dots, 0) = 0$, such that any integral (6.52) can be expressed in term of $Al(u_1, u_2, \dots, u_\rho)$ and its differentials.

Weierstrass was pleased to state that all Abelian transcendental functions could be expressed in terms of the al -functions and the Al -functions whose analytical form he was able to determine. Although this was enough to solve the proposed problem, he recognised that he was unable to find an expression of them that “corresponded to their true analytical character”, i.e. remained unchanged for all values of $u_1, u_2, \dots, u_\rho$.

As the series forming the numerator and denominator of the functions al and Al remain convergent for increasing values of $u_1, u_2, \dots, u_p$ corresponding to increasing values of μ , he conjectured that these series transform into still convergent series for $\mu \rightarrow \infty$, so that one could obtain in this way the sought-for expression of those functions. However, he did not find a rigorous proof of this conjecture. In fact, explained Weierstrass, despite his successes so far he could not obtain an analytical definition of the two functions whose ratio represented an arbitrary Abelian function.

“Here”, he stated (1856, 347), “we encounter a problem that, as far as I know, has not yet been studied in its general form, but is nevertheless of particular importance for the theory of functions”. According to [Krazer and Wirtinger \(1920, 625\)](#), “this was to tackled in the continuation of the paper” promised by Weierstrass. In the course of his life he returned many times to this problem in an attempt to solve it. As [Markushevich \(1996, 247\)](#) has remarked, the factorisation theorem for entire functions Weierstrass that was able to establish some 20 years later (see Sect. 6.7) can be regarded as an outcome of this research, for it provided a positive answer to the problem in the case of a single variable.

In order to show that his approach permitted one to treat the theory of elliptic and Abelian function on a par, in the concluding part of his 1856 paper Weierstrass presented a detour on elliptic functions, where he summarised the main results he had obtained in his 1840 paper. However, the promised continuation of Weierstrass’s paper never appeared. As we saw in Sect. 5.7, in 1857 a completely new approach to the theory of Abelian integrals was published by Riemann. What Riemann did surpassed by far anything Weierstrass had been able to produce. (It is worth noting that, in spite of the fact that both Weierstrass and Riemann gave their papers the same title and used the same wording, they meant different things by it. Whereas Weierstrass defined Abelian functions to be the single-valued, analytic functions of several complex variables related to his solution of the Jacobi inversion problem, Riemann understood Abelian functions to be the integrals of algebraic functions introduced by Abel’s theorem.)

After the publication of Riemann’s paper Weierstrass decided to withdraw the continuation of his own. Even though Riemann’s work “was based on foundations completely different from mine, one can immediately recognise that his results coincide completely with mine” Weierstrass later stated (in *Math. Werke* 4, 9–10). “The proof of this requires some research of algebraic nature” which he was not able to complete until the end of 1869, when Weierstrass succeeded in giving the solution of the general inversion problem in the form he presented in his lectures on Abelian functions. However, his paper was flawed by some inaccuracies that he himself later recognised in a letter to Borchardt in 1879.¹⁵

¹⁵Weierstrass (mistakenly) kept maintaining that any domain of $\mathbb{C}^n$ is the natural domain of existence of a meromorphic function. This mistake was to be pointed out in papers by Hartogs and E.E. Levi in the first decade of the twentieth century (see Sect. 9.4.4).

6.2.6 *From Braunsberg to Berlin*

The publication of his 1854 paper on Abelian functions opened the way to Weierstrass's academic career. In January 1855 Crelle wrote to the Prussian Minister of Education expressing the hope that such an exceptional mathematician as Weierstrass could get an academic position in order to continue his research. Asked about this by the Minister, Dirichlet, although confirming Crelle's view and hope, commented that until then Weierstrass "has given only partial proofs of his researches [in his 1854 paper], where the intermediate explanations are lacking" (quoted in (Dugac 1973, 52)).

In the same year, the death of Gauss turned to be the decisive fact for Weierstrass's entry in the academic scene. As was usual in German universities, replacing a chair involved a lot of academic negotiations. Dirichlet moved from Berlin to Göttingen to succeed Gauss, and subsequently Kummer was hired from Breslau to Berlin to replace Dirichlet, thus leaving his chair in Breslau vacant. Weierstrass, "our teacher" as we have seen Richelot call him, applied for it, but Kummer confidentially opposed him. Given the enormous success of Weierstrass's later lectures in Berlin, it is ironic to note that Kummer's hesitations depended on his doubts about Weierstrass's ability as a teacher. But, in a letter to Kronecker, Kummer stated that Weierstrass "could have been named a member of the Berlin Academy of Sciences with all honour" (as indeed happened soon thereafter, in November 1856) but his great paper on Abelian functions "did not yet offer the necessary guarantee" that he, as the sole professor of mathematics in Breslau, could provide all what was needed "for the education of young mathematicians" (Lorey 1915). So Joachimsthal was hired instead at Breslau to continue Kummer's "mathematical school".

Once he was settled in Berlin, on June 14th Kummer proposed to the Philosophy Faculty that they nominate both Weierstrass and Carl Wilhelm Borchardt as professor Extraordinarius. Borchardt had lectured in Berlin since 1848 and had taken over the editorship of Crelle's *Journal* after Crelle's death in 1855. The decision was postponed, with the effect that two days later, thanks to the joint efforts of Alexander von Humboldt and Richelot, Weierstrass was appointed a professor at the Berlin Gewerbeinstitut (later the Gewerbeakademie, today the Technische Universität). In summer 1856 he was also offered a chair at Vienna University. In September Weierstrass visited Vienna with Kummer, but in the end he declined the offer. Kummer was so impressed by his colleague that, back in Berlin, he wrote to the Minister of Education urging him to give Weierstrass a professorship at the University. Three days later Weierstrass was appointed Extraordinarius at Berlin University.

Leopold Kronecker, a 32-year-old independently wealthy man from Liegnitz, had also settled in Berlin in 1855. He had been taught mathematics at the Liegnitz Gymnasium by Kummer, studied at Berlin University under Dirichlet and Steiner, and had followed Kummer's lectures at Breslau. He then graduated from Berlin in 1845 with a thesis on algebraic number theory under Dirichlet's supervision. Following his father's wishes, Kronecker worked as a banker while still keeping in

scientific touch with Kummer. In Berlin Kronecker found the appropriate scientific milieu for developing his deep interest in mathematics. Free from financial troubles Kronecker devoted himself completely to number theory, elliptic function theory, and algebra, which he made the subject of a number of papers. On the strength of this work he was elected a member of the Berlin Academy of Sciences in 1860. As such, he was also allowed to give free courses at the University. Thus, by the mid-1850s the celebrated “triumvirate” of Kummer, Kronecker and Weierstrass, which was to dominate the German mathematical milieu for some 25 years, had settled in the Prussian capital. For the former teacher from Braunschweig, a new life began at the age of 40.

6.3 Weierstrass in Berlin

At the beginning of his teaching career in Berlin Weierstrass, the future master of rigour, apparently had almost no interest at all in rigour and foundations. His inaugural address to the Berlin Akademie der Wissenschaften in 1857 suggests what was then at the core of his mathematical interests, and the direction he intended to follow in his research, and this talk reveals a somewhat unexpected picture of the great analyst. Weierstrass began by recognising the “powerful force of attraction” that the theory of elliptic functions had exerted on him since his student days under Gudermann. This theory, Weierstrass went on, “has retained a definite influence on my whole mathematical education”. Then he referred to Jacobi’s proof of the existence of periodic functions of several variables whose “fundamental properties” are based on Abel’s theorem, and stated that “one of the main problems of mathematics” which he decided to investigate was “to give an actual representation of Abelian functions”. He recognised that he had published results “in an incomplete form”. “However”, Weierstrass continued, “it would be foolish if I were to try to think only about solving such a problem, without being prepared by a deep study of the methods that I am to use and without first practicing on the solution of less difficult problems”.

This reference to his papers on Abelian functions, which justified his hiring at both Berlin University and the Akademie, made clear enough what was the aim and scope of his future research. At this point he could have expressed his concern about the need for greater rigour in analysis. Instead, he claimed he would have been very happy if his study could be of some relevance for the applications of mathematics, specifically physics. In fact, Weierstrass stated he was not at all indifferent to the possible applications of a mathematical theory and emphasised the need for a deeper understanding of the relationship between mathematics and physics, “a subject which lies very close to [my] heart”.

His concluding remarks were devoted to the relationships between pure, abstract mathematics and applications. After mentioning the abstract speculations about conic sections developed in the Antiquity by the Greeks long before these curves

were applied to astronomy, Weierstrass claimed he was living in the hope that more functions exist of the same kind as Jacobi's theta function, which at the very same time shows, on the one hand, how to split a number into square numbers, and on the other hand is the only function which can represent the "true law" of oscillation of a pendulum.

According to these premises, it is not surprising that his very first course delivered at the Berlin University in WS 1856/57 was devoted to "Selected chapters of mathematical physics"; Lazarus Fuchs was among the students who attended that course. It was followed in SS 1857 by a course on the "General theorems concerning the representation of analytic functions by means of convergent series", where he expounded his own relevant results, and his first course on "the theory of elliptic functions", which was largely inspired by his very first paper of 1840.

6.3.1 Weierstrass's First Course on Elliptic Functions

Among the six students who attended this course were Fuchs and Leo Koenigsberger, according to whom they could never forget the "powerful impression and incomparable excitement" exerted on them by the "deep and over-arching content" of that lecture combined with the "natural, unvarnished" way of expounding it that was "free from any rhetoric".

More than fifty years later [Koenigsberger \(1917\)](#) published a summary of the content of the course based on his own "partly verbatim" notes in order to give "a picture" of Weierstrass's lectures. Weierstrass began with a short historical introduction, mentioning Fagnano's work on the lemniscate, Euler's addition theorem for elliptic integrals and their reduction to the normal form by Legendre. Then he referred to elliptic functions as introduced in Jacobi's *Fundamenta nova*, mentioned Abel's theorem on their expressions as quotients of power series and eventually introduced the theta series.

Weierstrass observed that, contrary to the transformations based approach of the *Fundamenta nova*, in his lectures on elliptic functions Jacobi had taken an approach based on theta series (see Sect. 4.2). Although in Weierstrass's view the latter approach was not "the true scientific and natural one" he kept it because it was the most convenient for understanding the properties of elliptic functions. Accordingly, he introduced theta series and their properties, defined his own *AI*-functions to represent the elliptic functions *snu*, *cnu*, *dnv*, and devoted the last part of the lectures to a throughout discussion of the "most general relationships" between elliptic integrals, ending with the discussion of Abel's theorem for hyperelliptic integrals.

[Koenigsberger \(1917, 421\)](#) remarked that in the whole lecture course Weierstrass only occasionally let his listeners guess that power series "were the bricks which provided him with the means to build all of function theory in order to penetrate the most hidden properties of Abelian functions with a unitary and rigorous method".

However, he did resort extensively to power series expansions. Thus, for instance, he expanded the complete integrals

$$K = \int_0^1 \frac{d\xi}{\sqrt{(1-\xi^2)(1-k^2\xi^2)}}, \quad K' = \int_0^{1/k} \frac{d\xi}{\sqrt{(\xi^2-1)(1-k^2\xi^2)}} \quad (6.54)$$

in power series in order to express the three elliptic functions by means of theta series containing the argument and the integrals only.

Having proved that the power series expansions of K and K' are convergent for all real and complex values of k , Weierstrass tackled the question whether $\operatorname{Re}(\tau) > 0$, $\tau = K'/K$ for complex values of k because the theta series are convergent only under this condition. In order to prove this he stated without proof the identity theorem for power series of several variables. The theorem states that if two analytic functions defined on a domain D agree on an arbitrarily small ball centred on a point $c \in D$ then they agree on the largest ball centred on c and lying in D . Weierstrass stated it in the case when the overlap domain contains an appropriate open neighbourhood of the origin (Koenigsberger 1917, 409–410).

Throughout the lecture Weierstrass avoided mentioning Cauchy's methods, even though he could have done so when stating that when integrating a function which becomes infinite at a point one can avoid the singular point by following “an arbitrary curve” around it (Koenigsberger 1917, 413). In the concluding remarks of his paper Koenigsberger observed that the students who attended that lecture would have had no idea of the “great extension” the theory of functions had received over the years particularly because of “the numerous and fruitful discoveries made by Cauchy in the field of complex integration”.

In Koenigsberger's eyes this “omission” had already affected Jacobi's theory of elliptic functions, and “this struck the young mathematicians all the more forcefully because soon thereafter the surprising works of Riemann re-established the continuity with Cauchy's works and clarified their significance”. As we will see, avoiding Cauchy's integral theorem (and “transcendental” methods in general) was to remain a major point in Weierstrass's approach to the theory of analytic functions.

Eventually, Koenigsberger remarked that the normal form for elliptic integrals of the first kind

$$\int \frac{ds}{\sqrt{4s^3 + g_2s + g_3}}$$

and, consequently, the inverse function, which was to become standard in Weierstrass's later treatment of elliptic functions (see Sect. 6.6), were still missing. Koenigsberger added that he had found it for the first time in a manuscript on Abelian functions which Weierstrass gave him in 1862. Lampe (1899, 40), too, recorded that the functions $\wp$ and σ first appeared in Weierstrass's WS 1862/63 lectures on elliptic functions (Fig. 6.1).

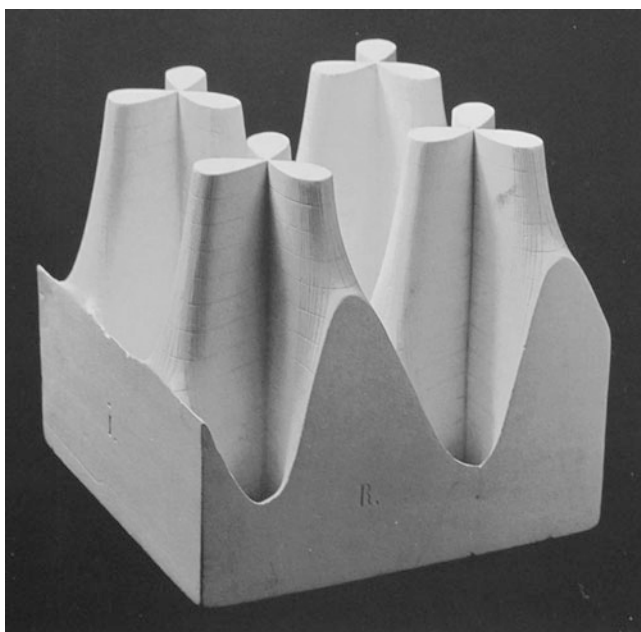


Fig. 6.1 The real part of the Weierstrass $\wp$ function (Fischer, *Mathematische Modelle—Mathematical Models*, Vieweg ©1986, number 131)

6.3.2 Lectures on Various Subjects

“The applications of elliptic functions to geometry and mechanics” was the subject of Weierstrass’s WS 1857/58 lectures. This was coupled with a course on the theory and applications of trigonometric series and definite integrals. Clearly, this was related to the problem of representing arbitrary single-valued functions of one real variable in Fourier series and integrals.

He referred to his 1857/58 lectures later on, in a letter to his former student Hermann Amandus Schwarz on 14 March 1885, where Weierstrass announced his theorem on the approximation of a single-valued, real function $f(x)$ continuous on an interval (a, a') by means of an absolutely, uniformly convergent series of trigonometric polynomials. In this connection, he mentioned his 1857/58 course and stated that “the lack of rigour” he found in all the relevant works he had at hand, and his “at that time unfruitful efforts to repair this lack” convinced him to avoid teaching that course again (see Sect. 6.5.6).

As for the convergence of Fourier series, it is worth mentioning that as late as 1873 in a letter to du Bois–Reymond on 30 November Weierstrass expressed the

“personal view” confidentially¹⁶—one he even confessed he “would refrain from stating it openly”—that there was no generally valid criterion for establishing the convergence of a Fourier series.

In spite of some sharp criticisms of du Bois–Reymond’s claim to have established a necessary condition for convergence, Weierstrass recognised that du Bois–Reymond’s works had shown that “the convergence condition stated by Dirichlet (1829) cannot be maintained anymore”, a remarkable result indeed, Weierstrass admitted, that contradicted what everybody, including he himself and Kronecker, had thought so far.¹⁷ It is hard to make sense of this rather surprising claim by Weierstrass, unless we admit the possibility that Dirichlet’s paper had become misunderstood. Indeed, Dirichlet himself seems to have been of the opinion that a continuous function, even one that has infinitely many maxima and minima, can always be represented by a convergent Fourier series, thus weakening the hypotheses of his 1829 proof. It seems that Gauss was the first to suggest, in a letter to Dirichlet, that no essential difficulties stood in the way of this extension of Dirichlet’s 1829 theorem. Dirichlet replied on 23 February, 1853 that “after a detailed consideration of the issue I find your conjecture entirely valid, provided one wishes to ignore certain entirely singular cases” (in Dirichlet, *Werke* 2, 386), and went on by to sketch a proof of this. However, his (and Gauss’s) conjecture was mistaken, as became clear in 1876 when du Bois–Reymond provided an example of a continuous function whose Fourier series fails to converge at isolated points (see du Bois–Reymond 1876a,b).

The lectures Weierstrass gave in 1858 and 1859 treated a number of different subjects, including “selected chapters of the integral calculus”, the “new geometry” (in summer 1858), and “the formulae of analytic dioptrics” (!) in WS 1859/60. At the very same time, however, in winter 1859/60 he lectured for the first time on the “Introduction to analysis”. This was followed in summer 1860 by a course on the integral calculus. Hints of the content of these courses can be found in the contemporary course he gave at the Gewerbeinstitut in summer 1861 (see 6.3.3).

Next term Weierstrass came back to his favourite subject, elliptic functions, and devoted a course to a selection of problems that can be solved by means of these functions. In WS 1861/62 he announced a course on the “General theory of analytic functions”. However, a serious breakdown caused by overwork forced him to stop lecturing for a whole academic year. Once he had recovered he resumed teaching elliptic functions and their applications, and in summer 1863 delivered his first course on the introduction to the theory of Abelian functions. Eventually, in WS 1863/64 he was able to teach the course he had announced on analytic function theory.

¹⁶See Weierstrass (1923a, 202).

¹⁷For a short discussion of the relevant works of du Bois–Reymond, see, e.g., Bottazzini (1986, 196, 210–211).

6.3.3 *A Course on Differential and Integral Calculus*

We can get an impression of Weierstrass's early lectures on analysis by looking at a set of (unpublished) lecture notes taken by Schwarz from the course on "Differential and Integral Calculus" given by Weierstrass in summer 1861 at the Gewerbeinstitut in Berlin.

According to Schwarz's own words, this was the very first course by Weierstrass that he was happy to follow. Lampe also attended this lecture. Two sets of lecture notes are kept in the library of the Mittag-Leffler Institute. The one of 82 typewritten pages and dealing only with differential calculus is clearly to be attributed to Schwarz and will be discussed here. The second (anonymous) handwritten set of lecture is much wider in extent. The first part of it deals with differential calculus, and in addition to what can be found in Schwarz's, it includes applications to the theory of plane curves. The second part deals with the integral calculus and its application to the evaluation of surfaces (the area of the ellipse and the hyperbola), and finally with double and multiple integrals and their applications.¹⁸

Even though Weierstrass did not consider complex functions in these lectures, and therefore their subject is not of our primary concern, the manuscript is interesting in many ways. Weierstrass began with the definition of both variable magnitudes (when they can take "not simply many but infinitely many isolated values") and functions: "when two variable magnitudes are so related that to every determinate value of the one corresponds one determinate value of the other, the latter is called a function of the former". It is worth noting that in later years Weierstrass refused to adopt this definition, which he ascribed to Dirichlet, because in his view it was too general and therefore useless (see Sect. 6.5.6).

As it was usual at that time, Weierstrass distinguished between single-valued and many-valued functions. Having defined the entire and rational functions, he introduced the concept of a continuous function. This is a function "such that infinitely small variations of the function correspond to infinitely small variations of the argument".

This seemingly old-fashioned (actually, Cauchy-ist) way of speaking was justified by the following more precise statement in term of inequalities: "Let $f(x)$ be a function and be x a determinate value. Suppose that x becomes $x + h$ and consider the corresponding variation $f(x + h) - f(x)$. If it is possible to determine a limit δ for h such that, for all the values of h whose absolute value is smaller than δ , the difference $f(x + h) - f(x)$ becomes smaller than any given arbitrarily small ε , then

¹⁸Actually, geometrical applications of the differential and integral calculus were part of the lectures, as Schwarz explicitly recorded in a letter to Weierstrass on 17 June 1888 when, in answer to a request, Schwarz sent him the set of his 1861 lecture notes. After pointing out that his notes "do not contain the application to the research of the properties of plane curves, in particular your highly satisfactory theory of the contact of curves", he apologised for "so many inaccuracies" and "the possible mistakes" contained in the notes, to be attributed to his being a beginner.

one says that infinitely small variations of the function correspond to infinitely small variations of the argument”.

Commenting on this definition Dugac (1976, 6–7) perhaps went too far in stating that from this time onwards we can date the substitution of inequalities—which imply the topological notion of neighbourhood—for the intuitive idea of “tending towards”. Indeed, while one might easily agree that Weierstrass was more systematic in his use of inequalities than anybody before him, one must not forget that such inequalities had already been introduced by Cauchy as early as 1821. Even the first intuition of the “topological notion of neighbourhood” can be found in the concept of *voisinage* (of a point) that Cauchy used in (1821a) when defining the continuity of a function.

Dugac (1976, 7) also guessed that Weierstrass might have been influenced by Dirichlet, whom he visited during his stay in Berlin from August to October 1844. On that occasion he might have learned of the content of some of Dirichlet’s courses, including the one on the theory of integration, and as Riemann had done (see Sect. 5.2), Weierstrass, too, might have found in it the source of his definition of continuity.

Even though Weierstrass’s previous definition of a continuous function still requires some more care about signs and absolute values in order to make it rigorous by modern standards, one can still recognise hints of his expected style. The same holds true for the definitions he gave later on, when introducing the “fundamental concepts” of the differential calculus. There he stated for instance: “Let h be a magnitude which can assume infinitely small values, and let $\phi(h)$ be an arbitrary function of h with the property that it becomes infinitely small for infinitely small values of h ”—that is, he added as an explanation—“as soon as any determinate arbitrarily small magnitude ε is given, one can always determinate a magnitude δ such that for all values of h whose absolute value is smaller than δ , $\phi(h)$ is smaller than ε ”.

The definition of a continuous function was followed by the statement of the intermediate value theorem:

Let a continuous function of x have a determinate value y_1 for a determinate value x_1 of the argument and a determinate value y_2 for another determinate value x_2 of the argument, and let y_3 be any value between y_1 and y_2 , then there must exist at least one value x_3 between x_1 and x_2 for which the function takes the value y_3 .

Weierstrass’s proof is worth following in detail because it is interesting in many ways. It was based on two lemmas.

- Lemma 1 stated that a continuous function $f(x)$ such that $f(x_1) = y_1 \neq 0$ assumes in a appropriate neighbourhood of x_1 values which have the same sign as $f(x_1)$.
- Lemma 2 stated that if $f(x)$ is continuous and $f(x_1)$ and $f(x_2)$ have opposite signs, then there must exist at least one value $\tilde{x}$ between x_1 and x_2 for which $f(\tilde{x}) = 0$.

The truth of lemma 1, Weierstrass observed, “follows immediately” from the continuity of the function. The proof of lemma 2 is based on reiterated applications

of lemma 1. Indeed, it follows from lemma 1 that there exists a “continuous sequence” of values x in the neighbourhood of x_1 for which $f(x)$ has the same sign as $f(x_1)$. “This continuous sequence needs necessarily to have a limit because $f(x)$ does not have the same sign as $f(x_1)$ for all values of x lying between x_1 and x_2 ”. Indeed, lemma 1 ensures that there exists an appropriate neighbourhood of x_2 such that $f(x)$ has the same sign as $f(x_2)$ there. Let this limit be $\tilde{x}$. Then, for all values $x_1 < x < \tilde{x}$ the function $f(x)$ has the same sign as $f(x_1)$, and for $\tilde{x} < x < x_2$ there is no value of x for which $f(x)$ has the same sign as $f(x_1)$. Consequently, $f(\tilde{x}) = 0$ because, by the previous lemma 1, it can have neither the sign of $f(x_1)$ nor the sign of $f(x_2)$. As a concluding remark Weierstrass stated (without proof) that such a continuous function on an interval takes a continuous sequence of values, i.e. a continuous map of a compact interval is a compact interval, as [Dugac \(1973, 64\)](#) boldly translated it in modern terms.

A comparison with Cauchy’s proof of lemma 2 as given in Note III of his *Cours d’analyse* by means of pairs of convergent “Cauchy sequences”, shows that, as far as rigour is concerned, Weierstrass would have done better in both the statement and the proof of his lemma by simply referring to the book of his former French colleague.

Weierstrass’s sloppy argument above concerning the existence of the “limit” $\tilde{x}$ provides evidence that, in spite of his later claims, he had a satisfactory theory of neither the real numbers nor the upper (resp. lower) bound theorems at this early stage in his career.¹⁹

According to [Dugac \(1973, 56\)](#), Weierstrass began to set out his theory of the real numbers for the first time when lecturing on the “general theory of analytic functions” in WS 1863/64. That said, one could argue that, except for Schwarz and Lampe, the Gewerbeinstitut students—who were to become future technicians—were perhaps not best suited to appreciate Weierstrass’s subtleties.

This view is supported by Kiepert’s “personal reminiscences”. Kiepert attended Weierstrass’s lectures from October 1865 to Easter 1871. According to him, Weierstrass’s lectures at the Gewerbeinstitut “did not fall on fruitful ground” because the students were so overburdened with lectures that they had no time for re-working them, and without this work they could not follow “the higher flights of Weierstrass’s lectures” ([Kiepert 1926, 59](#)). Things went better at the university for the students there had a “lively interest in mathematics, even though things never became *easy* to us”. Kiepert recorded that he had shared the duty of taking notes of Weierstrass’s lectures with a class-mate who stenographed what Weierstrass said word by word. Then they spend hours together re-working it all.

Then Weierstrass introduced the basic concepts of the differential calculus, beginning with the remark that the variation $f(x+h) - f(x)$ can “in general” be split into two parts,

¹⁹This contradicts Mittag-Leffler’s later claim that Weierstrass was in possession of his theory as early as 1841/42, and presented it in his lectures in 1857 or, at the latest, in 1859/60 (see [Kopfermann 1966, 80](#)).

$$f(x+h) - f(x) = ph + h \times (h),$$

where $h \times (h)$ is a quantity vanishing with h (or, in modern terms, $(h) = o(1)$) as becomes clear when Weierstrass proved the uniqueness of the derivative.

Having introduced differentials and derivatives, Weierstrass proved some lemmas for determining differentials, including higher order differentials, and established the relevant rules for differentiating given functions.

He then studied the variation of a function, using the properties of derivatives, established the intermediate value theorem (Lagrange's theorem), and ended with what he called "the true fundamental theorem of all analysis": if $f(x)$ is n -times differentiable at x_0 where all the derivatives vanish up to order $n-1$, then²⁰

$$f(x) - f(x_0) = \frac{(x-x_0)^n}{n!} f^{(n)}(x_0 + t(x-x_0)), \quad 0 \leq t \leq 1.$$

After this, Weierstrass turned to the development of functions in power series via the Taylor series (including the evaluation of the tail in Cauchy's form).

Having introduced the concept of differentiation for functions of a single variable, Weierstrass next turned to the case of several independent variables and proved the following theorem concerning mixed partial second derivatives, which is usually credited to Schwarz (the relevant property was already known to Euler and was considered a commonplace by eighteenth-century mathematicians):

Let $f(x,y)$ be a function of two variables which is continuous together with its first and second derivatives; one should prove that

$$\frac{\partial}{\partial y} \left(\frac{\partial f(x,y)}{\partial x} \right) = \frac{\partial}{\partial x} \left(\frac{\partial f(x,y)}{\partial y} \right).$$

To this end Weierstrass provided a proof which has since become standard. He let x and y vary separately and first wrote down the Taylor series expansions of $f(x+h,y)$ and $f(x+h,y+h)$ as far as the second order term, and then the expansions of $f(x,y+h)$ and $f(x+h,y+h)$. He then obtained the required equality of the mixed derivatives by equating the two expansions for $f(x+h,y+h)$.

After introducing higher order differentials for functions of several variables and establishing their properties, including the Taylor series expansion and the implicit function theorem, the last topic presented by Weierstrass was the differentiation of infinite series. Without mentioning Cauchy, he began by establishing in a rigorous way the latter's celebrated theorem about the sum of a series of continuous functions (1821a, 120). In doing this, Weierstrass reformulated the condition given by Cauchy in (1853a). Given a series $\sum \varphi_n(x)$ of functions that are continuous on an interval $[a,b]$, and assuming that the series is convergent for all x within the interval, Weierstrass asked for the condition one has to impose in order that the series represents a continuous function there. His answer was that for increasing m and

²⁰As Dugac (1973, 66) aptly remarked, this theorem is taken from Cauchy (1829c, 313).

with r any arbitrary positive integer “the sum $\varphi_m(x) + \varphi_{m+1}(x) + \cdots + \varphi_{m+r}(x)$ can be made smaller than any arbitrarily given magnitude”.

Following Gudermann, Weierstrass called this mode of convergence “Convergenz in gleichem Grade” (or uniform convergence, as it is called nowadays). The proof was based on a rigorous $\varepsilon - m$ argument.²¹ (Schwarz wrote that this is a necessary and sufficient condition whereas the proof clearly shows only that it is a sufficient condition.)

Weierstrass further considered two series of functions, denoted $\sum \varphi_n(x)$ and $\sum \varphi'_n(x)$, which are continuous on a given interval and such that $\varphi'_n(x)$ is the derivative of the corresponding term $\varphi_n(x)$ for each n . Under the hypothesis of the uniform convergence of the series in the interval, he was able to prove that the function $\varphi'(x) = \sum \varphi'_n(x)$ is actually the derivative of the function $\varphi(x) = \sum \varphi_n(x)$ there.

Weierstrass next applied these theorems to obtain the expansion in power series of such elementary transcendental as the exponential and trigonometric functions. Eventually, given the expansion in power series of a derived function $\varphi'(x)$ he proved that $\varphi(x)$ can be determined with a process that in fact is equivalent to a term-by-term integration (without naming it).

This course exerted a great influence on Schwarz and, in turn, on Heinrich Heine. According to [Dugac \(1973, 67\)](#), the latter is likely to have known of this course from Schwarz, who taught at Halle from 1867 to 1869. In fact, in his 1872 paper Heine openly recognised that he drew inspiration from notes of Weierstrass's lectures taken by his students and from “oral communications from [Weierstrass] himself and Herrn Schwarz and Cantor” as well ([Heine 1872a](#), 182).

6.4 Weierstrass's Programme of Lectures

As his course on differential calculus clearly shows, Weierstrass apparently first became deeply concerned with rigour when he began lecturing on the introduction to analysis. In particular, he realised that the theory of analytic functions required a rigorous foundation. Indeed, in his eyes these functions were to provide the foundations for the whole theory of both elliptic and Abelian functions, the latter being the goal he pursued all his mathematical life. As [Poincaré \(1899b\)](#) put it, Weierstrass's work could be summarised as follows:

1. To deepen the general theory of functions of one, two, and several variables—this was the basis on which the whole pyramid should be raised.
2. To improve the theory of transcendental and elliptic functions and to put them into a form which could be easily generalised to Abelian functions, the latter being a “natural extension” of the former.
3. Eventually, to tackle Abelian functions themselves.

²¹It is amusing to note that in the course of the proof Weierstrass happened to assume that ε splits into three parts whose sum is ε .

The realisation of this programme became the content of his university lectures, especially after July 1864, when Weierstrass was appointed Ordinarius at Berlin University and gave up teaching at the Gewerbeinstitut. Over the years the aim of establishing analysis, and the foundation of analytic function theory in particular, with absolute rigour became one of Weierstrass's major concerns. From the mid-1860s to the end of his teaching career he presented the whole of the analytical corpus in a cycle of lectures delivered in four consecutive semesters, according to the following programme:

1. Introduction to analytic function theory
2. Elliptic functions
3. Abelian functions
4. Applications of elliptic functions or, at times, the calculus of variations.

Examples of all of these lecture courses, except for the introduction to analytic function theory, were published in Weierstrass's *Werke*. For some twenty years he subjected his introduction to the theory of analytic functions to continuous refinements and improvements, without ever deciding to publish it. It seems that a set of lectures was intended to be published in a projected tenth volume of his *Werke*, but the project was never realised, and only seven volumes were actually published.

Weierstrass presented most of his original discoveries in his lectures, and only occasionally communicated to the Berlin Akademie some of his particularly striking results, such as the counterexample to the Dirichlet principle in 1870 or the example of a continuous nowhere differentiable function in 1872. This habit was coupled with an almost "Gaussian" dislike of publishing his results in printed papers until they had reached the required level of rigour. Moreover, he openly discouraged his students from publishing lecture notes taken from his own courses.

In many cases he would ask his most talented students to loan him their notes of his lectures, which he would then use as the starting point for improving his lectures in subsequent semesters.²² All this eventually gave Weierstrass's lectures in Berlin an aura of uniqueness. It also makes Weierstrass's case unique for historians of mathematics. More than for other mathematicians, one has to look primarily at the lecture notes of his courses in order to understand the development of his ideas, as well as the many letters he exchanged with his students, rather than at the printed papers in which he only occasionally published some of his results. This makes rendering his mathematical thought an intriguing and difficult job.

Looking at the *Berichte* of the Berlin Akademie der Wissenschaften, for instance, one sees that on many occasions only the titles of Weierstrass's contributions were printed. Even the papers dealing with his celebrated counter-examples were only "read" at the Akademie by Weierstrass. Therefore, contemporary mathematicians could hardly get a precise idea of the actual content of Weierstrass's oral communications. Among those most frustrated at this turn of events was Felice Casorati, a professor at the University of Pavia.

²²Occasionally he would even lose them, as e.g. Runge (1926, 177) recorded in his case.

Fig. 6.2 Felice Casorati
(*Acta Mathematica Table
générale des tomes 1–3,
1882–1912*, p. 135)



FELICE CASORATI

6.4.1 Casorati's Notes

Casorati had met the Göttingen and Berlin mathematicians, including Riemann and Weierstrass, in 1858 when travelling round Germany with Enrico Betti and Francesco Brioschi (Fig 6.2).²³ Some years later, when he began to write his treatise on complex analysis, with its lengthy historical, up-to-date introduction (Casorati 1868a), he was able to correspond energetically with Betti and ask his advice on questions particularly related to Riemann's approach (see Sect. 5.11.5 and Bottazzini 1977b, App. III). But when it came to writing about Weierstrass's work, Casorati (1868a, 43) observed that he was unable to report properly on what the great analyst had done because of the lack of publications. He therefore tried to obtain direct access to the sources by visiting Weierstrass in Berlin in 1864, and in the following years embarking on an intense correspondence with Schwarz in order to find out about the most recent developments Weierstrass had presented in his lectures. So, in 1864, when Casorati might have enjoyed a unique opportunity to visit Riemann in Pisa, and in the absence of any recent publications from Weierstrass, Casorati decided to travel to Berlin, in order to understand what was going on in the town that was becoming the centre of mathematical world.

Casorati recorded the content of his conversations with the Berlin mathematicians in a scientific diary that provides a careful account of the problems that were at the forefront of their mathematical research at that time, and a vivid picture of

²³See Volterra (1902) and Bottazzini (2000).

the Berlin mathematicians' research programme in the making. During his stay in Berlin Casorati met Kronecker and Weierstrass, separately and occasionally together. At times, students and other mathematicians also attended the meetings. They discussed questions of both real and complex analyses, which were to be taken up (by Weierstrass in particular) in the following decades—from the concept of continuity which, according to Kronecker, “was still a confused idea”, to the Dirichlet principle and the concept of analytic continuation.

On one occasion, Kronecker claimed that “mathematicians . . . are a bit arrogant in using the concept of a function”. Even Riemann “who is generally very precise, is not beyond censure in this regard”. Kronecker's criticism concerned Riemann's use of the Dirichlet principle: “If a function increases and then decreases or vice versa, Riemann says there must be a minimum or a maximum (see the proof of the so-called Dirichlet principle) while we should restrict the conclusion to the realm of functions which we might call reasonable” (in [Bottazzini 1986](#), 262). Apparently, this alluded to the distinction between a maximum (resp. a minimum) and upper (resp. lower) bound, which was still unclear at the time and was to be clarified later by Weierstrass's theorem on continuous functions on a closed and bounded subset of $\mathbb{R}$ or $\mathbb{R}^n$ (see Sect. 6.5.3). Ironically, Casorati's hopes of writing a planned second volume of his treatise were dashed by Weierstrass's later criticism of Riemann's use of the Dirichlet principle, and in the event it was never written.²⁴

Even Casorati's functions with infinitely many periods were listed by Kronecker among the “unreasonable” functions. In this connection, and to Casorati's surprise, Kronecker claimed to know of “functions that do not admit differential coefficients, that cannot represent lines, etc.”²⁵ Schering, who also attended this meeting, mentioned Riemann's still unpublished but quite well known Habilitationsschrift, where an example was given of an integrable function that was discontinuous at every rational point.²⁶

Casorati's scanty notes do not allow one to decide whether Kronecker actually discovered such “pathological” functions, or simply reported what Weierstrass had done on his own. As we have seen (see Sect. 6.3.3) there is no hint of pathological functions in Schwarz's lecture notes of the 1861 course Weierstrass gave at the

²⁴It seems, however, that he entertained the hope—like many an author—of completing his project. He wrote to Poincaré in February 1884 that “the surprising progress today [in complex function theory] greatly increases the desire that I have always had to continue that publication by also extending the *history* of the complex variable that only reaches in volume 1 (pp. 1–143) as far as 1865”. Casorati to Poincaré, 18 February 1884, Poincaré Archive, Nancy.

²⁵According to an “oral tradition” reported by [Hankel \(1870, 69\)](#), Jacobi was said to have expressed similar views in his lectures, claiming that “one could conceive of continuous, infinitely many-cornered curves”; Hankel himself (1870, 75) offered a nice example of such a curve. In addition, he claimed that Dirichlet and Gauss had been aware that the existence of differential quotients is not a necessary consequence of continuity. But he offered no evidence in Dirichlet's case, and to our knowledge, there is in fact no evidence in his works. As for Gauss, Hankel pointed to [Gauss \(1840, art. 16\)](#) but these offer, at most, only a very cryptic hint. And indeed Weierstrass came to the opposite opinion in his (1872, 71).

²⁶See [Riemann \(1854a, 274\)](#) and [Bottazzini \(1986, 244\)](#).

Gewerbeinstitut. According to Schwarz's recollection, in that course Weierstrass only stated that all the attempts to prove in general terms that a continuous function is differentiable have so far failed without exception (Schwarz 1973, 33–34). But this, as Dugac (1973, 92) rightly commented, was by then a rather widespread opinion following Riemann's work on definite integrals.

Although Riemann's work was continually mentioned in Casorati's talks with his German colleagues, this was generally done in a rather critical way. "Riemann's things are creating difficulties in Berlin" Casorati recorded. Thus, for instance, Kronecker stated that in Riemann's 1857 paper on Abelian functions the theta series in several variables "came out of the blue". Most notably, implicitly referring to this paper Weierstrass claimed that "he understood Riemann, because he already possessed the results of his [Riemann's] research". Weierstrass went on to claim that "Riemann's disciples are making the mistake of attributing everything to their master, while many [discoveries] had already been made by and are due to Cauchy, etc.; Riemann did nothing more than dress them in his manner for his convenience".

This rather crude assertion is not only a remarkable underestimate of Riemann's work, but it also illustrates one of Weierstrass's long-standing concerns, so evident in his lecture programme: to build a rigorous theory of elliptic and Abelian functions as a response to Riemann's discoveries. Some years later, however, after Riemann's death, Casorati himself had to recognise that Weierstrass's attitude had changed, for the latter wrote him on March 25, 1867 that

our scientific efforts are better understood and appreciated in Italy than in France and England, especially in the latter country where a stifling formalism threatens completely to choke off the feeling for deeper research. How significant indeed it is that our Riemann, whose loss we cannot mourn enough, is studied and appreciated, apart from Germany, only in Italy. In France he seemingly is well recognised but poorly understood, and in England he remains almost unknown.²⁷

Weierstrass also charged his talented student Schwarz, the "master of the representation" as he occasionally called him, with the major commitment of providing a "Weierstrassian" dressing to Riemann's mapping theorem (see Sects. 5.2.4 and 7.7.1.2).

As opposed to Riemann's geometric approach to complex functions, Kronecker and Weierstrass emphasised the basic importance of power series and analytic continuation. Admittedly, even Riemann had referred to this method in various places [see, e.g., his (1857b, 88)] but, in Kronecker's and Weierstrass's opinion, nowhere had he treated it with the necessary rigour. "We always suppose", Kronecker remarked sceptically, "that a function can be continued to any part of

²⁷Quoted in Neuenschwander (1978b, 72). In the same letter Weierstrass ventured a rare political statement: "The happy development of science in your country can nowhere be followed with more interest than in Northern Germany. You should also be convinced that nowhere has Italy more sincere and disinterested friends. Happily, we are ready to extend to science the alliance between you and us that has been so successful in politics, in order that even in this field more and more barriers may be knocked down, which an unfortunate politics has erected for such a long time between two peoples who are in many ways spiritually related".

the plane where the variable must go (Briot and Bouquet, Cauchy, ...), that is, that we can always give it a path that avoids the critical points, as if such points could not quite disrupt the connection between the parts of the plane” (quoted in [Bottazzini 1986](#), 263).

The reference to Briot and Bouquet’s book is worth emphasising, because a German translation of it had just appeared in 1862. As Betti had done, even Weierstrass read this book “with the greatest care”, and realised that the theorems stated there needed a careful revision from the point of view of rigour. As for analytic continuation, Weierstrass told Casorati that Riemann seemed to share the widespread idea, as shown in his paper on Abelian functions. “Riemann is a little more scrupulous”, Kronecker admitted, “but maintains too much silence on this, so that his disciples can be thrown into the said error” (in [Bottazzini 1986](#), 263).

The “error” in question was to think that it is always possible to continue a function to any point of the complex plane along a suitable path that avoids critical points (branch-points and poles). According to Casorati’s report, this belief seems to have been shared even by Weierstrass at some early stage of his mathematical research—earlier than 1842, one would say in the light of his (1842). “But this is not possible”, Casorati recorded Weierstrass as saying, “It was precisely while searching for the proof of the general possibility that he realised it was in general impossible”. Kronecker provided Casorati with an excellent example of such an impossibility, given by the function

$$\theta_0(q) = 1 + 2 \sum_{n=1} q^{n^2}. \quad (6.55)$$

This series is one of the first examples of what was later called a gap series, which have the circumference of their disc of convergence as a natural boundary. In fact, the function $\theta_0(q)$ cannot be analytically continued outside of the disk $|q| < 1$. Kronecker had discovered this example in 1863 and had communicated it to Weierstrass, as recorded by Casorati in his notes and, later, by [Schwarz \(1872b, 241\)](#).²⁸

The series (6.55) had already been used by Jacobi (*Fundamenta nova*, Sect. 65) in his proof of Fermat’s theorem on the representation of a number as a sum of four squares. As we have seen, following Jacobi’s approach to elliptic functions, theta series and their applications became a widely studied subject. Their connections with “pathological” functions had been made evident by Riemann who found the examples he presented in his Habilitationsschrift by considering the limit cases of certain theta series.

In order to understand the behaviour of the function (6.55) outside the circle $|q| = 1$, Kronecker observed that “it is necessary to resort to other means, and not to those of making q follow a path” joining a point within the disk with a point outside it. On another occasion Weierstrass repeated once more his former belief that points

²⁸On the basis of three fragments on elliptic modular functions found in Gauss’s *Nachlass*, Fricke claimed that Gauss had been aware of the existence of natural boundaries (see Gauss, *Werke* 8, 103–104).

at which a function “ceases to be definite [...] could not form a continuum, and consequently that there is at least one point P where one can always pass from one closed portion of the plane to any other point of it. But he later realised that this was not the case”.

Casorati also recorded that Weierstrass “found the theorem that every function (monodromic is to be understood) that does not have points at which it ceases to be definite is necessarily rational. (The function $e^{1/x}$ is not defined at the point $x = 0$ because it can have any possible value)”, thus apparently repeating what (Briot and Bouquet 1859, 38) had mistakenly stated. This sloppy and imprecise way of speaking on Weierstrass's lips is rather puzzling. Should one argue that at that time Weierstrass had no clear idea yet of essential singularities, and of the behaviour of a function in the neighbourhood of such a singularity? Or should it be attributed to the character of Casorati's notes where every night he recorded for his personal use the essential content of the talks he had during the day? The same applies to the following statement where Weierstrass commented on the surprising behaviour of the function $\theta_0(q)$. “There is another expression for it”—Weierstrass told Casorati—“which also has meaning outside of the circle, that is, inside and outside but not on the circumference. And yet this function satisfies a differential equation. A circumference of this type is therefore entirely made of points where the function is not defined, it can take any value there”.

In this rather cryptic way Weierstrass announced a result he was to publish only in 1880 (see Sect. 6.9). This was intimately connected with the example of a continuous nowhere differentiable function that he was to publish some years later, possibly as a reaction to a suggestion by Schwarz (see Sect. 6.5.5). The significant role of the series (6.55) in Weierstrass's counter-example could perhaps explain Kronecker's claim about “pathological” functions that puzzled Casorati.

6.4.2 Algebraic Foundation of Function Theory

As Casorati's notes suggest, by the early 1860s, and in response to Riemann's achievements, Weierstrass began to build his theory of analytic functions in a systematic way on arithmetical foundations, and to present it in his lectures. Weierstrass's approach is summarised in a passage from a letter he sent to Schwarz on October 3, 1875—a sort of “confession of faith” made to his former pupil—which has been often quoted but is still worth reading in full:

The more I think about the principles of function theory—and I do it incessantly—the more I am convinced that this must be built on the foundations of *algebraic truths* [our emphasis], and that it is consequently not correct when the “transcendental”, to express myself briefly, is taken as the basis of simple and fundamental algebraic propositions. This view seems so attractive at first sight, in that through it Riemann was able to discover so many of the most important properties of algebraic functions. (It is self-evident that, as long as he is working, the researcher must be allowed to follow every path he wishes; it is only a matter of systematic foundations.) (in *Math. Werke* 2, 235).

Thus, the “master of analysis” turned out to be an algebraist!

A closer look at Weierstrass's private "confession" to Schwarz reveals an algebraic way of thinking which could have actually been subscribed to by Kronecker. As is shown by Casorati's notes, in the 1860s Kronecker and Weierstrass shared the same criticisms of Riemann's methods, the same distrust of geometrical intuition, and the same point of view about rigour and foundations. Casorati also recorded that once Kronecker told him that he had learned "to be more exact by cultivating algebra and the theory of numbers. In the use of infinitesimal analysis he had not found the occasion to acquire this exactitude".

The influence Kronecker exerted on Weierstrass's way of thinking in his early years in Berlin seems to have been underestimated by historians. It is likely to have been greater than is commonly thought, and certainly greater than Weierstrass himself was ready to admit in his later days, when they came to a complete disagreement about the foundations of the real numbers and analysis. It may also have played a role in turning Weierstrass's work to the search for an algebraic foundation of analysis. Even Kronecker's criticism of Dirichlet's principle, alluded to in his conversations with Casorati (see also Sect. 6.5.4), was eventually overshadowed by Weierstrass's counter-example, and became forgotten. But it did not escape more attentive contemporaries such as Heine, who in his (1870, 360) explicitly referred to the criticisms of both Kronecker and Weierstrass.

Be that as it may, Weierstrass's "confession of faith" was not just a matter of taste. There was a deep mathematical reason, which in his eyes fully justified his "faith" in an algebraic foundation of analytic function theory. As he explained to Schwarz in the very same letter, he had been "especially strengthened [in this belief] by his continuous study of the theory of analytic functions of several variables" which was required to build the theory of Abelian functions. At that time there was no way to deal with functions of several complex variables by resorting to "transcendental" methods as Cauchy and Riemann had done for functions of *one* variable. It is therefore quite significant that Weierstrass's lectures on analytic function theory culminated with the elements of a theory of functions of several variables.

6.5 The Theory of Analytic Functions

When Weierstrass began to expound his "programme" at Berlin university, an increasing number of students crowded into his lectures, coming to Berlin from Germany and abroad. The recorded figures are impressive (and also very different). Thus, for instance, when writing to his former student Koenigsberger on October 25, 1870 Weierstrass complained that the audience for his lectures on elliptic functions had reduced in number from 50 in 1869 to 20 because of the Franco-Prussian war (see Mittag-Leffler 1902a, 132). Kiepert, however, in his "personal reminiscences" of Weierstrass's 1869 course on Abelian functions (Kiepert 1926, 59), stated that 107 listeners attended Weierstrass's lectures at the beginning of the course, but their number quickly decreased to seven and of these seven only a few gave the impression that they could follow the lectures. Mittag-Leffler, who attended

Kronecker's and Weierstrass's courses in 1875, reported in a letter to Hermite on June 21 at the end of the lectures: "Something that surprises me very much is how Mr. Weierstrass and Mr. Kronecker could find so many listeners—between 15 and 20—for such difficult and advanced courses" (in [Dugac 1984](#), 234–235).

Apparently, the number of listeners was quickly to increase. Some years later, in April 1878 Weierstrass wrote to Sonya Kovalevskaya that he had 102 listeners. According to Kneser, in 1881 this number had increased to two hundred. Kneser impishly reported that Kronecker kept lecturing for his "circle of initiates" in room 17 at Berlin University, while Weierstrass had to move because room 17 had become too small for his (and Kummer's) audiences.

In a letter to Mittag-Leffler of January 8, 1881 Kovalevskaya said that Weierstrass had up to 250 students. Hermite, to whom Mittag-Leffler communicated this, replied on 13 February 1881 that he was "amazed by the extraordinary success" of Weierstrass's teaching. Darboux, he said, had explained such an "astonishing" number of listeners as a consequence of the war and the subsequent centralisation in Berlin. Then, on his part, after regretting such "an exaggerated centralisation that has done us increasing harm in every respect" Hermite went on to say that the gathering of students from Europe and all over the world to attend Weierstrass's lectures was "the legitimate success of [Weierstrass's] genius coupled with assiduous work" (in [Dugac 1984](#), 101).

Even more astonishing is the report by the Russian mathematician M.A. Tikhomandritskii, who followed Weierstrass's lectures in 1884. According to him, the number of students attending the first lecture was so high that Weierstrass had to move to the amphitheatre of the chemical Institute, which could contain more than one thousand people who, however, reduced to less than 150 listeners after a few lectures (quoted in [Dugac 1973](#), 61–62).

One wonders about these figures. There is evidence, for instance, that Riemann had only a handful of students in Göttingen. Around 1850 Dirichlet lectured in Berlin to about 30 students. Should we conclude that in 30 years the students in mathematics alone at the university of Berlin increased to more than eight times the number they had been in 1850? Or should we conclude that Weierstrass's lectures were so popular they attracted students coming from other faculties?

Be that as it may, given the unquestionable success of Weierstrass's lectures one wonders about his style of teaching. A lively recollection of it is provided by Runge, who, after three semesters in Munich, went to Berlin in 1877 with his friends Hurwitz and Max Planck to attend the lectures of the great mathematician. In WS 1877/78 Weierstrass had planned to lecture on Abelian functions. Runge, who asked Weierstrass for advice, was discouraged from attending the lectures and advised instead to begin in the subsequent term with the new cycle of lectures. Weierstrass was right, Runge recognised. Given the lack of publications about Weierstrass's own foundation of the theory of functions, he could hardly have followed the lectures on Abelian functions.

[Runge \(1926, 176\)](#) recorded that Weierstrass used to build the whole edifice of his mathematics from the foundations "without gaps" and without assuming anything that he himself had not proven. Apparently, Weierstrass used to ask a

listener to write at the blackboard. Sometimes a proof would escape him, and then he “improvised something that came to his mind during the lecture” before clarifying all that completely next lecture. He would say that all ought to be expressed in an “arithmetical tongue”, but sometimes I had the feeling, Runge added, that Weierstrass “secretely clarified a matter to himself by resorting to a figure but, like Gauss, the trails of such an intuitive aid were accurately effaced”. Moreover, “his lecturing, even though less accurately prepared, was more exciting than Kirchhoff’s”. Runge added that he had talked with Weierstrass about things that he did not treat in lectures, such as Cauchy’s integral theorem or Cauchy’s theory of residues, and he concluded that Weierstrass had perhaps a greater influence than Riemann, even though the latter surpassed the former “in the depth, the richness and the fertility of his ideas” (Runge 1926, 178).

The lecture notes of Weierstrass’s introduction to analytic function theory provide first-hand evidence of the development of this theory from the mid-1860s onwards. After Weierstrass resumed his teaching activity in 1863 and up to WS 1884/85 Weierstrass lectured many times on the introduction to analytic function theory. Even the courses held in SS 1857 and in WS 1858/59 on “General theorems concerning the representation of analytic function by means of infinite series” can be listed among his early lectures on the subject. In order to give the reader an account of it at various stages of Weierstrass’s teaching, the following different sets of lectures will be particularly discussed here:

1. SS 1868, lecture notes by Killing;
2. SS 1874, lecture notes by Hettner;
3. SS 1878, lecture notes by Hurwitz [the first 10 chapters summarised and published as an essay (*Saggio*) by Pincherle²⁹ in 1880].
4. SS 1886, lecture notes by various students—this being properly a lecture on “Selected Chapters from the Function Theory”, but in fact Weierstrass’s last lecture course on function theory.

According to Mittag-Leffler, there is no surviving edition of Weierstrass’s WS 1884/85 lectures on the introduction to analytic functions.³⁰ After that course,

²⁹After graduating from the Scuola Normale Superiore in Pisa as a student of Betti’s, Salvatore Pincherle obtained a professorship at the higher secondary school (*liceo*) in Pavia, where he got in touch with Casorati. On the latter’s advice, Pincherle spent the academic year 1877/78 in Berlin, where he attended Kronecker’s and Weierstrass’s lectures. Back in Italy, he published the *Saggio* that offered the first presentation in Italy of Weierstrass’s theory of analytic functions, and became in some respect the Weierstrassian counterpart of Casorati’s textbook (1868a). In 1881 Pincherle got a chair at the University of Bologna where he taught until his retirement in 1928. As an analyst, he had a pioneering role in the early development of functional analysis. In 1922 he was the founding President of the Unione Matematica Italiana, and from 1924 to 1928 he served as the President of the International Mathematica Union. In this capacity he organised and chaired the ICM in 1928 in Bologna, the first after World War I that was open to all mathematicians irrespective of nationality, thus terminating the discrimination against the mathematicians of the Central Powers. (For Pincherle’s role in the IMU, see Lehto (1998, 36–50).)

³⁰However, Ullrich (1989, 148) lists two sets of lectures of that course.

Weierstrass lectured on the “theory and applications of bilinear, quadratic forms” in Winter 1886–1887, and on the “theory of hyperelliptic functions” in summer 1887 before giving his last lecture, “The calculus of variations”, in winter 1889–1890 when he was 75.

6.5.1 *The Concept of Number*

Weierstrass’s lectures on analytic function theory always began with the introduction of the fundamental concepts of arithmetic. The oldest record of Weierstrass’s lectures on the subject is given by E. Kossak’s booklet *Die Elemente der Arithmetik* (1872) which is also the first published work inspired by Weierstrass’s lectures.³¹ Kossak had followed Weierstrass’s WS 1865/66 lectures on the “General theory of analytic functions”, but his work included only the first part of those lectures devoted to the elements of arithmetic. The booklet was completed by Kossak’s own contributions to the history of the notion of number. In the Weierstrassian milieu Kossak’s work was not much liked. Schwarz, who knew Kossak and “expected more from him”, criticised it strongly in a letter to Weierstrass of June 20th June 1872, saying that “the problem which the author had to solve consisted exclusively in a careful and correct presentation of the thoughts you have expounded in that lectures; in this respect it seems to me that Kossak’s presentation cannot satisfy even the most modest demand”. There is no record of Weierstrass’s reply. More than 20 years later, however, writing to Mittag-Leffler on April 5, 1885 Weierstrass confirmed his disappointment with Kossak’s introduction to his theory of functions.

Even though Kossak claimed he had basically used “Weierstrass’s own words” in preparing his book—and this is essentially correct in the light of other versions of Weierstrass’s lectures—Hettner’s and Hurwitz’s lecture notes (see below) provide a closer and more faithful view of Weierstrass’s introduction to the systems of numbers than does Kossak’s. As we discuss below (see Sect. 10.4), things were far even worse with Otto Biermann’s *Theorie der analytischen Funktionen* (1887). Weierstrass’s judgement was trenchant: “I cannot acknowledge this book as a faithful reproduction of my lectures”.³²

According to Hettner, Weierstrass complained of the bad experiences he had had “in previous years” where the theorems he presented in his lectures had been either left out or reported incorrectly. Therefore, he had decided he would always present an introduction to the basic arithmetical concepts. To be sure, a great deal of the algebraic treatment of the fundamental concepts of arithmetic which Weierstrass

³¹This was published as a *Program-Abhandlung* of the Friedrichs-Werder Gymnasium in Berlin.

³²Weierstrass letter to Schwarz on 12 June 1888. In his letter on 17 June Schwarz reported that according one of his colleagues in Göttingen Biermann’s book was “anything one wishes but Weierstrass’s theory of functions”.

presented in his lectures is missing from both Kossak's and Biermann's books. In the case of Biermann's, this coupled with a number of inaccuracies and mistakes.

Each year the part Weierstrass devoted to the arithmetic of the natural and the complex numbers amounted to between one fifth and a quarter of the whole lecture course.³³ More than anything else this reveals the care Weierstrass put in to establishing the arithmetical foundation of his theory. As Hettner reported, Weierstrass maintained that "the main difficulties of higher analysis are due indeed to a unrigorous and insufficiently comprehensive presentation of the fundamental arithmetical concepts and operations".

Weierstrass's first step was the introduction of the number concept. According to [Kopfermann \(1966, 77\)](#), his treatment of the natural numbers had "an almost mystic character". The construction is vague and incomplete. By comparison with Dedekind's and Peano's axiomatic theories, which came out in the late 1880s, as well as Frege's contemporary work on the foundations of arithmetic, Weierstrass's concept of a whole number was simply loose and naïve.

Apparently, according to Weierstrass, natural numbers are "out there". In his words: "We can figure out every thing in the external world we can represent to ourselves as repeated, and only separated from each other by space and time, and so from *one* magnitude build new ones determined by it" (Weierstrass 1968, 1). Thus a magnitude (*Größe*) a can be thought of as a multiple of a magnitude b when a can be considered as the union of several parts, all of them having the same characters in common with b and being separated from it only by space and time.

In Kossak's booklet the concept of number is introduced by considering elements of the same kind (independently of their nature) and "thus one has a representation of this set by means of its number" (see Dugac 1873, 68). Almost the same conception is stated in Hettner's 1874 lecture notes: "a natural number is the representation of the union of things of the same kind", whereas in Hurwitz's 1878 notes things are a little more sophisticated: the concept of a number arises "through the reunion in the mind of things for which one has discovered a common token, especially of things which are identical in thought".

Not surprisingly, this psychological foundation of the number concept drew sharp criticism from Frege, most notably at various places in his *Die Grundlagen der Arithmetik* ([Frege 1884](#)).³⁴ In his "Overview of general arithmetic" presented in his 1886 lectures, [Weierstrass \(1988b, 37\)](#) began by stating that "although the concept of number is extremely simple" it is not easy "to give a textbook definition of it".

³³Some 16 of 108 (printed) pages in Killing's lecture notes, 135 of Hettner's 707 manuscript pages of the 1874 lectures, 44 of 166 (printed) pages in Hurwitz's 1878 lectures (which corresponds to 120 of 369 pages in Rudio's manuscript of the same year, and about one half of Pincherle's *Saggio*.)

³⁴[Dugac \(1973, 83–84\)](#), defended Weierstrass's approach against Frege's criticism, which he said was based mainly on Biermann's book. But this cannot be right: Frege's book predates Biermann's by three years, and if Dugac was referring to the later criticisms in Frege's *Grundgesetze* II (1903), the criticism is misplaced, because Frege explicitly based his picture of Weierstrass there on Kossak, Biermann and a set of notes from Weierstrass's lectures. We thank James Tappenden for pointing this out to us.

Once more he resorted to the claim that the “human mind” has first of all the ability to “build a representation of external and internal objects” then of keeping this representation fixed as well, and then of reproducing this image repeatedly. This is enough to generate the idea of an “unknown” (*unbekannte*) unit. Once this is done, we can easily get the idea of a (natural) number as a multiple of the unit.

By and large, the absolute rigour Weierstrass was looking for—and in fact, all of his arithmetised processes of analysis—was eventually based on a concept of natural number that was as “sophisticated” as the one attributed to Kronecker in the famous joke about the natural numbers being conceived of as a gracious present from God.

6.5.2 Complex and Hypercomplex Numbers

Having introduced natural numbers this way, Weierstrass’s next step was to make a distinction between natural (or ordinary) and complex numbers with respect to the “units” involved in their definition. “By a complex number we understand a set (*Aggregat*) of numbers of different units. We call these different units the elements of the complex number” (Weierstrass 1988a, 3). Needless to say that this view and terminology, inspired by Gauss (1832), are quite different from the usual one nowadays.

Next Weierstrass introduced the concept of “proper part” of a unit a as a new unit whose a -fold multiple produces the “old” unit. Thus, he stated, by a numerical magnitude (*Zahlgrösse*) we mean every complex number whose elements are the unit and its (possibly infinitely many) “proper parts” (Weierstrass 1988a, 4). In this way he introduced the rational and irrational numbers and how to calculate with them. As this theory is not of our primary concern, we refer to Dugac (1973) for a detailed discussion of it, but we note that Weierstrass’s account of the real numbers is based on the idea of a *known* law that specifies the proper parts, and so strictly speaking it is constructive.

According to Dugac (1973, 57), Weierstrass is likely to have elaborated his theory of real numbers during 1863, and presented it in his WS 1863/64 lectures. However, the oldest record of his lecturing this subject is given by his WS 1865/66 lecture course on analytic function theory, which was attended by Moritz Pasch, and taken by Kossak as a basis for his 1872 booklet. In spite of Schwarz’s and Weierstrass’s dislike of it, Dugac (1973, 69) has remarked that in its essential parts Kossak’s definition of irrational numbers is “perfectly rigorous”. More refined versions can be found in Hettner’s lectures notes, as well as in Hurwitz’s and in Pincherle’s *Saggio*.

Having introduced irrational numbers via their expansions in infinite series, Weierstrass turned to complex numbers. As recorded in Hurwitz’s manuscript, Weierstrass (1988a, 25–39) did this in two ways. First he introduced complex whole numbers in a geometric manner as Gauss (1832) had done. Given two (oriented) straight lines r and t , say, in the plane that meet at a point A , the units e and e' are represented by (congruent) segments AB, AB' taken on one straight line, say r , that lie on opposite sides with respect to A , while i, i' are represented by (congruent)

segments AC, AC' on t lying on opposite sides with respect to A . Then, for the sake of convenience, Weierstrass considered orthogonal straight lines r and t (*Hauptaxen*), and congruent segments AB and AC as representing the units e and i . Accordingly, given a point A (the origin) in a plane, a complex number composed of (integer) multiples of the units e, i, e', i' is represented by a point of the plane. Equality, addition and subtraction of such numbers are easily defined by resorting to their representations as vectors in the plane. This can be obviously extended to complex numbers built with the four units and their “proper parts”.

As for the multiplication of complex numbers, first Weierstrass (1988a, 29) gave the following table for the units e, i, e', i'

$$\begin{aligned} ee &= e & ie &= i & e'e &= e' & i'e &= i' \\ ei &= i & ii &= e' & e'i &= i' & i'i &= e \\ ee' &= e' & ie' &= i' & e'e' &= e & i'e' &= i \\ ei' &= i' & ii' &= e & e'i' &= i & i'i' &= e' \end{aligned}$$

and observed that commutativity holds for any two units, and in general for arbitrarily many of their “proper parts”. The next step was to define the multiplication of any two numbers in such a way that the standard properties defined by

(I) $ab = ba$, (II) $a(b + c) = ab + ac$, (III) $(ab)c = (ac)b$ for any a, b, c hold and, as a consequence of (I) and (II), also

$$\begin{aligned} (a + b + c + d + \dots)(a_1 + b_1 + c_1 + d_1 + \dots) = \\ aa_1 + ab_1 + ac_1 + ad_1 + \dots + ba_1 + bb_1 + bc_1 + bd_1 + \dots + \dots \end{aligned}$$

As a consequence of the latter, the product of any two numbers reduced to the addition of products of any two “units”. Accordingly, one had to define the products of two units. These are completely determined once the following conditions are satisfied:

$$ee = e, ei = i, ii = e'.$$

from which one gets the table above as a consequence of rules (I) and (II), as Weierstrass verified in detail.³⁵ The division of two complex numbers can be defined by the equation $(a : b) \cdot b = a$, and both multiplication and division can be interpreted geometrically.

Weierstrass (1988a, 31) then introduced complex numbers “in a purely analytical way, without any reference to their geometrical meaning”. To this end he built an (abstract) algebraic structure on a two-dimensional real vector space in great detail, and eventually showed that the (ordinary) field of complex numbers is obtained by giving the units the values $e = 1, e = -1$ (and accordingly, $ii = -1, 1/i = -i$). In

³⁵Weierstrass’s reasoning amounts to proving the theorem that, if in a system of complex numbers with two units and no nilpotent elements the associative and distributive properties hold, then the commutative property also holds.

modern terms, he proved that up to isomorphism the field of complex numbers is the only two-dimensional associative and commutative real algebra with no nilpotent elements.

However, by that time the theorem had already been published independently by Hankel in his book *Theorie der complexen Zahlensysteme* (1867) where it is stated as follows: “A higher complex number system, whose formal laws of calculation are determined by the conditions of §28 [i.e. the system is a commutative ring with a unit, which is a finite dimensional vector space over $\mathbb{R}$] and whose products of units are in particular linear functions of the original units, and in which no product can vanish unless one of its factor be zero, contains a contradiction in itself and *cannot exist*” (Hankel 1867, 107).

According to Hankel (1867, 107), his theorem answered the question raised by Gauss (1831, 178) whose solution he had promised but never given: “why the relations among things that represent a manifold of more than two dimensions cannot yield other admissible kinds of magnitudes in general arithmetic”. (As we will see shortly below, in the 1880s this somewhat obscure passage was interpreted differently by Weierstrass and Dedekind.)

After his book was published, Hankel wrote a letter to Grassmann on September 4, 1867 in which he said that he recently received a set of Weierstrass’s lecture notes from Schwarz that “deals *ex-professo* with complex numbers”. In Hankel’s view, Weierstrass went “not very deep, or rather not very far”, and limited himself to dealing with a precise and accurate construction of the elementary concepts of real and complex numbers. Nonetheless, Hankel added, he was pleased to have found an almost complete coincidence between his own and Weierstrass’s point of view, a fact that he took as “a guarantee” that this theory will soon find its appropriate place in the organism of mathematics.³⁶

As for Weierstrass, in the concluding remarks to the section on complex numbers of his 1878 lectures he stated: “If one were to consider complex numbers with arbitrarily many units, then one would find that calculations with such numbers can always be reduced to calculations with numbers built by four units only” (Weierstrass 1988a, 39). In modern terms, this amounts to saying that every finite dimensional, associative and commutative real algebra with a unit and no nilpotent elements is (isomorphic to) a ring-direct sum of copies of $\mathbb{R}$ and $\mathbb{C}$, i.e. the theorem known as Weierstrass–Dedekind theorem.

Eventually, the section on complex numbers comes to an end with theorems on infinite products and series of such numbers, including the results on absolute convergence already established by Weierstrass in his 1856 paper on analytical powers (see Sect. 6.2.4).

According to Kopfermann (1966, 83–84), Weierstrass had lectured on complex numbers and “complex numbers with more than two units” at the Mathematics Seminar in summer 1872. Stated in modern terms, he there proved “in a constructive manner” that the complex numbers are the only commutative division algebra of

³⁶Hankel’s letter is quoted in Grassmann (1894–1911, 3.2, 277).

rank 2 over $\mathbb{R}$. In the same seminar he also proved the Weierstrass–Dedekind theorem.³⁷

Weierstrass took up the subject of hyper-complex numbers in lectures at the Mathematics Seminar during the WS 1882/83, and again in 1883 in a letter to Schwarz published in the *Göttingen Nachrichten* (Weierstrass 1884). According to his own words, Weierstrass (1884, 396) had tackled this subject for the first time as early as his WS 1861/62 course in order to answer the question raised by Gauss in 1831. However, there is no trace left of lectures notes of this course. Apparently Schwarz followed it, and later on (probably in 1877) he tried to reconstruct Weierstrass’s theory on the basis of lecture notes of Weierstrass’s 1863/64 and 1865/65 winter courses, but he was unable to do so. His attempts at reconstructing his teacher’s theory are preserved in a manuscript dated May 15th, 1877 and kept in the Schwarz *Nachlass* at the Berlin Akademie.

Schwarz resumed the matter in a letter to Weierstrass on May 9, 1883 where he reported to his old teacher about his visiting Paris. According to Schwarz’s own account, the troubling point was to prove that in each domain of hyper-complex numbers with units e_0 , the neutral element under multiplication, and $e_1, e_2, \dots, e_{n-1}$, there always exists an element x such that the units $e_1, e_2, \dots, e_{n-1}$ can be represented by means of $e_0, x, x^2, \dots, x^{n-1}$ i.e. the latter span the whole domain. “As a consequence of your communications in the relevant lectures I regarded it as correct”, wrote Schwarz “even though I did not succeed in proving it. I have already talked with you once about this point, but you had just raised other things that were in the foreground of your interest, and so a proper treatment of this special question was missing”. After meeting Stéphanos Cyparissos³⁸ in Paris Schwarz doubted the correctness of the statement above because Cyparissos had provided him with an example where the theorem seemed to fail. Cyparissos considered a three-dimensional system of hyper-complex numbers of the form $x = \alpha_0 e_0 + \alpha_1 e_1 + \alpha_2 e_2$ with $\alpha_i \in \mathbb{R}$. Let e_0 be the neutral element under multiplication, and $e_1 e_1 = 0, e_1 e_2 = e_2 e_1 = 0, e_2 e_2 = 0$ be the multiplication rules for e_1 and e_2 , then one has $x^2 - 2\alpha_0 x + \alpha_0^2 e_0 = 0$. Thus, e_0, x, x^2 are linearly dependent and “certainly e_1 and e_2 cannot be expressed by e_0, x, x^2 ”. Given this, “the question arises: which theorem should replace it?” Schwarz asked Weierstrass.

In his long reply—a letter that he began on May 19 and ended on June 27—Weierstrass (1884, 331) produced “an authentic account” of his theory because “the notes of even the best listeners contain something not properly understood—with or without it being the fault of the docent—namely where it is not purely a matter of reproducing calculations”. As for the problem raised by Cyparissos’s example, Weierstrass stated that he had become aware of it earlier as a result of his own study

³⁷A set of (undated) lectures given by Weierstrass at the Mathematics Seminar and dealing with the general case of the Weierstrass–Dedekind theorem is appended to Hettner’s manuscript of the SS 1874 lectures.

³⁸A Greek mathematician, Cyparissos studied in Paris and in 1884 he returned to Athens where he became a professor at the local University and the Technical High School.

of the noncommutative case provided, e.g. by Hamilton's quaternions. In particular, he emphasised that the "essential difference" between the ordinary arithmetic and the arithmetic of hyper-complex numbers was given by the existence of "divisors of zero". In Weierstrass's interpretation, Gauss's insight was founded on the fact that, when $n > 2$, the product of two magnitudes can vanish without either of its factors being equal to zero, and in the end he reduced the answer to Schwarz's question to this: Given an arbitrary element $x = \xi_1 e_1 + \xi_2 e_2 + \cdots + \xi_n e_n$ of the domain, can its powers x^ν be reduced to the form

$$x^\nu = \xi_1^{(\nu)} e_1 + \xi_2^{(\nu)} e_2 + \cdots + \xi_n^{(\nu)} e_n \quad (\nu = 1, 2, \dots, n).$$

The decisive requirement that answered Schwarz's question was that the determinant $|\xi_\mu^{(\nu)}|$, $(\mu, \nu = 1, 2, \dots, n)$ must not be identically zero.

Weierstrass's paper was followed by papers by [Dedekind \(1885, 1887\)](#) and by Julius Petersen in 1887. Dedekind related this topic to his previous work, appended as 10th supplement to his 1871 edition of Dirichlet's lectures on number theory. He rephrased Weierstrass's theory in his own terms and provided a slightly different interpretation of Gauss' 1831 passage that he expressed more explicitly in 1887 by conjecturing that Gauss's insight was connected to his work on the division of the circle and the related arithmetic of multi-valued quantities.³⁹

Commenting on this, Weierstrass wrote Schwarz on March 11th, 1887 that he found Dedekind's paper "extraordinarily weak". "I will not leave it without a reply—Weierstrass went on—for it shows that he has absolutely not understood my definition of a complex quantity". As for Dedekind's interpretation of Gauss's statement, needless to say that in Weierstrass's view it was "completely wrong". In spite of his claim, however, there is no published record of Weierstrass's promised reply.

More than one year later, Weierstrass referred to this topic once more in a letter to Schwarz on June 12, 1888. After sadly commenting at length on Biermann's "Jesuitic" behaviour (see Sect. 10.4), "for the moment"—Weierstrass concluded—he had to take care solely of his poor health, and thus to avoid getting angry, as had happened to him recently after reading Kronecker's remarks on Dirichlet's final works ([Kronecker 1888b](#)). Weierstrass construed Kronecker's paper as "an attack against me that had been prepared for a long time". In those remarks, and without naming Weierstrass, Kronecker sharply criticised the way in which a method of integrating the equations of celestial mechanics had been alluded to in the recent announcement in *Acta* of King Oscar's Prize. The method, it was suggested, was supposedly found by Dirichlet and "communicated to a friend, a mathematician" (actually, Kronecker), and using it Dirichlet had been able "to demonstrate the stability of our planetary system in an absolutely strict manner".⁴⁰ According to Kronecker, what it was stated in the announcement "in no way corresponded" to

³⁹As this subject is not of our primary concern, we refer the interested reader to [Lützen \(2001b\)](#) for a detailed account of Weierstrass's theory and Petersen's and Dedekind's work as well.

⁴⁰See *Acta* 7, I–VI.

Dirichlet's views and to what had really occurred.⁴¹ On the other hand, Weierstrass went on ironically, even Kronecker's work on complex numbers (Kronecker 1888a) "should show how he [Kronecker] has already given the true foundations of this theory in his *Festschrift* and that he is able to do so so much better than such feeble algebraists as Weierstrass, Dedekind and Petersen, while he [Kronecker] has done nothing more than darkening and making difficult a thing that is very simple to deal with".

6.5.3 The Bolzano–Weierstrass Theorem

According to Pincherle's *Saggio*, in Weierstrass's lectures the development of analytic function theory proper was preceded by "some theorems on magnitudes in general" and some general remarks on the concept of function. This holds true also for both Hettner's 1874 and Hurwitz's 1878 manuscripts, but while the remarks on the concept of function are presented in a note appended to Hettner's 1874 lecture notes, in Hurwitz's 1878 manuscript the historical development of the concept of function is followed by chapters on rational functions, power series and the differential calculus, and eventually by the study of what in modern terms are called the topological properties of $\mathbb{R}$ and $\mathbb{R}^n$. There Weierstrass (1988a, 83–92) introduced such concepts as the δ -neighbourhood of a point of $\mathbb{R}^n$, the definition an open set—as we would denote what he called "a continuum"—and the definition of a path-connected domain as well. Then he stated and proved, in a way that has since become standard, such fundamental theorems as the Bolzano–Weierstrass theorem in $\mathbb{R}$ and $\mathbb{R}^n$, the existence of the upper (resp. lower) bound and the existence of (at least one) accumulation point for an infinite, bounded set of real numbers (including their extension to sets of $\mathbb{R}^n$ and $\mathbb{C}^n$).⁴² As a consequence, he proved that a continuous function on a closed interval is uniformly continuous, and attains its upper and lower bounds there. He extended this theorems to functions of n variables and stated that all the theorems he proved for domains of real variables "can be immediately transposed to domains of complex variables".

According to Schwarz, Weierstrass knew of Bolzano's theorem as early as 1870, or even before, and used it in his research. Actually, in spring 1870 the admissibility

⁴¹Kronecker's criticism is discussed in detail in Barrow–Green (1997, 59–61).

⁴²Bolzano's theorem, proved in (Bolzano 1817) states: "if a property M does not apply to *all* values of a variable quantity but does apply to *all* values *smaller* than a certain u , then there is always a quantity U which is the greatest of those of which it can be asserted that all smaller x possess the property M " (italics in the original). See the translation in (Russ 2004, 269). It is worth mentioning here that by the 1870s even Bolzano's name was largely unknown. Thus, for instance, in 1876 Darboux asked Hoüel "who is this Bolzano of whom you tell me about a theorem on the continuity of functions?" (quoted in Dugac 1973, 152). Even more significant was Sylow's confession in 1902 that he could not understand the remark "Bolzano is a clever man" in Abel's manuscripts because "[he] only knew of Bolzano as the name of a town" (Sylow 1902, 13).

of Weierstrass's methods of proving this and related theorems was the subject of a lively discussion among the mathematicians belonging in the Berlin milieu. This mainly concerned Cantor's early work on trigonometric series, and his proof of a uniqueness theorem giving conditions for the representation of a function by such series.⁴³

Cantor encountered difficulties and on February 17th turned for help his friend Schwarz, who was by then a professor at the Eidgenössische Polytechnische Schule in Zurich.⁴⁴ Schwarz was able to establish a crucial lemma.⁴⁵ On February 27 Schwarz sent a proof of his lemma to Heine for advice. The latter took his time to become acquainted with the former's methods, and eventually on March 8 he answered Schwarz: "I do not deny that your proof of the theorem using the Bolzano–Weierstrass principle does *not* seem to me completely demonstrative, and therefore I cannot admit that the theorem is settled. Cantor tells me that he has sent his research—including, I believe, a communication of your proof of the theorem—to Weierstrass" (quoted in German in Dauben 1979, 308). In fact, as Cantor (1870b, 82) recognised in the published paper, Schwarz's proof was essentially based on the following theorem that was frequently cited and proved in Weierstrass's lectures: A function $f(x)$ that is continuous on a closed interval $[a, b]$ attains its upper bound g , so there is at least one value x_0 such that $f(x_0) = g$.

The matter seems to have occasioned lively discussions in Berlin, for in a letter to Cantor on March 26, 1870 Schwarz reported that he had recently received a letter from Kronecker where the latter stated that "Bolzano's conclusions are evidently false arguments", adding that Kummer, Borchardt, and Heine all shared this opinion. In his reply to Schwarz on March 30 Cantor remarked that "Herr Kronecker stands in any case in contradiction with the Weierstrass–Bolzano theorem of the lower and upper bound", and added that he himself regarded this theorem "not only as correct but also for the foundation of the most important mathematical truths", and his friend Thomé shared his view completely. "You are right—Cantor concluded—when talking of the luck we have to be able name Weierstrass as our teacher; I agree with all my heart". In turn, on April 1st Schwarz expressed his delight to Cantor at the success of Weierstrass's approach in spite of Kronecker's criticism, and concluded: "I'm pleased that you, Thomé and myself are on Weierstrass's side".⁴⁶

Apparently, discussions on this and related topics went on for quite some time in Berlin, for after his visit there Heine wrote to Schwarz on May 26, 1870 that Kronecker and Weierstrass were very often together, and they talked very much about Cantor's and his own (Heine 1870) proofs without coming to a true understanding.

⁴³See Dauben (1979, 30–36).

⁴⁴The school had opened in 1855. It was renamed the Eidgenössische Technische Hochschule in 1911.

⁴⁵Schwarz's original proof was eventually published in his *Ges. Math. Abh.* 2, 341–343.

⁴⁶See Meschkowski (1967, 68 and 228–229) and Meschkowski and Nilson (1991, 24–25).

Almost a year later, writing to Ulisse Dini⁴⁷ on March 3rd, 1871 Schwarz stated that the Bolzano–Weierstrass theorem and its proof played a major role in Weierstrass’s “method of proof”, being indeed the first step of it, the second one being the extension of this theorem to several variables. The third step consisted in Weierstrass’s peculiar ability “to draw conclusions from special properties of a function”. As an application Schwarz added Weierstrass’s proof of the theorem that, stated in modern terms, says: a continuous function on a compact set is uniformly continuous there. In his concluding remarks Schwarz did not fail to mention that Kronecker raised an “objection” to Weierstrass’s “method of proof” on the grounds that “certainly there is no method for actually carrying out in a given case the process that the proof procedure demands, for example numerically”.⁴⁸

By that time Dini had just published a paper on complex function theory (Dini 1870–1871) where, following Riemann’s approach, he weakened the assumptions of Laurent’s theorem by producing the power series expansion of an analytic function $w = u + iv$ defined on an annulus, or on the annular region between two confocal ellipses, on the sole condition that the values of u are given on the boundary and the value of v at any interior point of the domain. In a footnote Dini (1870–1871, 245) added that he was well aware of “various objections” made against Riemann’s proof of the relevant existence theorem,⁴⁹ and in his work he assumed the existence of u and that its first derivatives $\partial u / \partial p$ was bounded even on the boundary. Dini sent an offprint of the paper to Schwarz who in turn in his answer on February 5, 1871 objected that “the restriction to bounded $\partial u / \partial p$ is “not necessarily founded in the nature of the thing”.⁵⁰

“This circumstance—Schwarz went on—whose knowledge I owe to the lectures of Herr Weierstrass, led me to follow another route in this research” thus referring to a series of his papers (Schwarz 1869a, 1870b, c). Then Schwarz entered a thorough discussion of the concept of a continuous function of two variables involved in his use⁵¹ of the Poisson integral

⁴⁷A pupil of Betti, Dini was one of the outstanding Italian analysts of the time, and his book on the foundations of real analysis (Dini 1878) played a pioneering role in spreading Weierstrass’s and Cantor’s ideas. After graduating from the Scuola Normale in Pisa in 1864, Dini obtained a scholarship to visit Paris, where he got in touch with Bonnet and Bertrand, and Berlin. After his return to Pisa, in 1866 he began his teaching activity at the Scuola Normale that lasted for more than fifty years. In the early 1870s he was elected to the Pisa City Council, and from 1880 he ceased doing original research in mathematics in order to devote himself completely to political and organisational activity as a member of the Italian Parliament.

⁴⁸Quoted in Bottazzini (1992b, 79–80).

⁴⁹See Riemann (1851, Sect. 19).

⁵⁰All the more because, as Schwarz (1872a, 176) was to remark, if the function u is subject to the sole condition of being continuous on the boundary, one cannot deduce from this that on the boundary u has a bounded partial derivative $\partial u / \partial p$ for this may even not exist, as Weierstrass has pointed out in his lectures “for many years”.

⁵¹See Schwarz (1870c, 146).

$$u(r, \varphi) = \frac{1}{2\pi} \int_{-\pi}^{\pi} f(\psi) \frac{1-r^2}{1-2\cos(\psi-\varphi)+r^2} d\psi \quad (0 \leq r < 1). \quad (6.56)$$

There Schwarz referred to the definition given by Heine (1870, 361). One month later, on resuming the matter in his letter to Dini on March 3rd, 1871, he pointed out that “Herr Weierstrass at least has *always* worded the definition as precisely as Herr Heine”.⁵² (Italics in the original)

Schwarz not only felt it necessary to make explicit reference repeatedly to Weierstrass’s “method of proof” and related results in his letters, but also in his printed papers. He did this, for example, in his (1870c, 147), where he stated that in his lectures Weierstrass had produced a proof that a (non-constant) function of one or several variables, continuous in a given domain including its boundary, actually attains its upper bound at least at one point within or on the boundary of the domain. He referred to this theorem to prove that a harmonic function attains its extremal values on the boundary of its domain as Riemann (1851, Sect. 11) had claimed.

It is also worth mentioning that Schwarz (1870c, 151–152) observed there that if the function $u(1, \varphi) = f(\varphi)$ —obtained by putting $r = 1$ in (6.56)—“has at no point the character of an entire function” then the circumference $|z| = 1$ is a “natural boundary”⁵³ for the analytic function

$$F(z) = \frac{1}{2\pi} \int_0^{2\pi} f(\psi) \frac{e^{\psi i} + z}{e^{\psi i} - z} d\psi \quad |z| < 1,$$

whose real part is the function u as defined by (6.56).

On this occasion Schwarz added that “for some years” Weierstrass had called attention to the “important circumstance” in function theory that “the domain of the argument of an analytic function can not always be extended arbitrarily but, on the contrary, in many cases it is a determined, bounded one” (Schwarz (1870c, 152). This was almost a *verbatim* quote from (Weierstrass 1866a) where for the first time in print Weierstrass made an (admittedly cryptic) allusion to the existence of natural boundaries of analytic functions—“a circumstance” he said, “that does not seem to have been remarked upon so far, even though it is of great importance in function theory”.⁵⁴ Apparently he was unaware of the fact that Hermite had already noticed the same circumstance when dealing with elliptic functions in his *Note* (Hermite 1862) (see 7.3.2) translated into German in 1863. In his paper Schwarz presented

⁵²One year later Schwarz summarised the matter in a long footnote appended to his (1872a, 177–178), where he presented the counterexample to separate continuity given by $f(x, y) = \frac{2xy}{x^2+y^2}$ for which he referred to Thomae (1870b, 13–16).

⁵³This seems to be the first occurrence of such a term in print.

⁵⁴It is worth remarking that the relevant passage from the *Monatsberichte Berlin* (p. 617) is not included in the re-worked version of the paper published in the *Werke*. Weierstrass was to discuss the matter in detail many years later in Weierstrass (1880c). See Sect. 6.9.

the alternating method that we will discuss in detail below (see Sect. 7.7.1.2). “With this method”—Schwarz stated proudly when presenting it in a lecture on May 30, 1870—“all the theorems which Riemann has tried to prove in his papers by means of the Dirichlet principle, can be proved rigorously” (1870b, 133). He asked Weierstrass for advice, and this turned out to provide the latter with the occasion for communicating his criticism of Dirichlet principle to the Berlin Akademie, as we will see in the next section.

6.5.4 A Counterexample to Dirichlet’s Principle

Riemann’s theory of complex functions seems to stand in the background of Weierstrass’s work and lectures on complex function theory. Evidence of this is provided by his (still unpublished) correspondence with his former student Schwarz. This unique source of information amounts to some 250 letters, many of them very long, from 1867 up to 1893. One of the first topics they discussed was Riemann mapping theorem, whose proof rested on a suitable application of Dirichlet’s principle (see Sect. 5.2.4).

At Weierstrass’s suggestion, Schwarz tackled this question after his student days and succeeded in establishing the theorem in particular cases, without resorting to the questionable principle. In a letter to Schwarz on November 27th, 1868 Weierstrass expressed his complete agreement with the former’s solution of the problem of mapping the surface of a tetrahedron conformally onto the surface of a sphere (Schwarz 1869b).⁵⁵ Weierstrass also encouraged his young student to publish his “representation of an ellipse on a circle. As insignificant as this subject may *now* appear”, Weierstrass went on, “yet perhaps it is good to show, by just such an example, how productive it is when someone joins a good, analytical *special*-knowledge to the tools of pure geometry” (Italics in the original).

On Weierstrass’s advice, Schwarz arranged for this short note to be published in the *Annali* (translated into Italian), and in a letter to his teacher on July 14, 1870 he confessed that he was “very anxious” waiting for his judgement about the note. Schwarz also asked Weierstrass whether he had “objections to raise” to the larger paper on the integration of the equation $\Delta u = 0$ (Schwarz 1870c) that he had sent him.⁵⁶

Apparently, Weierstrass’s answer has been lost. It is quite significant, however, that three days later, on July 14, 1870 at a plenary session of the Akademie

⁵⁵Strictly speaking, the map is an onto map from the net of a tetrahedron in the plane to the sphere, and, as Schwarz said (1869b, 94), is conformal everywhere except at the vertices. It is given by a suitable Schwarz–Christoffel transformation.

⁵⁶As a matter of curiosity, it is worth mentioning that Schwarz also confessed there to have collected “little by little” all the papers that Riemann had published, to have partly transcribed them, and to plan of binding them together—a sort of personal edition of Riemann’s works, one could say.

Weierstrass read the paper⁵⁷ “Remarks on the so-called Dirichlet principle”, where he presented his celebrated counterexample, and then submitted Schwarz’s paper for publication in the *Monatsberichte* of the Akademie.

According to [Burkhardt and Meyer \(1900, 494\)](#) Weierstrass had raised doubts about the correctness of Dirichlet’s principle as early as “the end of the 1850s”. Doubts about the admissibility of the principle “in its Riemannian generality” were also raised by Kronecker in his talks with Casorati in Berlin in 1864, and repeated in the private communications they had in summer 1869 in Italy, as the latter reported in a letter to Battaglini on November 22, 1869 (quoted in [Neuenschwander 1978b](#), 28–29). A cryptic allusion in print to “well-founded objections” was made for the first time by [Schwarz \(1869a, 83\)](#). In his communication to the Akademie Weierstrass limited himself to stating that after Riemann’s use of Dirichlet principle “some doubts” have been raised against its admissibility that turned out to be “absolutely sound”.

To prove this claim, after quoting at length from the “accurate lectures notes” that Dedekind had taken and provided him of Dirichlet’s SS 1856 course on the forces acting according to Newton’s law, Weierstrass observed that one can only state that the expression

$$\int \left[\left(\frac{\partial u}{\partial x} \right)^2 + \left(\frac{\partial u}{\partial y} \right)^2 + \left(\frac{\partial u}{\partial z} \right)^2 \right] dt \quad (6.57)$$

has a lower bound that the integral (6.57) can approach as close as one wishes without necessarily attaining it. This made Dirichlet’s argument fail.

In order to make this claim “evident” Weierstrass produced the following example. Let J be the integral

$$J = \int_{-1}^{+1} x^2 (\varphi'(x))^2 dx, \quad (6.58)$$

where $\varphi(x)$ and $\varphi'(x)$ are continuous in the interval $(-1, 1)$ and such that $\varphi(-1) = a \neq b = \varphi(+1)$. He proved that the lower bound of J is 0 by supposing that

$$\varphi(x) = \frac{a+b}{2} + \frac{b-a}{2} \frac{\arctan x / \varepsilon}{\arctan 1 / \varepsilon},$$

where $a \neq b$ are arbitrary constants, and $\varepsilon (> 0)$ is arbitrarily small. Indeed, a straightforward calculation shows that

$$J < \frac{\varepsilon}{2} \frac{(b-a)^2}{\arctan 1 / \varepsilon}.$$

⁵⁷As recorded in the *Monatsberichte*, p. 575.

Yet 0 is not a minimum for J because in this case $\varphi(x)$ would be a constant, contrary to the hypothesis. “Clearly, Dirichlet’s way of arguing leads to a false result in the case here considered”, Weierstrass concluded (1870, 54). In addition, as we have seen, in 1871 Prym showed by an example that even for the Dirichlet integral a lower bound for (6.57) might not exist.⁵⁸

Weierstrass decided not to publish his counterexample until 1895 when it was included in vol. 2 of his *Werke*, and one might wonder about the importance he attributed to it. Hints of a tentative answer are provided by Klein’s later recollections. In a lecture on the significance of Riemann’s work in modern mathematics that Klein delivered on September 26, 1894 in Vienna, he mentioned Riemann’s use of Dirichlet’s principle and Weierstrass’s counterexample to it. “With this a large part of Riemann’s developments fails” Klein (1894–1895, 492) commented. On the other hand, Klein went on, one has to think that “Riemann originally derived the theorem itself from physical intuition, which here again proved its value as a heuristic principle, and only afterwards based it on the said method of proof in order to have a mathematical train of thought complete in itself”. And in a footnote appended to this passage in his *Ges. Math. Abh.* Klein added (Klein 1894–1895, 492): “I remember that Weierstrass once told me that Riemann had never laid any decisive value on finding his existence theorems by means of the ‘Dirichlet’s principle’. Therefore, even his [Weierstrass’s] critique of ‘Dirichlet’s principle’ would have not made any particular impression on him. In any case, the problem arose of proving the existence theorems in another way. Then Weierstrass ought have charged his special pupil Schwarz with this”.

On the other hand, in his lectures on the history of nineteenth-century mathematics Klein somewhat modified this statement by implicitly crediting Burkhardt and Meyer’s claim. Indeed, there he stated that Riemann knew of Weierstrass’s criticism and acknowledged its exactness but he replied, as Weierstrass once told him [Klein], that “he [Riemann] considered the Dirichlet principle only as a useful tool, which was to hand—nonetheless his existence theorems hold true” (Klein 1926–1927, 1, 264).

Be that as it may, even if it was unpublished the existence of Weierstrass’s counterexample soon became very well known in mathematical circles. As usual, Schwarz charged himself with heralding Weierstrass’s point of view in print (Schwarz 1870c, 1872a) and in letters to foreign mathematicians. Thus, for instance, when on August 15th, 1872 Casorati asked him to explain some points in those papers, Schwarz was pleased to answer on August 20th with a detailed exposition of his own results before concluding the letter with critical remarks about the papers by Prym (1871) and Weber (1870), and on Neumann’s book (1865) as well. In a subsequent letter to Casorati on October 9, 1872 Schwarz proudly affirmed that it

⁵⁸Some thirty years later this result was independently re-obtained by Hadamard, see von Renteln (1996) and Maz’ya and Shaposhnikova (1998, 375–376), who point out that Prym gave a single but instructive counter-example whereas Hadamard exhibited as whole family of Fourier series that are counter-examples.

is not too much to say that the Dirichlet principle could be replaced by rigorous methods of proof that do for the theory of Abelian integrals what Riemann has obtained with the aid of this principle. As for Weierstrass's rigorous methods, he referred to the presentation he had given in his letter to Dini on March 3rd, 1871.

In the meantime Dini had produced a long memoir inspired by Schwarz (1872a). In this work Dini tackled the problem of determining a function u that is harmonic within a circular annulus and is “generally continuous together with its normal derivative $\partial u/\partial p$ ” on the boundary. Then he extended this to the case of a sphere with values on the boundary given by a linear function of u and $\partial u/\partial p$, and eventually treated the case of $\Delta^2 u = f$ where f is a continuous function together with its first and second derivatives in the given domain (Dini 1871–1873).

In autumn 1872 Casorati based his lectures on Dirichlet's principle at the Istituto Tecnico Superiore in Milan on Schwarz's papers. In spite of the latter's claims, however, Casorati was not completely satisfied. He considered that Schwarz's papers dealt with particular cases and kept working on the subject without being able to fully overcome the difficulties involved in it. Thus, in the hope of getting first-hand information about methods and results of the German mathematicians, he was happy in 1877 to support the plan of his young, talented colleague Pincherle to spend an academic year in Berlin.

6.5.5 Continuous Nowhere Differentiable Functions

In the third part of Pincherle's *Saggio* the general concept of a function is introduced with the following definition: “If a real or complex variable quantity y is linked to n other real or complex variable quantities $x_1, \dots, x_n$ in such a way that one or several determinate values of y correspond to all systems of values of these n variables within given limits, then y is said to be a function of the n variables $x_1, \dots, x_n$ in the most general sense of the word function” (Pincherle 1880, 246). This was followed by the $\varepsilon - \delta$ definition of continuity for a function of one variable within given limits. The definition echoed in more precise terms the one that Weierstrass had given in his 1861 course (see Sect. 6.3.3): “A function of one variable x will be said to be continuous within certain limits of values of x if for any x_0 within those limits and any arbitrarily small number ε it will be possible to find a neighbourhood of x_0 such that for all values x' in this neighbourhood one has $|f(x') - f(x_0)| < \varepsilon$ ” (Pincherle 1880, 246). This definition was extended by analogy to functions of several variables $x_0, x_1, \dots, x_n$ by considering a suitable neighbourhood U of $(x_1^0, x_2^0, \dots, x_n^0)$ depending on (an arbitrarily small) ε such that for any $(x'_1, x'_2, \dots, x'_n)$ in U one has $|f(x'_1, x'_2, \dots, x'_n) - f(x_1^0, x_2^0, \dots, x_n^0)| < \varepsilon$.

Pincherle then recorded the definition of the derivative of a function $f(x)$ at a point x_0 . As Weierstrass had done in 1861, $f'(x_0)$ was said to be the derivative of $f(x)$ at x_0 if

$$f(x_0 + h) = f(x_0) + hf'(x_0) + h\tilde{\varphi}(x_0, h), \quad (6.59)$$

where $\tilde{\varphi}(x_0, h) \rightarrow 0$ for $h \rightarrow 0$.

According to Hurwitz's lecture notes, Weierstrass first gave the definition

$$f(x+h) = f(x) + hf'(x) + hf_0(x,h)$$

in the case when $f(x)$ is a power series $f(x) = \sum A_\lambda x^\lambda$ and $|x| + |h| < r$, where r is the radius of the convergence disk of the series, and $f_0(x,h)$ is a power series becoming infinitely small with h . Then he added that if (6.59) occurs for any continuous *analytical expression* $f(x)$, to use his term, then $f'(x)$ is said to be its first derivative. This was followed by the theorem that any power series is always differentiable, and its derivative is a power series that has the same convergence disk as the primitive series. As a power series is differentiable infinitely many times, the same holds true for all of these infinitely many derivatives. Then Weierstrass extended this to power series of n variables.

In Hettner's manuscript notes (Weierstrass 1874, 234–236) one reads that a function $f(x)$ is differentiable at a point x_0 if there exists a number c such that

$$f(x_0 + h) = f(x_0) + h \times c + h \times h_1,$$

where $h_1 \rightarrow 0$ for $h \rightarrow 0$, c being independent of h , and $f(x_0) + h \times c$ being a linear function that Weierstrass proved to be unique. This is accompanied by the comment: "Herein lies the true concept of the differential quotient". Then Weierstrass extended the definition of differential quotient to functions of several variables analogously.

By comparison with his 1861 course, in the meantime Weierstrass had found his example of continuous nowhere differentiable functions (see Eq. (6.61)). This is recorded in all the lecture notes we are considering. Thus, after mentioning the fact that in many treatises one still finds the theorem: "every continuous function admits a derivative", whose proof is (implicitly) based on some property which is not included in the general concept of a function, Pincherle (1880, 247) added that many counter-examples "found in recent times" such as the function $f(x) = \sum_{n=1}^{\infty} \frac{\sin n!x}{n!}$

clearly show that one would search in vain for a proof.⁵⁹ Instead, according to both Hettner's and Hurwitz's manuscripts, Weierstrass (1874, 221–234; 1988a, 79–81) proved the existence of continuous nowhere differentiable functions with the same example that he had presented to the Akademie in 1872.

As occurred with Dirichlet principle, also in this case Weierstrass was provided with the occasion by Schwarz. In a letter on June 20th, 1872, Schwarz called his master's attention to the last issue of the *Nouvelles Annales de mathématique* where a "certain" Gilbert had once more repeated "the absurdity that obviously a function has a differential quotient".⁶⁰ On the other hand, Schwarz asked, how could one

⁵⁹This is essentially Darboux's example from his (1875); see also Alexander (1995).

⁶⁰A professor at the university of Louvain, Philip Gilbert made this "theorem" the subject of a memoir *couronné* by the Belgian Académie (Gilbert 1873a). He had also repeated and "proved" this claim in his contemporary treatise on calculus (Gilbert 1872a). Later on he corrected his claim

make this reproach of a “provincial mathematician” if “Bertrand opens his textbook with a supposed proof of this statement?”.⁶¹

It would be useful, Schwarz commented sarcastically, to drop a line to the editors of the *Nouvelles Annales* to invite Bertrand to prove that

$$f(x) = \sum_{n \geq 1} \frac{\sin n^2 x}{n^2} \quad (6.60)$$

has a derivative. Schwarz sent one more letter to Weierstrass who answered “only partly” on July 8, apologising for the delay. After promising to send him a letter with “a real mathematical content” very soon, he turned to Abelian functions, and sketched his future work concerning them (see Sect. 6.8). There is no record of Weierstrass’s promised letter. Instead, some ten days later on July 18, 1872 he read a paper “on continuous functions without determined differential quotients” at the plenary session of the Akademie.⁶²

There Weierstrass began by remarking that even such rigorous mathematicians as Gauss, Cauchy, and Dirichlet apparently shared the view that a continuous function has always a derivative (possibly except at isolated points). Riemann was the first to state that this could not be generally admitted. According to the recollections of some of his students, in 1861 or perhaps even earlier in his lectures Riemann produced a suitable example of a function⁶³ to contradict that commonly accepted view. “Unfortunately Riemann’s proof has not been published”, Weierstrass added, neither could it be found in his *Nachlass* nor it is recorded in oral communications. “This is all the more to regrettable as I never could gather with certainty how Riemann expressed himself to his audience”. This raises historically interesting questions: Did Riemann actually discover that example? And, if so, what did Riemann really say? As for the first question, on the basis of two letters Christoffel sent to Prym in 1865 and other sources, Butzer and Stark (1986, 45–46) “provide some evidence to the effect that—contrary to Weierstrass’s assertion—the so-called ‘Riemann example’ cannot be brought in recorded (!) connection with either Riemann or his students”. Interestingly, in his letters to Prym Christoffel discussed the differentiability of the very similar example $f(x) = \sum_{n \geq 1} \frac{\cos n^2 x}{n^2}$ without mentioning Riemann. In short, according to Butzer and Stark there is no evidence that would attribute the example under discussion to Riemann other than Weierstrass’s claim.

(Gilbert 1873b) in the light of (Schwarz 1973), as he did in the second edition (1878) of his treatise by adding a note inspired by (Darboux 1875).

⁶¹ Actually, in Bertrand’s (1864, 3) he said that “for any function $\phi(x)$ whatsoever the ratio $(\phi(x+h) - \phi(x))/h$ has a finite limit for h tending to zero”.

⁶² As is recorded in the *Monatsberichte*, p. 560.

⁶³ This turned out to be the very same function (see Eq. (6.60)) that Schwarz mentioned in his letter to Weierstrass, without any reference to Riemann!

Ullrich (1997), on the other hand, offers further evidence to support Weierstrass's assertion, in the form of sets of Weierstrass's lecture notes and Kronecker's lectures on integration (Kronecker 1894, 8 and 13). Be this controversial matter as it may, the evidence produced by Ullrich (1997, 250–253) allows us to address the second, mathematically more interesting question. We can rephrase it more precisely in modern terms: did Riemann speak of a function that was non-differentiable at points of a dense set of $\mathbb{R}$, or instead of a nowhere differentiable function? Writing to du Bois-Reymond on November 23, 1873 Weierstrass (1923a, 199–200) repeated that it was unknown “whether Riemann had affirmed that his function had at no point a determined differential quotient”, adding that in Riemann's school seemingly nobody had any idea about the existence of such functions. To support his claim Weierstrass referred to Hankel (1870, 77), adding that a function $f(x)$ with a determined differential quotient at irrational values of x , but not at rational values, is such that its derivative $f'(x)$ would fail to be a function not only in the ordinary sense but also in Riemann's. In his SS 1874 lectures Weierstrass (1874, 220) repeated this in a more precise way, stating that it was unclear whether Riemann thought of his function as being nowhere differentiable or failing to be differentiable at some points in any interval. In the latter case, he said, its derivative cannot be considered as a function.

In this connection, however, a statement by Betti is worth mentioning. When he reported on the content of his talks with Riemann in a letter to Tardy on January 2nd, 1864, Betti commented “on Riemann's remark about the existence of continuous functions of a real variable that do not have a derivative”, and explained that “they are functions that have an infinite number of maxima and minima in an infinitesimal range” (in Cerroni and Martini, 2009, 89). This is an astute remark, for Schwarz's example in his (1973) is of a continuous strictly increasing function that fails to be differentiable at a dense set of points, and Lebesgue (1904) was later to prove that a continuous strictly increasing function is differentiable almost everywhere. So continuous functions that are nowhere differentiable must be as Betti described—infinitely oscillating.

Once Riemann's example became more widely known, Weierstrass stated in the printed paper (1872, 71–72) that most mathematicians dealing with this matter apparently shared the view that it was enough to demonstrate the existence of functions that fail to have a differential quotient at points in any interval, however small. But, he went on, it is “extraordinarily easy” to create examples of functions that have this property. For this reason he was willing to believe that Riemann had in mind only functions that do not have a determined differential quotient at any value of their argument. That the function represented by the series (6.60) enjoys this property “seems to me somewhat difficult to prove” Weierstrass remarked.⁶⁴ A much easier task, Weierstrass concluded, is to produce an example of a continuous nowhere differentiable function like the following

⁶⁴Weierstrass's guess was right. In 1916 Hardy proved that Riemann's function is not differentiable in a dense subset of $\mathbb{R}$, and indeed Gerver (1970) has shown that $f'(x) = -1/2$ at values $x = \zeta\pi$, with $\zeta = (2A + 1)/(2B + 1)$ for integers A and B .

$$f(x) = \sum_{n=0} b^n \cos a^n \pi x, \quad (6.61)$$

where a is an odd integer and $0 < b < 1$. Weierstrass limited himself to stating that $f(x)$ is continuous.⁶⁵ In order to prove that it is not differentiable at any point x_0 he observed that one can determine an integer α_m (m an arbitrary positive integer) such that $-\frac{1}{2} < a^m x_0 - \alpha_m \leq \frac{1}{2}$. By putting

$$x' = \frac{\alpha_m - 1}{a^m}, \quad x'' = \frac{\alpha_m + 1}{a^m}$$

one has $x' < x_0 < x''$, such that x', x'' approach x_0 arbitrarily closely for increasing m .

Under the further assumption that $ab > 1 + \frac{3\pi}{2}$ a straightforward calculation showed that the ratios

$$\frac{f(x') - f(x_0)}{x' - x_0}, \quad \frac{f(x'') - f(x_0)}{x'' - x_0}$$

have opposite signs and become infinite as $m \rightarrow \infty$. Thus, Weierstrass concluded, at x_0 the function $f(x)$ has neither a finite nor an infinite, determined differential quotient.

However important this counterexample may be in the history of modern analysis, Weierstrass apparently did not attach too much significance to it. Indeed, he did not rush to publish his “piquant” example—as he used to call it—but let the paper be published only in vol. 2 of his *Werke* (Weierstrass 1872).

The first example of such functions appeared in print almost one year later, in September 1873, when Schwarz published his own example of a continuous nowhere differentiable function, which he had communicated one month earlier to the mathematical section of the Schweizerischen Naturforschenden Gesellschaft (Schwarz 1973). There Schwarz remarked that Riemann’s 1854 example of an integrable function discontinuous at every rational point, had in fact provided the example of a function—i.e. its primitive function—that is never differentiable at a rational point.

Eventually, Weierstrass’s counter-example appeared in print in 1875, when du Bois-Reymond published it and the relevant proof in his (1875, 29–31).⁶⁶ He had received the example from Weierstrass who in turn communicated the paper to

⁶⁵This follows immediately by observing that each term of the series is bounded by $\pm b^n$, the partial sums are continuous, and the series is uniformly convergent since $0 < b < 1$. Therefore, its sum is a continuous function.

⁶⁶The publication date 1875 is somehow misleading for in his letter Weierstrass referred to du Bois-Reymond’s paper already in 1874. (See Sect. 6.6). Interestingly, Boltzmann (1895) referred to this example in a paper on Brownian motion; an early indication of the possible physical significance of functions others were to dismiss as pathological.

Borchardt for publication as early as November 1873. In his letters to du Bois-Reymond Weierstrass added that Riemann had once said that he had come across his function (6.60) in his research on elliptic functions, and commented at length on the fact that “now we know functions with properties of which one had no inkling before and which as you say, contradict all our previous ideas” (Weierstrass 1923a, 201).

Later on, on April 19, 1874 Weierstrass communicated to Schwarz an example found by Sonya Kovalevskaya adding that one could construct general series of the form $\sum_{n=0} b_n \cos(a_n x + c_n)$ having the same properties as his own function (6.61). However, he added, further research on this subject “would be of interest only if one could find general laws according to which one could be able to judge whether any of those functions is differentiable or not”. Assuming that a_n and b_n are positive constants, he tentatively conjectured that $f(x) = \sum_{n=0} b_n \cos(a_n x + c_n)$ is nowhere differentiable if $\sum b_n$ is convergent and $\sum a_n b_n$ divergent “but”, he concluded, “it does not seem easy to prove this rigorously”.

It is worth remarking here that one finds the differential calculus in almost every set of Weierstrass’s lecture notes on analytic function theory. However, the integral calculus is treated only in Killing’s 1868 notes, and in Weierstrass’s last lecture (see Sect. 6.9.4). In Killing’s notes, integrals are introduced at the end of the course, after the treatment of series, including Lagrange series for the inversion of functions and analytic continuation. Remarkably, Weierstrass first introduced the periods of the integrals in a Riemannian way, by considering simply connected domains and reducing multi-connected domains to simply connected ones by means of cuts along paths, then he sketched a definition of the integral in the complex domain along the lines of Cauchy’s 1825 paper (Weierstrass 1968, 96–102). On the other hand, in Killing’s notes Riemann is explicitly mentioned by Weierstrass in connection with the branches of a function: “Following Riemann, the totality of the elements derived around the points of the line is named a branch of the function” (Weierstrass 1968, 73).

6.5.6 Analytic Functions

Following Weierstrass, in the *Saggio* Pincherle proved the theorem that a continuous function $f(x)$ of one real variable on a closed interval attains its upper and lower bounds, then he added some comments on the definition of the general concept of a function, including historical remarks on the early definitions (due to Leibniz and Johann Bernoulli) and the definitions given by Euler, Lagrange, Cauchy and Dirichlet. In particular, he criticised Dirichlet’s concept of a function as being “too vague and indeterminate” so that “it is impossible to find any properties common to all functions”. According to Weierstrass, “we cannot create a theory of functions if we do not in some way limit the class of functions for which we want to give common properties” (Pincherle 1880, 254). These will be the analytic functions that enjoy two “essential properties”:

1. They are defined for real and complex values of the variable, and
2. They enjoy all the properties that “used to be attributed to all functions in the treatises on the calculus”.

Thus, functions that lose their meaning for complex values of the variable, such as $f(x) = \sum_{n=1}^{\infty} \frac{\sin n!x}{n!}$ and his own example (6.61) “will not be considered as truly analytic functions but only as limiting cases of them” (Pincherle 1880, 254).

Then he explained that analytic functions would be introduced by proceeding from the simple to the complicated, from functions constructed from a finite number of the simplest arithmetical operations (addition and multiplication) to functions built from an infinite number of them and their (finite or infinite) combinations in order to get the fundamental, characteristic property of analytic functions, i.e. their expansion in Taylor series.

Weierstrass pointed out that analytic functions in *his* sense are the functions $u + iv$ which Cauchy and Riemann characterised by requiring that the Cauchy–Riemann equations are satisfied. In his view, however, their general definition of a function appears to be founded on a property of an arbitrary character, whose generality cannot be established a priori. In addition, their definition requires that the functions u and v be chosen among the functions of two real variables which admit partial derivatives. But “in the current state of science” such functions form a class which cannot be precisely defined, and for this reason their definition will not be adopted. As it assumes knowledge of, and indeed the existence of the derivatives, “it is poorly adapted for building the theory of analytic functions upon it”, one reads in Hurwitz’s notes (Weierstrass 1988a, 49). He repeated this opinion even in his last lecture in 1886 (Weierstrass 1988b, 115).

Having summarised the principal properties of rational, entire functions of a single variable, including the Lagrange interpolation formula, Weierstrass turned to infinite series of rational functions by emphasising the analogy of this procedure with what he had done in the introduction of numbers, where he had first considered numbers with a finite number of elements then numbers with infinitely many elements (Weierstrass 1988a, 59). Apparently Hilbert (1897b, 332) referred to this when he stated that for Weierstrass the power series is both “conceptually” and “formally” the analogue of the irrational number. This is not surprising, if Dugac’s (1973, 76) claim is right that Hilbert knew of Weierstrass’s analysis from the lecture notes of his friend Hurwitz. (According to Hilbert, “conceptually” Weierstrass considered the power series as the analogue of the irrational number. That the analogy holds also “formally”, Hilbert added, is shown by the calculation with series which follows the usual rules of arithmetic—addition, subtraction, multiplication, and division).

Weierstrass then defined uniform convergence for series of functions and proved that the sum of a uniformly convergent series of rational functions in a given domain is a continuous function there. Then he treated power series of one variable in detail, and stated his now famous theorems for them (the existence of a disk of convergence, uniform convergence within the disk, inequalities for the coefficients of a series without resorting to Cauchy integral theorem, as he had done in 1841, and so forth), and he remarked that “analogous propositions” hold for series of several

variables. Then he stated and proved his double series theorem for one and several variables, established the identity theorem for series assuming their agreement in an (arbitrarily small) open neighbourhood of the origin, and defined term-by-term differentiation for series of one and several variables.

Next Weierstrass turned to study the behaviour of a function on the boundary of its domain of definition. The (Cauchy) inequalities allowed him to prove that the power series expansion of a single-valued analytic function converges in each open disk contained in the domain of definition of the function. Then a compactness argument (making an essential use of “topological” results such as the existence of a lower bound for bounded, infinite sets of real numbers), proves the existence of a singular point on the circumference of the disk of convergence of the series. This proof can be extended to series of several variables. Eventually, Pincherle sketched Weierstrass’s method of analytic continuation of a power series (a “function element”, in Weierstrassian terminology) by means of chains of overlapping disks. This summary of Weierstrass’s lectures was enough, Pincherle concluded, to enable one to read Weierstrass’s (1876b) paper.

At this point in the lectures Hettner’s manuscript reports that Weierstrass explained the import of his research on the convergence of power series, and his “ideal goal” in the lecture. The former consisted in the (rather “Riemannian” (!), one would say) discovery that the characteristic properties of functions lay in their singularities. The latter was to “represent analytically” functions that are completely defined in any way whatever, and “to get a priori the form and the conditions of such a representation” (Weierstrass 1874, 359–360). Actually, this was the aim Weierstrass had pursued in his (1876b) paper, including the representation theorems he had found by the end of 1874 (see Sect. 6.7). Obviously, these theorems are not included in Hettner’s lecture notes of the SS 1874 course, whereas they are presented in Hurwitz’s manuscript, which also includes chapters on singular points, on the inversion of analytic functions, and culminates in the final chapter on analytic domains (*Gebilde*) that play a central role in Weierstrass’s theory of Abelian functions (see Sect. 6.8).⁶⁷ All this provides evidence of Weierstrass’s remarkable attitude to presenting his newly founded results in his lectures.

6.5.7 The Fundamental Theorem of Algebra

In his lectures on analytic function theory Weierstrass used to prove the fundamental theorem of algebra as an immediate consequence of Liouville’s theorem (without naming it this way). According to Hurwitz’s lecture notes, Weierstrass (1988a, 104–108) tackled the subject as an application of his proof of the existence of (at least) one singular point on the boundary of the convergence disk of a power series.

⁶⁷We explain what Weierstrass meant by *Gebilde* below, Sect. 6.8.1. The term is difficult to translate and we have opted to keep the German word in this book.

He began by considering the function

$$f(x) = \frac{a_0 + a_1x + \cdots + a_nx^n + \cdots}{b_0 + b_1x + \cdots + b_mx^m + \cdots} = \frac{\varphi(x)}{\psi(x)}$$

and, assuming that the series $\sum a_kx^k$ and $\sum b_kx^k$ are convergent and the intersection of their disks of convergence is not empty, he asked the question under which conditions the ratio $\frac{\varphi(x)}{\psi(x)}$ can be expressed as a power series $\sum c_kx^k$ and what its convergence domain is. Weierstrass first proved that when the ratio $\frac{\varphi(x)}{\psi(x)}$ never becomes infinite anywhere in the intersection domain, then the series $\sum c_kx^k$ is certainly convergent there. If the contrary occurs for some values x , then the convergence circle of $\sum c_kx^k$ passes through the least of the values of $|x|$.

As a particular case he considered the rational function

$$f(x) = \frac{a_0 + a_1x + \cdots + a_nx^n}{b_0 + b_1x + \cdots + b_mx^m} = \sum_{k=0}^{\infty} c_kx^k$$

and, by resorting to Cauchy's inequalities (without naming them), he proved that

1. If the power series $\sum c_kx^k$ remains smaller than any given quantity for any value of x then it has necessarily a bounded convergence domain, and
2. If the power series is convergent and it never becomes greater than any given quantity, then it reduces to a constant (Weierstrass 1988a, 107).

This is equivalent to Liouville's theorem.

Then, from the ratio

$$f(x) = \frac{1}{b_0 + b_1x + \cdots + b_mx^m}$$

the existence of (at least) one root of $b_0 + b_1x + \cdots + b_mx^m = 0$ followed immediately. (By considering the logarithmic derivative $\frac{f'(x)}{f(x)}$ Weierstrass easily concluded that the roots are m in number.)

In this connection, it is worth mentioning that one of Weierstrass's recurrent concerns was to provide a constructive proof of the fundamental theorem of algebra without resorting to continuity arguments. His first attempt at a "new proof" of the theorem was given in 1859 with a paper that he presented to the Berlin Akademie on December 12th. In the *Monatsberichte* (1859, 758) it is only recorded that "Herr Weierstrass read a paper: Contributions to the theory of equations", but the paper itself was not published until 1894 when it was printed in the first volume of his *Werke* (Weierstrass 1859).

To give the sought-for constructive proof Weierstrass considered the polynomial

$$f(x) = x - \varphi(x),$$

where

$$\varphi(x) = a_0 + a_2x^2 + a_3x^3 + \dots + a_\rho x^\rho.$$

Then he constructed the sequence defined recursively by

$$x_0 = 0, x_1 = \varphi(x_0), \dots, x_n = \varphi(x_{n-1}), \dots$$

and stated that “it can be shown that under determined conditions when n increases indefinitely x_n approaches a determined limit $\tilde{x}$ that satisfies the equation $f(\tilde{x}) = 0$ ” (Weierstrass 1859, 247).

To prove this, he considered the difference

$$\varphi(x') - \varphi(x) = (x' - x)\{a_2(x + x') + a_3(x^2 + xx' + x'^2) + \dots\} = (x' - x)\varphi(x, x').$$

with x' chosen arbitrarily. Thus, $\varphi(x, x')$ is an entire function of $x, x', a_2, a_3, \dots, a_\rho$ and from the sequence above he obtained the recursive sequence

$$\begin{aligned} x_1 &= a_0 \\ x_2 - x_1 &= x_1 \varphi(0, x_1) \\ x_3 - x_2 &= (x_2 - x_1) \varphi(x_1, x_2) \\ &\dots \\ x_n - x_{n-1} &= (x_{n-1} - x_{n-2}) \varphi(x_{n-2}, x_{n-1}), \end{aligned}$$

which, by putting

$$\varphi(0, x_1) = \psi_1; \varphi(x_1, x_2) = \psi_2; \dots; \varphi(x_{n-2}, x_{n-1}) = \psi_{n-1}; \dots,$$

he rewrote as

$$\begin{aligned} x_1 &= a_0 \\ x_2 - x_1 &= x_1 \psi_1 = a_0 \psi_1 \\ x_3 - x_2 &= (x_2 - x_1) \psi_2 = a_0 \psi_1 \psi_2 \\ &\dots \\ x_n - x_{n-1} &= (x_{n-1} - x_{n-2}) \psi_{n-1} = a_0 \psi_1 \psi_2 \dots \psi_{n-1} \dots \end{aligned}$$

This yields

$$x_n = a_0 \{1 + \psi_1 + \psi_1 \psi_2 + \dots + \psi_1 \psi_2 \dots \psi_{n-1}\}$$

and

$$x_n - x_m = a_0 \{ \psi_1 \psi_2 \dots \psi_m + \psi_1 \psi_2 \dots \psi_{m+1} + \dots + \psi_1 \psi_2 \dots \psi_{n-1} \}$$

for $m < n$.

Assuming that $a_0, a_1, \dots, a_\rho$ are all positive, Weierstrass's next step was to prove that for a suitable choice of ξ (> 1) and for any a_0 smaller than a certain limit one has

$$x_i < a_0 \xi, \quad \psi_i < 1 - f'(a_0 \xi) \quad (i = 1, 2, \dots, n-1)$$

and, consequently,

$$1 + \psi_1 + \psi_1 \psi_2 + \dots + \psi_1 \psi_2 \dots \psi_{n-1} < \frac{1}{f'(a_0 \xi)} < \xi.$$

This can be proved also for arbitrary (positive and negative) $a_0, a_1, \dots, a_\rho$ by considering the absolute values $|a_i|$ with $(i = 0, 1, \dots, \rho)$. Thus, $|\psi_1|, |\psi_2|, \dots$ are all smaller than an arbitrarily given quantity < 1 .

Then, assuming that $|\psi_\mu| < \varepsilon$ for $\mu > n$ for a suitable, determined n , if $v > \mu > n$ one has

$$x_v - x_\mu = a_0 \{ \psi_1 \psi_2 \psi_\mu + \psi_1 \psi_2 \dots \psi_{\mu+1} + \dots + \psi_1 \psi_2 \dots \psi_v \}$$

and, consequently

$$\left| \frac{x_v - x_\mu}{a_0} \right| < \varepsilon^\mu + \varepsilon^{\mu+1} + \dots + \varepsilon^v < \frac{\varepsilon^\mu}{1 - \varepsilon}.$$

Then, for arbitrarily large μ , and $v > \mu$, the difference $x_v - x_\mu$ becomes arbitrarily small. This allowed Weierstrass to conclude that for increasing μ x_μ approaches a determined limit $\tilde{x}$, and the difference $f(\tilde{x}) - f(x_\mu)$ becomes arbitrarily small. As

$$f(x_\mu) = x_\mu - \varphi(x_\mu) = x_\mu - x_{\mu+1}$$

and $x_\mu - x_{\mu+1} \rightarrow 0$ for $\mu \rightarrow \infty$ then $f(\tilde{x}) = 0$ and this concluded the proof.

It is worth noting here that in the essential step—the proof of the existence of the limit $\tilde{x}$ —Weierstrass made an implicit use of the Cauchy criterion for sequences of real numbers (without naming it) that in turn holds on the assumption of the completeness of $\mathbb{R}$. Even though not yet in possession of the rigorous theory of real numbers that he was to produce some years later in 1863, nonetheless Weierstrass should have been aware of the fact that the existence of $\tilde{x}$ involved “considerations of continuity” that he wanted to avoid. As he stated in his (1891), for this reason he considered this proof somewhat unsatisfactory, and he decided not to publish this paper.

In the second paragraph Weierstrass proved that, given arbitrary, distinct numbers $c_1, c_2, \dots, c_\rho$ and $f_0(x) = (x - c_1)(x - c_2) \dots (x - c_\rho)$, one can determine upper bounds for the variables $u_1, u_2, \dots, u_\rho$ in such a way that the equation

$$f(x) = f_0(x) + u_1 x^{\rho-1} + u_2 x^{\rho-2} + \dots + u_\rho = 0$$

has ρ roots each of which approaches one of the values $c_1, c_2, \dots, c_\rho$ as close as one wishes.

Eventually, in the last paragraph Weierstrass proved that any equation $f(x) = x^p + A_1x^{p-1} + \dots + A_p = 0$ with non-vanishing discriminant (i.e. $f(x)$ and $f'(x)$ do have no common factors) has p distinct roots.

Apparently Weierstrass presented a new proof of the relevant theorem to the Akademie on July 9, 1868 for in the *Monatsberichte* (1868, 428) it is recorded that “Herr Weierstrass read on a new proof of the fundamental theorem of algebra”. This time, too, the paper was not published (nor it is included in his *Werke*). Eventually, in 1889 Weierstrass produced a “new proof” of the theorem that he presented to the Akademie on February 21st.

Two years later the paper was published in the *Berichte* (Weierstrass 1891) perhaps because Weierstrass heard of a similar proof that Méray (1891) had found independently and published that very same year. In a footnote appended to the paper Weierstrass explained that he had avoided publishing the proof given in his two previous papers on the subject because “it was not completely free from considerations of continuity” and for this reason “it was somewhat unsatisfactory” in his eyes. This time he proudly stated that the distinctive feature of his own proof by comparison with the series of rigorous proofs already known lay essentially in that “it is produced in a purely arithmetical way without resorting to aids and concepts that are alien to algebra” (Weierstrass 1891, 251).⁶⁸

Given n variables $x_1, x_2, \dots, x_n$ Weierstrass denoted the coefficient of x^{n-v} in the expansion of $(x - x_1)(x - x_2) \dots (x - x_n)$ by

$$(x_1, x_2, \dots, x_n)_v \quad (v = 1, 2, \dots, n).$$

Then the theorem could be reformulated as follows: Let $C_1, C_2, \dots, C_n$ any given quantities, then there exists always a system of determined values of $x_1, x_2, \dots, x_n$ satisfying the n equations

$$(x_1, x_2, \dots, x_n)_v = C_v \quad (v = 1, 2, \dots, n)$$

such that when

$$f(x) = x^n + C_1x^{n-1} + \dots + C_n$$

then

$$f(x) = \prod_v (x - x_v) \quad (v = 1, 2, \dots, n)$$

for each value of x .

Weierstrass’s aim was to prove this theorem “in the most direct way” by providing an effective procedure such that when $C_1, C_2, \dots, C_n$ are given numbers, one can “with certainty” compute n numbers that satisfy the equations $(x_1, x_2, \dots, x_n)_v = C_v$ without any need to previously prove their existence.

⁶⁸Despite their deep disagreements in this period, Weierstrass sounded oddly close to the views of Kronecker at this moment.

He succeeded in doing this by producing the sought-for sequence of n numbers first when the discriminant D of $f(x)$ does not vanish. If $D = 0$, then $f(x)$ can be reduced by means of rational operations to a product of factors each of which has a non-vanishing discriminant, and this concluded the proof.

In spite of Weierstrass's claim, however, when producing a proof of the fundamental theorem of algebra satisfying the conditions of Brouwer's intuitionism [Kneser \(1940\)](#) labelled Weierstrass's 1859 and 1891 proofs simply as "continuity proofs" for they sought to obtain the quantities $\alpha_1, \alpha_2, \dots, \alpha_n$ in the polynomial $\varphi(x) = (x - \alpha_1)(x - \alpha_2) \dots (x - \alpha_n)$ step-by-step by small variations such that in the end $\varphi(x) = f(x)$.

6.6 Elliptic Functions

Weierstrass experienced the period from the early 1870s to the early 1880s, when was in his sixties, as one of the most fruitful times of his scientific life. In this Indian summer of his, Weierstrass succeeded in finding many of his most significant results in function theory and was also able to give the theory of elliptic and Abelian functions the definitive form which is expounded in his *Werke*.

As usual, Weierstrass had planned to teach elliptic functions theory once more in WS 1874/75. He must have also planned to publish "a collection of elliptic formulae" because, he assured Schwarz on September 1st, 1874, "I haven't lost sight of the projected collection", and he asked him for "a good booklet" of the material. After the publication of Koenigsberger's book, however, it had become necessary for Weierstrass to write an introduction to it "presenting and justifying [his] methods clearly".

The book Weierstrass was referring to was Leo Koenigsberger's *Vorlesungen über die Theorie der elliptischen Functionen* (see below, Sect. 10.3). Koenigsberger was by then a professor at Heidelberg University. As noted above, he had taken Weierstrass's first course on elliptic functions back in 1857 (see Sect. 6.3.1), and he had remained on friendly relations with his teacher ever since. Schwarz was very disappointed with Koenigsberger's book, and promptly reacted as he had done two years before in Kossak's case. He had expected to "get in his hands a work of one of your students", he wrote to Weierstrass on October 2nd, 1874, but he realised very soon that "Herr Koenigsberger is not a student of yours, at least not in the sense I understand it".

Schwarz was not sparing with his criticisms. The book was written in a very bad German and, above all, it was flawed by a number of inaccuracies and mistakes particularly when dealing with general function theory. Being a student of yours, Schwarz wrote, "I cannot be surprised enough by the fact that Herr K. does not know that the existence of a differential quotient has nothing to do with the continuity of a function of a real argument, on which you have put so great a weight".

Inaccuracies also included an "actual mistake"—the claim that $\Delta u = 0, \Delta v = 0$ are necessary and sufficient condition for $u + iv$ to be a complex function of $x + iy$

(Koenigsberger 1874, 18)—the “usual confusion” between a function of “getting arbitrarily close” to a value and “actually taking” it (Koenigsberger 1874, 111 and 122), a misstatement of the Dirichlet principle (Koenigsberger 1874, 171), and so forth. Summing up, Schwarz concluded, “the main value” of Koenigsberger’s book consisted of “the hope of now having your elliptic functions in authentic form”. Indeed, “it would be a pity if your many students should wait for even longer for your theory of elliptic functions to be within the reach of mathematicians”. Schwarz offered his help in realising such a work, which would be “much more use than the books Hermite–Natani, Durège, Thomae, and Koenigsberger put together”. This planned, joint work was to take a further 10 years before it appeared (in an incomplete form) as Weierstrass (1883–1885).

The long answer Weierstrass sent Schwarz more than two months later⁶⁹ on December 16th, 1874 makes interesting reading in many ways. “I would very much prefer to keep silent”, Weierstrass began in reply to Schwarz’s criticism of Koenigsberger’s book. Of course he would not contradict Schwarz, but on the other hand “he always liked Koenigsberger very much”. Then he assured Schwarz that “Koenigsberger is absolutely not a student of mine in the sense you conceive of this term”. Weierstrass also mentioned a recent meeting he had with Koenigsberger in summer holidays. On that occasion he realised that the latter’s “views on the foundations of function theory diverge so essentially from mine as to make understanding between us impossible”.

However, in spite of his wish “to keep silent” Weierstrass went on pointing out his criticisms of “some points” of Koenigsberger’s work and asked his pupil to keep all that to himself. He began by commenting on a remark by Koenigsberger (1874, 13) according to which a single-valued, continuous function $f(x)$ having a finite number of maxima and minima in a finite interval has a determined, finite derivative there except at individual (*einzelnen*) points. Apparently, this claim was not contradicted by Weierstrass’s example (6.61). Objecting to Koenigsberger’s claim, Weierstrass wrote Schwarz to have a “piquant” example of an everywhere continuous function $f(x)$ that has no maxima or minima but it is “so capricious” that $(f(x+h) - f(x))/h$ for $h \rightarrow 0$ oscillates between two finite bounds when x is algebraic but behaves “completely reasonably” when x is transcendental. He returned to this point in a letter to du Bois–Reymond on December 15, 1874. “I get angry”, Weierstrass wrote, somehow reproaching his colleague, for the latter’s “very true remarks⁷⁰ have provided a weapon in the hands of my former student and friend Koenigsberger” (Weierstrass 1923a, 205), and now he produced the “piquant” example he had only hinted at to Schwarz. Later on, on February 10, 1876 Weierstrass explained his criticism in detail in a long letter to Koenigsberger himself (Weierstrass 1923b, 231–234). There he offered the same example of a function that has no maximum

⁶⁹Weierstrass had forwarded Schwarz’s letter to Sonya Kovalevskaya, see Bölling (1993, 157). On December 16 he asked her to return the letter in order to answer Schwarz, but the letter “was lost in an unfortunate accident”, as he wrote to Schwarz on January 28, 1875.

⁷⁰Weierstrass was referring to du Bois–Reymond (1875, 29).

or minimum but still fails to be differentiable on a dense set of $\mathbb{R}$. In so doing he referred to Cantor's recent paper on the denumerability of algebraic numbers.

In Weierstrass's own eyes, however, his criticism of Koenigsberger's approach to elliptic functions was much more significant. When dealing with an elliptic integral of the first kind, he observed in his letter to Schwarz on 16 December 1874, the "main thing, which has to be proved rigorously" is the fact that the upper bound of the integral is a single-valued function of the integral, which for finite values of the argument has the character of a rational function. "This was well recognised by Jacobi", Weierstrass added, observing in passing that he had found a note in the latter's *Nachlass* where Jacobi stated he had resorted to theta functions in his lectures because the "the theory of integrals between complex limits was still too poorly established in order to have a completely clear view of the dependence relationship between the value of an integral and its bounds". (See Sect. 4.2.1).

Admittedly, Koenigsberger had wanted to follow Weierstrass's route, as the latter recognised. "The two-fold Riemann surface is used by him to found and establish the analytic nature of the elliptic integral. I find nothing to fault in this", Weierstrass wrote. But Koenigsberger had discussed the inversion of this integral in a very unsatisfactory way, misunderstanding Riemann and (tacitly) resorting to Briot and Bouquet. In doing so he missed the main difficulty, which requires "a deep immersion in the theory of single-valued functions". Worse still, Weierstrass concluded, with such a mixture of theories "all systematic unity gets lost". Further criticism was addressed to the way in which Koenigsberger dealt with the representation of the relevant Riemann surface on a parallelogram, but there was no need to explain the matter in detail to a "master of representation" like Schwarz.

Eventually Weierstrass preferred to stop this "unpleasant criticism", and to turn instead to a description of his own work. In preparing a "draft" of the planned collection of elliptic formulae he recognised that, as a preliminary, he needed to draw up the elements of the theory of single-valued functions. He had always refrained from doing this because the piece of the foundation was still missing that would establish the theorem that "any single-valued function of *one variable* (Weierstrass's italics), which has only one limit-point at infinity, can be represented as the quotient of two everywhere convergent power series in such a way that numerator and denominator do not both vanish for any value of the argument". The foundation of this, and other related theorems—which Weierstrass had been looking for years "always following a wrong route"—was provided by a theorem establishing the existence of an entire, transcendental function having prescribed zeros. Eventually he succeeded in proving it by means of the theory of "prime functions" (see Sect. 6.7), which he proceeded to summarise. This allowed him to get the sought-for representation theorems (see Sect. 6.7) that he had collected in "a nice, small treatise" he planned to publish during the winter.⁷¹ In the meantime he was presenting the theory in his WS 1874/75 lectures on elliptic functions. In the

⁷¹The "small treatise" announced here eventually appeared as [Weierstrass \(1876b\)](#).

audience of that course there was Gösta Mittag-Leffler, a young Swedish researcher who took careful notes of it.

6.6.1 Mittag-Leffler's Lecture Notes

Mittag-Leffler had graduated from the University of Uppsala in 1872 with a thesis on the applications of the argument principle. Thereafter he obtained a position at that university with the requirement that he spend three years studying abroad. Accordingly, in October 1873 Mittag-Leffler took himself to Paris, where he spent the academic year 1873–1874. He followed Hermite's lectures on elliptic functions, took careful notes of them, and in his diary he recorded how difficult they were for him to understand.⁷²

By the 1870s Hermite had risen to a position of prominence in the Parisian mathematical world (Fig. 6.3). In 1869 he became a professor at the École Polytechnique and at the Faculté des sciences of the University of Paris. Perhaps more importantly he had married Louise, a sister of Joseph Bertrand in 1848. This was the beginning of a celebrated and influential “mathematical family”. Bertrand held positions at the Collège de France and the École Polytechnique and became the Permanent Secretary of the Académie des sciences in 1874. He was the nephew of Duhamel, another of Hermite's professors at the École Polytechnique. A niece



Fig. 6.3 Charles Hermite
(*Acta Mathematica Table
générale des tomes 1–3,
1882–1912*, p. 145)

⁷²The notes are kept in a booklet at the Mittag-Leffler Institute jointly with the notes on Weierstrass's course.

of Bertrand married Paul Appell, and one of their daughters married Émile Borel; a daughter of Hermite's married Émile Picard.

Hermite's mathematical eminence and his close ties to Bertrand were to give the pair considerable influence over promotions in the world of French mathematics. A whole generation of French mathematicians grew up following Hermite's courses at the École Normale, the École Polytechnique, and the Sorbonne, including among others Jordan, Darboux, Appell, Picard, Poincaré, Painlevé, Borel, and Hadamard.⁷³

As Hermite later wrote to Mittag-Leffler,⁷⁴ since 1843 "I have devoted myself entirely to the study of elliptic functions and of Gauss's *Disquisitiones arithmeticae*", and indeed he was highly regarded for his work on elliptic functions, for a series of papers on number theory, notably on quadratic forms, and for work on invariant theory. These overlapping topics were his field: "I cannot leave the elliptic domain", he wrote to Stieltjes on October 22, 1892, "as the proverb has it, where the goat is tethered, there she must graze".⁷⁵ But in fact he did more: in 1873 he proved that the number e is transcendental, and thereafter he wrote on differential equations and mechanics, as well as producing a series of lecture notes of his courses.

6.6.2 Hermite's Lectures on Elliptic Functions

According to Mittag-Leffler's notes Hermite's lectures were inspired by the approach he had first presented in his (unpublished) paper of 1849 (see Sect. 4.2.4), and then developed in detail in 1862 in the *Note* appended to the sixth edition of Lacroix's *Traité*.⁷⁶ Actually, his lectures could be considered as a summary of (part) of that *Note*.

Hermite began his lectures by introducing elementary transcendental functions such as the trigonometric and exponential functions. He established some properties of e^x , including its approximation via continued fractions, and gave a proof that e^x is irrational if x is an integer or a rational number. From this it follows, by considering $e^{i\pi}$, that π and π^2 are both irrational numbers. Bessel functions were the next transcendental functions mentioned by Hermite. After devoting "some words" to them he came to Riccati's differential equation whose solutions he briefly discussed.

Then he turned to trigonometric functions, focusing on their periodicity, and in lecture 6 he stated Jacobi's "remarkable" theorem that "a single-valued function cannot have more than two distinct periods and they cannot be both real" (see Sect. 4.3). Hermite added that "an essential condition" for the truth of the theorem is that the function be single-valued, and referred to Eisenstein (1845) for the

⁷³Apart from the obituaries (Darboux 1906) and (Noether 1902), the only full-length study of Hermite is (Brezinski 1990).

⁷⁴See Dugac (1984, 168). Also quoted in Goldstein et al. (2007, 377).

⁷⁵See Baillaud and Bourguet (1905, 2, 270).

⁷⁶See (Hermite 1862).

“particular history” of this theorem. Hermite proved the first part of Jacobi’s theorem by resorting to a number-theoretic argument by Dirichlet, then he established the second part that “a uniform and well defined function cannot have three imaginary and distinct periods”.

Next he referred to Dirichlet to state that any single-valued, entire function that has a period a can be expressed in the form

$$F(x) = \sum_{m=-\infty}^{m=+\infty} a_m e^{\frac{2mi\pi x}{a}}.$$

Having proved Liouville’s theorem, he asked whether the quotient of two such functions could be a doubly periodic function. To this end, he considered the function

$$f(x) = \frac{\sum_{m=-\infty}^{m=+\infty} A_m e^{\frac{2mi\pi x}{a}}}{\sum_{n=-\infty}^{n=+\infty} B_n e^{\frac{2ni\pi x}{a}}}.$$

“One sees”, he said, that $f(x+a) = f(x)$. Thus, one has to determine the constants A_m and B_m in order that the series are convergent, and also $f(x+b) = f(x)$. Putting $q = e^{\frac{i\pi b}{a}}$ and following the same route as in his 1849 paper (and in his *Note* as well), he eventually obtained $f(x) = \frac{\Phi(x)}{\Pi(x)}$ where

$$\Phi(x) = \sum_{m=-\infty}^{m=+\infty} a_m q^{\frac{m^2}{k}} e^{\frac{2mi\pi x}{a}}, \quad \Pi(x) = \sum_{m=-\infty}^{m=+\infty} b_m q^{\frac{m^2}{k}} e^{\frac{2mi\pi x}{a}}. \quad (6.62)$$

In order that $f(x)$ has the period b he found that $\Phi(x)$ (and $\Pi(x)$ analogously) satisfies the “extremely remarkable” equality

$$\Phi(x+b) = e^{\frac{-ki\pi}{a}(2x+b)} \Phi(x). \quad (6.63)$$

Then he proved that $\Phi(x)$ (and $\Pi(x)$) are the “most general single-valued functions”⁷⁷ satisfying both the conditions $\Phi(x+a) = \Phi(x)$ and (6.63).

“Beginning with this theorem”, Hermite then stated “one can prove all the other theorems of elliptic function theory”. Indeed, by putting $k = 1, 2$ in $\Phi(x)$ (6.62) and introducing Jacobi’s notations with $a = 4K, b = 2iK$ Hermite obtained Jacobi’s theta functions $\Theta_1(x)$ and $H_1(x)$ as well as $\Theta(x)$ and $H(x)$ and established some of their properties.

“I have pointed out many times” Hermite observed in lecture 11 “that in my opinion one finds the natural link between algebra and number theory in the theory of elliptic functions”. The first occurrence of it was given by the equality

⁷⁷Hermite first introduced these functions in his letter to Jacobi on August 1844 (see Hermite’s *Œuvres* 1, 18). They were called “intermediary functions” by Briot and Bouquet (1875, 236).

$$\left(1 + 2 \sum_{n=1}^{\infty} q^{n^2}\right)^4 = \left(2 \sum_{n=0}^{\infty} q^{\frac{(2n+1)^2}{4}}\right)^4 + \left(1 + 2 \sum_{n=1}^{\infty} (-1)^n q^{n^2}\right)^4, \quad (6.64)$$

which allowed Hermite to establish Fermat's theorem that twice the number of decompositions of an odd, positive number N as the sum of 4 squares is the number of decompositions of $4N$ in the sum of 4 odd squares.

Then he turned to the inversion of the elliptic integral of the first kind, introduced Jacobi's notations $\text{sin}am u$, $\text{cos}am u$, $\Delta am u$ for the elliptic functions, then the "more convenient" $\text{sn}u$, $\text{cn}u$, $\text{dn}u$ of Gudermann (and of Briochi), and eventually established their double periodicity and the addition formulae for them. "Liouville told me"—Hermite said at this point—"that Lagrange had proposed to Poisson as a question of great importance to try to find all the solutions of the equation $\text{sn}x = 0$. Had Poisson been able to solve this question he would have necessarily found at the same time the solution of $\text{sn}x = \infty$, and thus all the theory would have been available. For, as an immediate consequence of these two equations one obtains the functions of $\Theta(x)$ and $H(x)$ which, once they are found, give all the theory".

In lecture 17 Hermite established what we have already met as formula (6.55)

$$\sqrt{\frac{2K'}{\pi}} = \Theta_1(0) = 1 + 2 \sum_{n=1}^{\infty} q^{n^2},$$

which, because of the equations

$$\sqrt{K} = \frac{H_1(0)}{\Theta_1(0)} \text{ and } \sqrt{K'} = \frac{\Theta(0)}{\Theta_1(0)},$$

yields

$$H_1(0) = 2 \sum_{n=0}^{\infty} q^{\frac{(2n+1)^2}{4}}, \quad \Theta(0) = 1 + 2 \sum_{n=1}^{\infty} (-1)^n q^{n^2}. \quad (6.65)$$

These expressions, Hermite remarked, "have the peculiarity of existing only for $|q| < 1$ ", and had been Kronecker's starting point for his search for analytic expressions of quadratic forms. (This could also explain how Kronecker came across to the series (6.55), and discovered the peculiar properties he explained to Casorati).

In the subsequent lectures Hermite developed the elliptic functions $\text{sn}2Kx/\pi$, $\text{cn}2Kx/\pi$, and $\text{dn}2Kx/\pi$ in series of sn and cn of multiple arcs as Jacobi had done, and in doing so he re-obtained and proved Gauss's "remarkable theorem" that "the number of representations of any number N as the sum of two squares equals four times the excess of the numbers of the divisors of N congruent to 1 over the numbers of the divisors of N which are congruent to -1 ".

In lecture 22 he presented the expansions of $\text{sn}x$, $\text{cn}x$, and $\text{dn}x$ in trigonometric series by purely algebraic means, "an extremely remarkable and simply discovery belonging to Liouville". It was only in lecture 24 that, "aiming at treating all

doubly periodic functions from a common point of view”, he *defined* a single-valued function of a variable x as a function “that can be expressed by the quotient of two series ordered according to the increasing multiples of the variable, and convergent for any value of it”. This contrasts strikingly with Weierstrass’s view that this was the theorem characterising “single-valued” functions whose “true” foundation he was looking for at that time.

Returning to equations (6.62), Hermite observed that he had considered only the case $k = 2$ so far. The case $k = 1$ “makes of $\Pi(x)$ a sole and unique function with remarkable properties”. He turned to the study of $\Pi(x)$ for $k = 1$ with the aim of reducing any doubly periodic function to it. He began by establishing a priori the number of solutions of the equation $\Pi(x) = 0$, and reduced this problem “to the principles on which Cauchy has based modern analysis”, namely the calculus of residues. Taking $F(x) = \Pi'(x)/\Pi(x)$ one has

$$\int_{PQRS} F(x)dx = 2\pi i\Delta,$$

$PQRS$ being the lattice of the periods and Δ the sum of the residues of $F(x)$ within it. Because $F(x+a) = F(x)$, $F(x+b) = F(x) - 2ki\pi/a$ and $k = 1$, it follows that $\Delta = 1$ and the function has only one zero, “a fact of an extreme importance”. Starting from it one can get all doubly periodic functions with the properties of

1. Having a finite number of infinities within the period lattice and
2. “Being of such a nature that Cauchy’s theorem on the roots can be applied”.

To this end Hermite considered the function

$$\Theta(z) = \sum q^{m^2} e^{2mi\pi z/a}.$$

and supposed that P lay at the origin, so $\Theta(\frac{a+b}{2}) = 0$. Then, “introducing a new principle”, he defined the function

$$Z(z) = \frac{\Theta'(z - \frac{a+b}{2})}{\Theta(z - \frac{a+b}{2})}, \quad (6.66)$$

which is not a doubly periodic function, but is such that “all other doubly periodic functions can be reduced to it”, as he showed in the remaining part of this lecture.

In lecture 25 he developed $Z(z)$ in trigonometric series, and in the next one he explained how “to obtain the theory of elliptic functions from another source”, namely by considering them as defined as quotients of the previously introduced Jacobian functions $\Theta(x)$ and $H(x)$. They coincide with Weierstrass’s Al -functions, Hermite mentioned in passing. Following Jacobi, as an application he proved the Poncelet closure theorem (see Sect. 1.4) and re-obtained Legendre’s identity (1.16).

Eventually in lecture 27 he turned to the expressions of the elliptic integrals of the second and third kind. Jacobi's "canonical form" of the latter is given by

$$\int_0^x \frac{k^2 \operatorname{sn} a \operatorname{cn} a \operatorname{dn} a \operatorname{sn}^2 x}{1 - k^2 \operatorname{sn}^2 a \operatorname{cn}^2 x} dx = \frac{1}{2} \log \frac{\Theta(a-x)}{\Theta(a+x)} + x \frac{\Theta'(a)}{\Theta(a)}.$$

Commenting on this, Hermite remarked that the integral seems to involve the quantities x , a , and k but in fact it depends only on x and a . This, Hermite added, "is perhaps the most beautiful of Jacobi's discoveries", thus repeating almost word for word what he wrote in his *Note* appended to Lacroix's book.⁷⁸

In lecture 28 he tackled a question which had been asked for years by the French Académie, namely whether it is possible to expand any function in series by means of the three elliptic functions in analogy with the expansion of a function in trigonometric series. Through a number of skillful transformations Hermite reduced the problem to the integration of the second order ordinary differential equation

$$y'' = y \left(2k^2 \operatorname{sn}^2 x + \frac{1}{\operatorname{sn}^2 a} - 1 - k^2 \right),$$

and remarked that "the problem of the expansion of any function in series involving elliptic functions can now be solved in the same way as the analogous problem for the trigonometric series is solved by the equation $y'' = cy$ ", with c a constant. He first proved that two particular solutions of the differential equation above are

$$y = \frac{\Theta(x+a)}{\Theta(x)} e^{-x \frac{H'(a)}{H(a)}}$$

and

$$y = \frac{\Theta(x-a)}{\Theta(x)} e^{x \frac{H'(a)}{H(a)}}.$$

Then he showed that the sought-for expansion of a function $f(x)$ is given by a series of the form

$$f(x) = \sum A \frac{\Theta(x+a)}{\Theta(x)},$$

where a are the (infinitely many) roots of the equation $H'(a) = 0$ and devoted the concluding lectures to a careful study of them.

To sum up, the theory of elliptic functions as presented by Hermite in these lectures was essentially based on Jacobi's theta series approach embedded in a French setting which included Cauchy's, Liouville's and Hermite's own results, with a particular attention paid to the number-theoretical aspects.

⁷⁸See [Hermite \(1862, 201\)](#).

6.6.3 *Weierstrass's Lectures on Elliptic Functions*

In the course of the year Mittag-Leffler was able to establish friendly relationships with Hermite who, to his surprise, in the end addressed him with the words: “You have made a mistake, Monsieur, you must follow the course of Weierstrass in Berlin; he is the master of all of us”.⁷⁹

Following Hermite's advice, Mittag-Leffler spent the next term in Berlin where he attended Weierstrass's WS 1874/75 lectures on elliptic functions, and Kronecker's lectures on algebraic equations and number theory. Hermite, he decided, was right: “Nowhere I have found so much to learn as here”, Mittag-Leffler wrote enthusiastically on February 19, 1875 to Holmgren, his teacher in Stockholm.⁸⁰ Contrary to the German habit, he went on, Weierstrass and Kronecker avoid printed publications as much as possible. “Notoriously, Weierstrass does not publish, and Kronecker only results without proofs. Both of them present the results of their research in their lectures”.

After remarking that Weierstrass's approach was “completely analytical”, and geometry occurred only occasionally as an illustration, Mittag-Leffler added

this seems to me have a great advantage over Riemann's school, and also with respect to Clebsch's. Well, it might be true that a function theory can be built quite rigorously on Riemann surfaces as starting point, and that Riemann's geometric system suffices to clarify the properties of Abelian functions known so far, but on one hand it does not suffice to reproduce the properties of transcendental [functions] of higher order, on the other hand in this way elements are brought in function theory that in the end are foreign to it.

Another distinctive feature of Weierstrass's approach was his avoidance of general definitions, and all the theorems dealing with functions in general. “To him a function is a power series, and all is deduced from the power series”. This appeared as “an extremely difficult route” to Mittag-Leffler who thought, perhaps under the influence of his recent French experience, that one could reach the goal more easily by starting with general, although fully rigorous, definitions as Cauchy and Liouville had done.

As for Weierstrass's style of teaching, Mittag-Leffler stated that “from a purely formal point of view it is beneath any criticism”, adding that even the “most insignificant French mathematician would consider himself a completely incapable teacher if lecturing that way”. However, when after a great deal of difficult work one succeeds in reproducing a lecture by Weierstrass in the form that he had conceived it, then “all becomes clear, plain and systematic”. Perhaps, Mittag-Leffler guessed, this “remarkable lack of formal talent” explains why so few of Weierstrass's students have understood him completely, and why the mathematical literature inspired by

⁷⁹“Vous avez fait erreur, Monsieur, vous auriez du suivre les cours de Weierstrass à Berlin. C'est notre maître à tous!” (Mittag-Leffler 1902a, 131). Mittag-Leffler repeated this in his (1923b, 133) which was an extended version of his (1902a).

⁸⁰The quotations are taken from the German translation of this letter published in (Behnke and Kopfermann 1966, 54–55).

his approach is “still so insignificant”. However, “this does not at all prevent him from being regarded generally with an almost idolising reverence”.

As he had done in Paris, this time, too, Mittag-Leffler took notes that provide us with an extraordinary first-hand source with which to compare Weierstrass's and Hermite's contemporary treatment of the very same subject.

By comparison with his first course, in 1857, Weierstrass's approach to elliptic functions had changed markedly over the years. By 1874 all historical references had disappeared, and Weierstrass now began his lectures with an introductory part devoted to the theory of single-valued functions. Actually, he only gave a list of definitions and theorems (mostly without proof) which summarised (part of) the content of his lectures on the introduction to analytic function theory. Then, beginning with theorem 10, he presented the essentials of his most recent results related to the representation of single-valued functions, ending with the introduction of the prime functions (see Sect. 6.7).

The second part of Weierstrass's lectures opens with the question: Do single-valued functions $f(u)$ exist such that $f(u)$, $f(v)$ and $f((u+v)/2)$ satisfy an algebraic equation? The first theorem stated that every single-valued function $f(u)$, which has a limit-point at infinity and for which there is an algebraic equation between $f(u)$, $f(v)$ and $f((u+v)/2)$, is necessarily a periodic function. Then Weierstrass stated Jacobi's theorem that it is impossible for a single-valued function to have more than two (fundamental) periods. After mentioning the geometrical interpretation of the periods in the complex plane, Weierstrass proved that a function satisfying “a characteristic equation”, i.e. an algebraic addition theorem, “is either a rational function of the exponential function or a doubly periodic function”.

This theorem must have been stated by Weierstrass in previous lectures, because in a letter to him on 20 June 1872 Schwarz asked his master for permission to publish it in his paper “Ueber ebene algebraische Isothermen”, and this was granted. On the relevant page (Schwarz *Ges. Abh.* 2, 265) one reads that according to “a fundamental theorem that Weierstrass has stated and proved, and whose knowledge I owe to his lectures”, an analytic function of w having an algebraic addition theorem is either

1. (I) an algebraic function of w
2. (II) or an algebraic function of $e^{\mu w}$, μ being an arbitrary constant
3. (III) or eventually an algebraic function of an elliptic function in the proper sense, e.g. $\sin \operatorname{am}(\mu w; k)$.

Probably giving words to what his master thought, Schwarz added that “this theorem accounts for the analytic nature of elliptic functions even more characteristically than their double periodicity is able to do”.⁸¹

⁸¹In agreement with Hancock (1910, 33–34) one could even say that “with Weierstrass the problem of the theory of elliptic functions is to determine all functions of the complex argument for which there exists an algebraic addition theorem”.

The same fundamental theorem for analytic functions is also stated at the very beginning of Weierstrass's *Formeln und Lehrsätze*, condition III being expressed more precisely as:

“(III). or eventually an algebraic function of a function $\wp(w) = s$ satisfying the equation

$$\left(\frac{ds}{dw}\right)^2 = 4s^3 - g_2s - g_3,$$

where g_2 and g_3 are appropriate constants and $\wp(0) = \infty$ ” (Weierstrass 1883–1885, 2).

There it is also pointed out that III reduces to I when $g_2 = g_3 = 0$ and that III reduces to II when $g_2^3 - 27g_3^2 = 0$.

Among the analytic functions satisfying an algebraic addition theorem, the functions that “have the character” of entire or rational functions for finite values of the argument, and consequently are single-valued analytic functions, are of particular interest. For such functions $f(u+v)$ can be rationally expressed in terms of $f(u)$, $f(v)$ and their derivatives $f'(u)$ and $f'(v)$, and vice versa. In this case, the fundamental theorem holds in the form Mittag-Leffler reported, and that in *Formeln und Lehrsätze* (p. 3) is stated as $f(w)$ being either

1. A rational function of w , or
2. A rational function of $e^{w\pi i/\omega}$, with ω a suitable constant, or
3. A rational function of $\wp(w)$ and $\wp'(w)$.

the first two cases being special cases of the third one, as above.

In his lectures Weierstrass went on to ask if there is a doubly periodic function whose zeros and infinities can be arbitrarily prescribed. If such a function exists, he pointed out, it must have the form

$$C \exp \left(G(u) \frac{\sigma(u-u_1) \dots \sigma(u-u_v)}{\sigma(u-v_1) \dots \sigma(u-v_\mu)} \right), \quad (6.67)$$

where $u_1, \dots, u_v$ are the (simple) zeros, and $v_1, \dots, v_\mu$ the infinities of σ .

As a consequence of a representation theorem stated in the first part of the lectures, σ can be represented as

$$\sigma(u) = u \prod_{w \neq 0} \left(1 - \frac{u}{w} \right) \exp \left(\frac{u}{w} + \frac{1}{2} \frac{u^2}{w^2} \right), \quad (6.68)$$

where $w = 2\mu\omega + 2\nu\omega'$, 2ω , and $2\omega'$ being the fundamental periods, and μ, ν integers. Reversing the order in the *Formeln und Lehrsätze*, in the lectures he immediately derived from $\sigma(u)$ the function $\wp(u)$ which “is one of the most important [functions] of the whole analysis” and is defined as

$$\wp(u) = -\frac{d^2}{du^2} \log \sigma(u) = \frac{1}{u^2} + \sum_{w \neq 0} \left(\frac{1}{(u-w)^2} - \frac{1}{w^2} \right), \quad (6.69)$$

where $w = 2\mu\omega + 2\nu\omega'$, as above. By direct inspection one realises that $\wp(u)$ is an even function (and $\sigma(u)$ is an odd one), that it is doubly periodic and has only one infinite point of order two in the period parallelogram.

Having established a number of properties of $\sigma(u)$, Weierstrass resumed his original question and pointed out that “a function which everywhere in the finite part of the plane has the character of an entire function cannot be a doubly periodic function” (Theorem 10), and that “Every doubly periodic function can be represented in the form

$$\varphi(u) = C \frac{\sigma(u-u_1) \dots \sigma(u-u_v)}{\sigma(u-v_1) \dots \sigma(u-v_v)}, \quad (6.70)$$

where the u 's and v 's can be chosen in such a way to build two complete systems of non-congruent magnitudes (i.e. the difference of any two of them is not a (multiple of a) period) that satisfy the condition $\sum u_v = \sum v_v$ (Theorem 13). Such a function is doubly periodic, and it is called an elliptic function of degree v , and “there are only elliptic functions of degree 2 and above”.

Then he proved that $\wp(u+v)$ and $\wp(u-v)$ can be algebraically expressed in terms of $\wp(u)$, $\wp(v)$ and their derivatives, and that $\wp(u) = s$ is a solution of the differential equation

$$\left(\frac{ds}{du}\right)^2 = 4s^3 - g_2s - g_3,$$

where $g_2 = 60 \sum (\frac{1}{2w})^4$ and $g_3 = 140 \sum (\frac{1}{2w})^6$. Thus, returning to (6.70), Weierstrass observed that the “simplest elliptic function” can be represented as the function $C \frac{\sigma(u-v)\sigma(u+v)}{\sigma(u)^2}$ which, taking into account the properties of $\wp(u) - \wp(v)$, yields

$$\wp(u) - \wp(v) = \frac{\sigma(u-v)\sigma(u+v)}{\sigma(u)^2\sigma(v)^2}.$$

He then proved that $\wp(u)$ satisfies an algebraic addition theorem (Theorem 16) and in Theorem 18 he stated and proved that if $f(u)$ is an elliptic function with periods $2\omega, 2\omega'$ then it can be expressed rationally in terms of $\wp(u)$ and $\wp'(u)$, and vice versa $\wp(u)$ can be expressed rationally in terms of $f(u)$ and $f'(u)$. Eventually, he asked himself the “important questions”: Given arbitrary g_2 and g_3 can suitable ω and ω' always be found so that there is a $\wp(u)$? If so, how can ω and ω' be computed from g_2 and g_3 ?

As he was to do in *Formeln und Lehrsätze* Art. 9, to answer the questions he introduced the quantities e_1, e_2, e_3 such that

$$\wp'(u)^2 = 4(\wp(u) - e_1)(\wp(u) - e_2)(\wp(u) - e_3),$$

established some further properties, and concluded his lectures by discussing the cases $g_2^3 - 27g_3^2 \neq 0$, and $g_2^3 - 27g_3^2 = 0$ thus completing the proof of the fundamental theorem on single-valued functions admitting an algebraic addition theorem.

Looking at Weierstrass's lectures as reported by Mittag-Leffler, one is struck by their rigorous unity of approach. The inversion of the elliptic integrals has virtually disappeared, as have Jacobi's theta-series. Weierstrass's theory of elliptic functions is basically the theory of the function $\wp(u)$. The comparison with Hermite's rather "eclectic" approach is striking. Indeed, juxtaposing Hermite's lecture notes, where the motivations and aims are clearly explained, and the calculations are developed in detail, with Mittag-Leffler's scanty notes of Weierstrass's lectures suggests an interesting comparison of the lecturing "style" of these two masters of nineteenth-century analysis.

"Herr Weierstrass has given a very complete course of 6 hours a week on elliptic functions" Mittag-Leffler reported to Hermite on June 21, 1875. "It seems to me that in the interest of science one could not regret enough that he does not publish his nice researches on this theory, which without any doubt would interest you in the highest degree".⁸² With some sycophancy Mittag-Leffler added that he had very often been "extremely struck" by the great similarity between Hermite's and Weierstrass ideas. "It is surprising that Herr Weierstrass and Herr Kronecker can find so many listeners—15 to 20—for such difficult and high-level courses". After expressing his deep gratitude for receiving an offprint of (Hermite 1873a), Mittag-Leffler reported that according to Kronecker this was "an epoch-making work in the history of science".

Mittag-Leffler's regret over the lack of publication of Weierstrass's lectures was not to be overcome until 1915, when vol. 5 of Weierstrass's *Werke* appeared in print, devoted to elliptic functions. In fact, among the other listeners to Weierstrass's WS 1874/75 lectures was Johannes Knoblauch who also took careful notes of them. These notes later formed the main part of the volume of Weierstrass's *Werke* that Knoblauch edited, while the first nine chapters of it were based on a manuscript that Weierstrass "dictated to Herr Mertens in 1863".

There Weierstrass remarked that beginning the theory of elliptic functions with their "main property", i.e. the algebraic addition theorem, would require the preliminary knowledge of a number of theorems of general function theory. Therefore, he followed instead "the historical route" beginning with the inversion of the elliptic integral of the first kind $\int dx/\sqrt{R(x)} = u$. This amounts to integrating the differential equation $(dx/du)^2 = R(x)$ whose integral satisfies an algebraic addition theorem, as Euler had shown.⁸³ Then he proved that the form of the elliptic differential $dx/\sqrt{R(x)}$ is invariant under Möbius transformations. Following this he obtained the two fundamental invariants g_2 and g_3 of $R(x)$, and introduced the $\wp(u)$ as a solution of $(ds/du)^2 = 4s^2 - g_2s - g_3$. Having defined the function $\sigma(u)$, and established its properties, in the subsequent chapters he developed the theory of the functions $\sigma(u)$ and $\wp(u)$ as he had done in his WS 1874/75 lectures.

⁸²Quoted in Dugac (1973, 153).

⁸³So, ironically for a man who aimed at avoiding the transcendental method, including the integral, "the historical route" he chose to follow led him to build the theory of elliptic functions on a non-linear differential equation.

6.6.4 Formeln und Lehrsätze

In a letter to Weierstrass on January 31, 1881 Schwarz lamented that a suitable “collection” of formulae on elliptic functions according to his master’s approach was still lacking, and expressed once more—as he had done occasionally in his letters to Weierstrass since 1874—his wish to have such a thing for his students. To this end he had himself produced a first draft about which he asked for his master’s advice. This was the true beginning of their joint work on the subject. In his reply to Schwarz on March 6 Weierstrass confirmed his agreement, and one month later on April 9 he promised to send Schwarz a collection of the essential theorems related to the theory of transformation of elliptic functions. The work proceeded quickly, and on May 19 Weierstrass was proud to write Schwarz that his 138 listeners were “eagerly” looking for the first printed leaves of the booklet.

However, things went more slowly than expected, and the intense correspondence they exchanged is rich in details about the actual production of the booklet until March 9, 1883 when Weierstrass asked Schwarz to have 100 copies for the beginning of his summer semester lectures.

Contrary to what Weierstrass had planned to do in 1874, when the *Formeln und Lehrsätze* eventually appeared in print they had neither a methodological introduction nor historical and technical motivations. The booklet contains exactly what its title announces, a collection of “formulae and theorems” most of them stated without any proof. Only a handful of references were given, and the theory was presented in a definitive and final form.

The booklet entered the subject directly by establishing theorems on functions satisfying an algebraic addition theorem. If $\phi(u)$ satisfies an algebraic addition theorem, then an algebraic equation links $\phi(u)$ with its derivative $\phi'(u)$, and vice versa. In addition, such a $\phi(u)$ is a root of an algebraic equation whose coefficients are single-valued functions of u that “have the character of a rational function” for finite values of u .

Then the above-quoted fundamental theorem was stated that characterises functions satisfying an algebraic addition theorem. It was also stated that any transcendental analytic function possessing an algebraic addition theorem is necessarily a simply or doubly periodic function. The functions $\sigma(u)$ and “the most simple doubly periodic function” $\wp(u)$ were defined, and their properties were established as Weierstrass did in his lectures, but in much greater detail.⁸⁴

In addition, having introduced elliptic functions of arbitrary degree v as he did in his lectures, Weierstrass showed how they could be represented by means of

⁸⁴“What a singular and remarkable thing that $\wp(u)$ lay at the origin of so many unexpected, analytical relations that completely justify its introduction as an indispensable element having its own role distinct from the transcendentals $\operatorname{sn}x$, $\operatorname{cn}x$, and $\operatorname{dn}x$ ”, Hermite wrote to Stieltjes on August 7, 1893 after the publication of the French translation of the *Formeln und Lehrsätze* (in Baillaud and Bourguet, 1905, 2, 328).

the function $\frac{\sigma'(u)}{\sigma(u)}$ and its derivatives (Art. 14–16). Then he considered the three functions $\sigma_1(u)$, $\sigma_2(u)$, $\sigma_3(u)$ such that

$$\wp(u) - e_1 = \left(\frac{\sigma_1(u)}{\sigma(u)} \right)^2, \quad \wp(u) - e_2 = \left(\frac{\sigma_2(u)}{\sigma(u)} \right)^2, \quad \wp(u) - e_3 = \left(\frac{\sigma_3(u)}{\sigma(u)} \right)^2$$

and established their properties (Art. 18–21). This was followed by the transformation formulae for the functions $\sigma(u)$, $\sigma_1(u)$, $\sigma_2(u)$, $\sigma_3(u)$ and the transformation formulae for the quotient of any two of them, as well as the second order differential equation they satisfy (Art. 22–25).

Assuming (Art. 26) that the modulus k of Jacobi's elliptic functions is such that

$$k^2 = \frac{e_2 - e_3}{e_1 - e_3},$$

it was then shown that

$$\begin{aligned} \frac{\sigma(u)}{\sigma_3(u)} &= \frac{1}{\sqrt{e_1 - e_3}} \sin \operatorname{am}(\sqrt{e_1 - e_3}u, k) \\ \frac{\sigma_1(u)}{\sigma_3(u)} &= \cos \operatorname{am}(\sqrt{e_1 - e_3}u, k) \\ \frac{\sigma_2(u)}{\sigma_3(u)} &= \Delta \operatorname{am}(\sqrt{e_1 - e_3}u, k). \end{aligned} \tag{6.71}$$

The introduction of the complete elliptic integrals K and K' allowed Weierstrass (Art. 27) to express the periods of $\wp(u)$ in terms of them and to re-obtain (Art. 29) Legendre's identity (see (1.16)). Then (Art. 30) he presented the infinite product expansions of $\sigma_1(u)$, $\sigma_2(u)$, $\sigma_3(u)$. In Art. 34 he introduced the θ -functions and expressed the $\sigma(u)$ -functions in terms of them before developing the transformation theory of the θ -functions (Art. 36–37).

He discussed in great detail special cases of real invariants g_2 and g_3 (in Art. 44–50), and eventually (Art. 51–55) he presented some examples of conformal representations by means of elliptic functions before turning to the normal form of elliptic integrals (Art. 56–57) and their addition theorems and periods (Art. 58–61).

As this short synopsis shows, there is no doubt that the *Formeln und Lehrsätze* contain many developments of the material that Weierstrass had presented in his lectures on elliptic functions, even though there was nothing conceptually new in this booklet for someone who, like Mittag-Leffler, had followed his courses.

However, in spite of the large number of Weierstrass's students, still they formed a relatively close-knit circle, and Schwarz was right in emphasising the need for the mathematical world to become acquainted with Weierstrass's methods. The booklet in fact played a much more significant role than did his lectures in making Weierstrass's approach to the subject known to mathematicians all over the world. The first printing was very soon sold out, and the booklet was reprinted in a (slightly)

revised version in 1893 and translated into French that year. Thanks to that booklet, Weierstrass's $\wp(u)$ function became universally known, and its theory eventually became the standard way of presenting elliptic functions as it has remained ever since.

6.7 Representation Theorems

On the same day, December 16th, 1874, Weierstrass communicated to his closest students, Schwarz and Sonya Kovalevskaya, that he had finally succeeded in overcoming a major difficulty which had prevented him from building a satisfactory theory of single-valued functions of a complex variable for a long time. To be precise, he had established a representation theorem expressing a single-valued function as a quotient of two convergent power series. As he explained to Schwarz, and in almost the very same words to Kovalevskaya (see Bölling 1993, 160–162 and Mittag-Leffler 1923b, 150–152), his starting point was the following question: Given an infinite sequence of complex constants $\{a_n\}$ with $\lim |a_n| = \infty$ is there an entire, transcendental function $G(x)$ which vanishes at the points $\{a_n\}$ and only those, and in such a way that each of the constants $\{a_j\}$ is a zero of order λ_j say, if $\{a_j\}$ occurs λ_j times in the sequence? He had been able to find a positive answer by assuming that $a_n \neq 0$ for any n and by associating with the given sequence a sequence of positive integers $\{v_n\}$ in such a way that $\sum_{i=1}^{\infty} \left(\frac{x}{|a_i|}\right)^{v_i+1} < \infty$.

“This is always possible”, Weierstrass affirmed. Let $v_n = n - 1$ and consider the “prime functions” $E(x, 0) = 1 - x$ and

$$E(x, n) = (1 - x) \exp \left(\frac{x}{1} + \frac{x^2}{2} + \cdots + \frac{x^n}{n} \right), \quad (6.72)$$

which he introduced here for the first time.⁸⁵ The infinite product

$$\prod_{n=1}^{\infty} E(x/a_n, n) \quad (6.73)$$

is convergent for finite values of x , and represents an analytic function that has “the character of an entire function” and vanishes in the prescribed way. The representation theorem followed easily: every single-valued analytical function $f(x)$ that “has the character of a rational function” for every finite value of x can be represented as the ratio of two convergent power series in such a way that the numerator and denominator never vanish for the same value of x . This theorem, “full

⁸⁵In this connection Hilbert remarked that “the algebraic concept of prime ideal is realised by the transcendental, prime function” (Hilbert 1897b, 335).

of consequences” was until now “regarded as unproved in my theory of Abelian functions” Weierstrass admitted in his letter to Kovalevskaya quoted above. “I bestow my full admiration upon you for the proof of this fundamental theorem” that escaped some of your students who strove in vain to find it, Schwarz wrote in his turn to Weierstrass on February 6, 1875 asking for permission to present it in his lectures.

This, and related theorems, constituted the core of what Weierstrass called “a very nice, small treatise” that he presented to the Akademie on December 10, 1874 and originally intended to publish in the *Monatsberichte* for that month.⁸⁶ Instead, the material grew and Weierstrass presented an extended version of his small treatise to the Akademie on October 16, 1876.⁸⁷ This time, this seminal paper on the “systematic foundations” of the theory of analytic functions was eventually published.⁸⁸

6.7.1 Weierstrass’s Representation Theorems

Weierstrass began the treatise with some basic definitions. A single-valued complex function of one variable “behaves regularly” in the neighbourhood of a point a when it is bounded and continuous inside a certain disk centred at a . There the function can be expanded in power series. The set of the regular points is called the domain of continuity of the function. Then, Weierstrass continued, the theorem holds that “for every function $f(x)$ in the domain of the variable x there necessarily exist singular points, as I will call them, which are boundary points of the domain of continuity of the function, without themselves belonging to it” (Weierstrass 1876b, 78).

Weierstrass called a point a an essential singularity of a function if there is no integer n such that $(x - a)^n f(x)$ is regular at a . If there is such an integer n the point a is called an inessential singularity (later called a pole by other authors). The class of rational functions on a domain is characterised at the set of single-valued functions having only inessential singularities in that domain. In other words, Weierstrass went on, if a function is single-valued and has no essential singularity, then one can conclude that it can be represented as a quotient of two entire functions. This theorem gave Weierstrass the “clue” to the study and classification of transcendental functions.

Following the analogy suggested by rational functions, he asked the question “whether it is possible to form arithmetic expressions of the variable x and indeterminate constants which represent all the functions of a determinate class, and only those” (Weierstrass 1876b, 83). He provided a step-by-step answer, beginning

⁸⁶As recorded in the *Monatsberichte Berlin*, 766.

⁸⁷See the *Monatsberichte Berlin*, 537.

⁸⁸See (Weierstrass 1876b). It was reprinted some ten years later in (Weierstrass 1886).

with the easiest class to study, namely functions having only one singular point. If such a point is ∞ , then the function is entire and it is representable by a convergent power series for any finite value of x . The function itself is rational or transcendental according as ∞ is a pole or an essential singularity. Its general expression is given by

$$G\left(\frac{1}{x-c}\right), \quad (6.74)$$

where c is a pole or an essential singularity according as G is a polynomial or a transcendental entire function (if $c = \infty$, $1/(x-c)$ has to be replaced by x). This can be easily generalised to functions having n singularities (poles or essential singularities) c_n . Their “simplest” form is given by expressions like

$$\sum_{v=1}^n G_v\left(\frac{1}{x-c_v}\right), \quad (6.75)$$

or

$$\prod_{v=1}^n G_v\left(\frac{1}{x-c_v}\right) R^*(x) \quad (6.76)$$

“where $R^*(x)$ means a rational function becoming zero or infinite only at the essential singular points of the function to be represented” (Weierstrass 1876b, 85). Eventually, he considered the case of a function having n essential singularities c_n and, in addition, arbitrarily many (even infinitely many) poles. It can be expressed in the form

$$\frac{\sum_{v=1}^n G_v\left(\frac{1}{x-c_v}\right)}{\sum_{v=1}^n G_{n+v}\left(\frac{1}{x-c_v}\right)} \quad (6.77)$$

or

$$\frac{\prod_{v=1}^n G_v\left(\frac{1}{x-c_v}\right)}{\prod_{v=1}^n G_{n+v}\left(\frac{1}{x-c_v}\right)} R^*(x) \quad (6.78)$$

under the obvious condition that “the numerator and denominator do not both vanish for any value of x ”. Conversely, when the functions $G_1, \dots, G_{2n}$ are given arbitrarily, each such expression represents a single-valued function with (at most) n essential singular points and an unlimited number of poles. The proof of these theorems constitutes the main part of the paper.

Admittedly, (6.74) was already known and (6.75) could be easily obtained “from known theorems”. In order to prove the remaining formulae “in general”, he had to fill a “gap” in the theory of entire, transcendental functions, and, he said, he had succeeded “not long ago, after some unsuccessful attempts” (Weierstrass 1876b, 85).

This was related to the answer to the questions he had discussed in his letters to Schwarz and Kovalevskaya that [Weierstrass \(1876b, 86\)](#) reformulated as follows:

1. Is an entire function determined by its zeros?
2. given an infinite sequence of constants $\{a_n\}$ with $\lim |a_n| = \infty$ does there always exist an entire function having $\{a_n\}$ as its zeros?

The first question is easily answered: there are infinitely many functions having the same zeros as a given $G(x)$. They are represented by $G(x)e^{\bar{G}(x)}$, $\bar{G}(x)$ being any arbitrary entire function. As for the second, so far unanswered question—Weierstrass stated—it too has an affirmative answer. “With the help of these fundamental theorems”, Weierstrass went on, one will easily get a proof of (6.77) and (6.78).

In the paper Weierstrass added that he had been led to find an answer to these questions by his studies of the Gamma function, which he called Euler’s and Gauss’s factorial function. This is a meromorphic function, whose reciprocal is entire and which can be expressed by the “always convergent” infinite product

$$\prod_{n=1}^{\infty} (1 + x/n)(1 + 1/n)^{-x} \text{ or } \prod_{n=1}^{\infty} (1 + x/n)e^{-x \log(1+1/n)}.$$

“Starting from this remark” [Weierstrass \(1876b, 91\)](#) asked himself the question whether “any function $G(x)$ could be built from factors of the form”

$$(kx + l)^{\bar{G}(x)}.$$

Pursuing this idea he was eventually led to a result that allowed him to complete the theory of single-valued analytic functions with a finite number of essential singularities in a satisfactory way. This was given by the concept of “prime functions”.

6.7.2 Prime Functions

Weierstrass defined a “prime function” of x as any single-valued function of x having only one singular point (either a pole or an essential singularity), and at most one zero. Its most general expression is

$$\left(\frac{k}{x-c} + l \right)^{G\left(\frac{1}{x-c}\right)},$$

where c is the singular point, k and l are constants. The special case when $G\left(\frac{1}{x-c}\right)$ is replaced by a constant is also allowed.

Then, Weierstrass stated, any single-valued analytic function with one singular point is either a prime function or the product of prime functions with this same

singular point. Formulae (6.76) and (6.78) could be obtained by multiplication and division of suitable prime functions.

With this aim in mind he introduced the prime functions $E(x, n)$ as above and proved a factorisation theorem that, following Hille (1962, 1, 228), we can reformulate in modern terms as follows: given the above sequence $\{a_n\}$ and a sequence of positive integers μ_n then there exists an entire function $G(x)$ having at $x = a_n$ a zero of multiplicity μ_n for each n and no other zeros. If $a_n \neq 0$ for any n , then the function is represented by the product

$$G(x) = \prod_{n=1}^{\infty} E\left(\frac{x}{a_n}, p_n\right)^{\mu_n}, \quad (6.79)$$

provided that the integers p_n are chosen so that the product is uniformly convergent on compact sets.

Thus, given any entire function $G(x)$ either

1. It has no zeros, and can be represented as $e^{\tilde{G}(x)}$, or
2. It has a finite number of zeros, and can be represented as $G_0(x)e^{\tilde{G}(x)}$, where $G_0(x)$ is a rational entire function, or
3. It has infinitely many zeros a_n , with multiplicity m_n at $x = a_n$ and multiplicity λ at $x = 0$, and it can be represented as $x^\lambda G(x)e^{\tilde{G}(x)}$, $G(x)$ being given by (6.79)

where, in all cases, $\tilde{G}(x)$ is an entire function.

Having proved this, Weierstrass turned to the study of single-valued functions with one essential singularity (resp. n essential singularities, and either n or arbitrarily many poles) and established the relevant expressions (6.74)–(6.78) for them.

6.7.3 The Casorati–Weierstrass–Sokhotskii Theorem

In the concluding paragraph of the paper he tackled the problem of the behaviour of a function in the neighbourhood of an essential singularity. “One knows”, Weierstrass remarked, that a single-valued, entire function $f(x)$ is such that for arbitrarily large, positive constants a and b there always exist values of $|x| > a$ such that $|f(x)| > b$. This holds true in particular for any single-valued function with an essential singularity at ∞ . Now, let $f(x)$ be any single-valued function with $n \geq 1$ essential singularities, and let c be one of them. Then, given any arbitrarily small $\rho > 0$ and any arbitrarily large $R > 0$ there exist values of x such that $|x - c| < \rho$ and $|f(x)| > R$. Given any arbitrary constant C , the function $\frac{1}{f(x) - C}$ has the same essential singularities as $f(x)$. Thus, there exist values of x such that $|x - c| < \rho$ and $|f(x) - C| < 1/R$.

Therefore, Weierstrass concluded, “in an infinitely small neighbourhood of the point c the function $f(x)$ behaves in such a discontinuous way that it can come arbitrarily close to any arbitrarily given value, for $x = c$ however, it has no determinate value” (Weierstrass 1876b, 124). Consequently, the relevant expressions of such a function given above “fail to have any meaning for $x = c$ ”.

In this way Weierstrass stated the theorem today named after him, Casorati, and Sokhotskii. As usual, Weierstrass avoided mentioning any other author even though he certainly knew of Casorati’s work. Indeed, the theorem had been independently found and published by Casorati, who based his proof on Cauchy’s integral formula.⁸⁹ Casorati had also sent a copy of his book to Weierstrass, who answered him on December 21st, 1868 that he had “looked at it carefully”. Casorati was well aware of the importance of this theorem, and in his private notes he lamented that he had been mentioned neither by Weierstrass—“who”, Casorati noted, “had certainly found the theorem by himself, but to whom I had sent my *Teorica* as a present in 1868”⁹⁰—nor by Hölder (1882), who proved the theorem by making an essential use of Cauchy’s integral theorem. According to what Schwarz told Casorati when they met in September 1880 at Lake Como in Italy, Weierstrass had known the theorem since 1863, when he proved that an entire transcendental function with an essential singularity at infinity takes values which are arbitrarily close to any given one. There is supporting evidence for this in Pasch’s lecture notes of Weierstrass’s WS 1865/66 course, and in Killing’s 1868 lecture notes this theorem is stated and proved by resorting to the Bolzano–Weierstrass theorem.

It should also be noted that in conversations in 1864 with Casorati (see Sect. 6.4.1), Weierstrass first sketched the stereographic representation of the complex plane on a sphere (without mentioning Riemann). Then he added that when one thinks of two spherical surfaces, one for the independent variable and the other one for the values of a function, then “in the case of e^x a very small neighbourhood of ∞ on the first sphere would correspond to the whole, second spherical surface” (quoted in Neuenschwander 1978b, 80).

The Casorati–Weierstrass theorem represents an interesting case of multiple discovery. Indeed, contemporary with Casorati and Weierstrass, and independently of them, this theorem had also been found by the Russian mathematician Sokhotskii (see the Appendix to Chap. 3).

From 1875 onwards, Weierstrass presented the main results of this paper in his lectures on the introduction to analytic function theory. Thus, for instance, in Hurwitz’s notes (implicit) reference is made to the “clue” that provided the starting point for Weierstrass’s research when it is stated that “a rational function may well have singularities, but these can always only be inessential ones” i.e. poles. “But the converse of this property of a rational function also holds. And this converse is a

⁸⁹See (Casorati 1868a, 434–435) and (Casorati 1868b).

⁹⁰Quoted in (Neuenschwander 1978b, 7).

fundamental theorem of the theory of functions” (Weierstrass 1988a, 128). From this the representation of meromorphic functions followed. This proposition can also be found in Killing’s 1868 notes when dealing with “rational functions” (Weierstrass 1986, 83), but there is no hint there to the representations theorems that are treated in detail in Hurwitz’s lecture notes (Weierstrass 1988a, 147–152).

6.7.4 “A Promised Land”

The importance of Weierstrass’s “prime functions” can hardly be overestimated.⁹¹ They had a dramatic impact on Hermite when he first heard about them in 1877 on the occasion of the celebration of the one hundredth anniversary of Gauss’s birthday, when all the leading German mathematicians gathered in Göttingen. It was on this occasion that Hermite met Weierstrass for the first time, “whom by right the Germans call their mathematical hero”. Reporting on the meeting in a letter to Mittag-Leffler on April 23, 1878 Hermite recorded:

I was talking Mr. Schwarz about elliptic functions, and I received from him the notion of prime factors, a notion of capital importance and completely new to me. But scarcely the most essential things were communicated to me. Only for an instant, as if the horizon was unveiled and then suddenly darkened, did I glimpse a new, rich and wonderful country in analysis, a Promised Land that I had not entered at all. I had this vision in my mind continually during my entire journey back (in Dugac 1984, 52).

There is hardly any doubt that Hermite suggested to Picard that he should provide the French translation of Weierstrass (1876b) that appeared in 1879. In a subsequent letter to Mittag-Leffler on July 12, 1879, Hermite referred to this translation when he said again that “the completely new notion of prime functions and primary factors (*facteurs primaires*, in his wording) seems to me of capital importance in analysis. To me it was like a flash of light” (in Dugac 1984, 54). Hermite went on that he had used this notion in his lectures at the Sorbonne “without having studied or deepened it”, to prove that $\frac{1}{\Gamma(1+x)}$ is an everywhere holomorphic function,

$$\frac{1}{\Gamma(1+x)} = e^{Cx} \prod_{n \geq 1} \left(1 + \frac{x}{n}\right) e^{-\frac{x}{n}}$$

“which is the expression of a uniform, entire function in the form of a product of prime functions” (C is Euler’s constant).⁹² In spite of this, however, on August 2, 1879 Hermite confessed to Mittag-Leffler that he had “neither the strength nor the courage yet to make a deep study” of Weierstrass’s paper. Instead, he added, “I also

⁹¹In Poincaré’s (1899b, 8) opinion, the discovery of prime functions was Weierstrass’s main contribution to the development of function theory.

⁹²Hermite included this result in his lithographed lectures (Hermite 1882, 93).



Fig. 6.4 Gösta Mittag-Leffler with Poincaré, Landau, and Runge (*Collected Works of Edmund Landau* vol. 2, Thales Verlag ©1985)

think that the mathematical devil will come back and haunt you, and open for you an attractive perspective of studies and research on the path that Mr Weierstrass has enabled you enter, and which is so fruitful”.

6.7.5 Mittag-Leffler's Theorem

In 1875, during his stay in Berlin, Mittag-Leffler heard of an opening for a professorship in Helsingfors and decided to apply for it (Fig. 6.4). According to his subsequent recollections, when he paid a visit to Weierstrass and told him about his plans the latter reacted: “No, please, do not do that! I have written to the minister of culture and asked him to institute an extraordinary professorship for you here in Berlin and I just received the message that my application has been granted!” Of course, Mittag-Leffler went on, he “was not blind to the great advantages, mathematically speaking, of such a position compared to the one in Helsingfors. Hardly ever was there such a brilliant collection of distinguished mathematicians” as in Berlin, but “the conditions were not endurable for a foreigner”.

In the light of Mittag-Leffler's subsequent political positions, especially his taking the German side when the First World War broke out, it is amusing to read the following youthful statement of his:

It was not long after Germany's victorious war against France, and German arrogance was at a high point. Foreigners were treated with haughty condescension; *der grosse Kaiser*, Bismarck, Moltke etc. were words heard everywhere. It was taken as a matter of course that Holland, Sweden etc. would be members of a German Bund. For those who were not born Germans, it was impossible to live under such conditions. At least this is what I thought. Now things are different, the brilliant victory has not born the fruits that the Germans imagined and they have taken a more realistic view.⁹³

Be that as it may, at Weierstrass's suggestion Mittag-Leffler began working at a great paper dealing with the different methods that led to an analytical representation of elliptic functions (Mittag-Leffler 1876). This turned out to be the required Habilitationsschrift that eventually allowed him to obtain the professorship at Helsingfors in 1877.

In the meantime, inspired by Weierstrass (1876b) Mittag-Leffler presented several papers to the Swedish Academy⁹⁴ where he extended Weierstrass's representation theorems to functions having a finite number of poles with prescribed Laurent tails, and an accumulation point at ∞ . More precisely, he proved the following theorem: given an infinite sequence of complex numbers $\{a_n\}$ with $a_i \neq a_j$ for $i \neq j$ and $\lim_{n \rightarrow \infty} |a_n| = \infty$, and at each a_n ($n = 1, 2, 3, \dots$) an arbitrary polynomial

$$g_n \left(\frac{1}{x - a_n} \right) = \frac{A_1^{(n)}}{x - a_n} + \dots + \frac{A_{v_n}^{(n)}}{(x - a_n)^{v_n}}, \quad (6.80)$$

then there exists a single-valued analytic function $f(x)$ having poles only at points a_n with Laurent tails given by g_n , and regular otherwise for finite values of x . The function $f(x)$ can be represented as

$$f(x) = \sum_{n=1}^{\infty} \left(g_n \left(\frac{1}{x - a_n} \right) - \gamma_n(x) \right),$$

where $\gamma_n(x)$ is any polynomial and the series is uniformly convergent in any finite, bounded domain not containing any of the points a_n . Then the general function $F(x)$ with the prescribed properties is given by $F(x) = f(x) + G(x)$ where $G(x)$ is an arbitrary entire function.

⁹³We thank Arild Stubhaug for the following information: the quotation comes from a talk Mittag-Leffler gave on 9 November 1914 (after the War had begun), entitled "Memories from Finland", which was given to a very small and closed society called "De 17". This society consisted of 17 people who came together and created this society after Sonya Kovalevskaya was denied membership of the Royal Swedish Academy of Sciences in 1885 (initially there were only 13 members of the Society). The manuscript of the talk is kept at the Institute Mittag-Leffler, and it was published in the magazine *Normat* 2/2001 (pp. 176–182). See (Stubhaug 2010, 576).

⁹⁴*Öfversigt af k. Vetenskaps-Akademiens Förhandlingar* 1 (1877) 17–32; 33–43; 2 (1877) 31–41; 3 (1877) 5–15; 17–31.

6.7.6 Proofs by Weierstrass, Dini, Hermite, Schering

Although it was published in a language scarcely familiar to most mathematicians, Mittag-Leffler's theorem drew the attention of a number of them rather soon. On August 5, 1880 Kronecker read a paper submitted by Weierstrass to the Akademie in Berlin where a simplified proof of Mittag-Leffler's theorem was presented. Weierstrass (1880b, 189–190) reformulated Mittag-Leffler's theorem as follows: given an infinite sequence of complex numbers $\{a_n\}$ as above, and an infinite sequence of rational functions $\{f_n(x)\}$ such that for each n $f_n(x)$ becomes infinite only at a_n , and vanishes at $x = \infty$, then there always exists an analytic function $F(x)$ that has an essential singularity at ∞ and becomes infinite only at points $\{a_n\}$ in such a way that for each n the difference $F(x) - f_n(x)$ has a finite value at $x = a_n$. Therefore, in a suitable neighbourhood of the point $x = a_n$ it can be represented as $F(x) = f_n(x) + P(x - a_n)$, where P is a power series in $x - a_n$.

Mittag-Leffler had proved his theorem by showing that a new sequence of rational functions $\{F_n(x)\}$ could be obtained from the sequence $\{f_n(x)\}$ in such a way that the differences

$$F_n(x) - f_n(x), \quad n = 1, 2, \dots$$

are either entire functions or constants, and at the same time, the series

$$\sum_{n=1}^{\infty} F_n(x)$$

is uniformly convergent within any finite domain containing none of the points a_n . Thus, this series represents the sought-for function $F(x)$. Weierstrass showed that a simplification of the proof could be obtained by simplifying the way in which the functions $F_n(x)$ are produced. As Mittag-Leffler had done, Weierstrass also remarked that if $G(x)$ is either a polynomial or an entire function, then $f(x) = F(x) + G(x)$ is also a function with the prescribed properties. From this he deduced that any single-valued analytic function $f(x)$ having no essential singularity in the finite plane can be represented as a sum of rational functions, each of which has at most a pole in the finite plane. Then he reformulated Mittag-Leffler's theorem in the case when the essential singularity lay at a finite value $x = c$ instead of at ∞ . Eventually, by referring to the formula to (6.78) from his (1876b) paper, this result allowed him to obtain a “new general expression” for a single-valued analytic function $f(x)$ with a finite number of essential singularities as a series of rational functions that is uniformly convergent in any domain not containing (in either the interior or the boundary) any of the essential singularities of $f(x)$.

Unaware of Weierstrass's paper, in October 1880 Dini produced an independent proof of Mittag-Leffler's theorem. Dini (1881, 372) began by referring to the particular case of the representation theorem given by Betti in his paper (1860–1861) (see Sect. 5.11.5) and showed that Weierstrass's general theorem of 1876 could

be obtained from a suitable modification of it. Then he proved Mittag-Leffler's theorem, and in the concluding sections of his paper he treated some particular cases of both Weierstrass's and Mittag-Leffler's representation theorems under suitable hypotheses on the distributions of the zeros and the poles of the function. Answering a question asked by Hermite, in a letter on March 21, 1881 Mittag-Leffler stated that Dini's proof was the same as the one given by Weierstrass, or as "the one I gave one year before Weierstrass in a course at the university here" and rightly conjectured that Dini "did not know Weierstrass's paper when he published his work" (quoted in [Dugac 1984](#), 246).

Instead, Weierstrass's (1880b) paper had been studied "with the keenest interest" by Hermite, who in December 1880 wrote a long letter to Mittag-Leffler communicating to him a "slightly different method" of proving his theorem.⁹⁵ This was based on the idea of considering the logarithmic derivative of a function $\Phi(x)$, holomorphic in the whole plane, in such a way that the rational functions $f_n(x)$ reduce to $\frac{1}{x-a_n}$ (Hermite 1883, 48). Then, under suitable hypotheses he re-obtained Weierstrass's sequence of functions $\{F_n(x)\}$ such that $F(x) = \sum F_n(x)$ satisfies the condition required by Mittag-Leffler's theorem.

After remarking that his approach led to the splitting of the relevant holomorphic function into Weierstrass's prime functions, Hermite took this occasion to illustrate a new method for obtaining Weierstrass's expression of a function having a finite number of essential singularities and infinitely many poles as presented by his student Bourguet in his thesis ([Bourguet 1880](#)). Hermite's approach to obtaining Weierstrass's representation theorems from Mittag-Leffler's theorem by integrating logarithmic derivatives was adopted by some textbooks at the beginning of twentieth century. This provoked a strong reaction by Pringsheim in defense of Weierstrass's unity of methods:

But even if some textbooks now deviate so far from Weierstrass's methods that, using logarithmic integration, they derive the theorem in question as a consequence (!) of Mittag-Leffler's theorem (and in fact present this method not just in the form of an incidental, indeed quite obvious, remark but as the only and standard proof) this topsy-turvy way of doing things should not be sanctioned by anyone who sees mathematics as something other than a disordered heap of mathematical results ([Pringsheim 1915](#), 388).⁹⁶

In the second part of his paper Hermite turned to a problem related to Riemann's notion of *Querschnitt* (*coupure*, in Hermite's wording) that he had already sketched in a letter to Mittag-Leffler on November 1880 (see [Dugac 1984](#), 83–85). According to Hermite, the idea of *coupure* had an "entirely elementary origin" in the study of the integral

$$\Phi(z) = \int_{t_0}^{t_1} \frac{F(t, z)}{G(t, z)} dt,$$

⁹⁵See (Hermite 1883).

⁹⁶Quoted in ([Remmert 1998a](#), 131).

where t is a real variable, and $F(t, z)$, $G(t, z)$ are “real or imaginary” holomorphic functions of t and z . $\Phi(z)$ has a finite value everywhere except at points lying on the curve $G(t, z) = 0$. In Hermite’s view, this led “easily” to the notion of a *coupure*. Thus, for instance, given the integral

$$\Phi(z) = \int_0^{\infty} \frac{t^a \sin z}{1 + 2t \cos z + t^2} dt$$

one immediately recognises that the *coupures* are given by the straight lines $x = (2k + 1)\pi$, k being an integer. As the study of this integral went back to Legendre, Hermite (1881, 63) commented that a closer look at results known for a long time “could have led easily to the new analytical conceptions of our time”. In doing this, however, Hermite evidently did nothing else than dress up—without naming it—Cauchy’s old idea of *lignes d’arrêt* in the new analytic context provided by Weierstrass (1880c), a paper that had just appeared (see Sect. 6.9) and to which he referred. Eventually, in a concluding footnote Hermite presented an example of “function with lacunary spaces” found by one of his pupils, Poincaré.⁹⁷

This use of *coupures* was by this time something of an evasion, and in his (1887) Casorati reacted to Hermite’s paper by asking when will the idea of a Riemann surface become generally adopted as an effective tool in the study of many-valued functions? He noted that it was dismissed by Briot and Bouquet in their *Théorie des fonctions elliptiques* (1875) as “having many difficulties and ... no advantages for the task they had in mind”. His book (1868a) had set out overcome the difficulties, but now he found that no less a mathematician than Hermite had retreated to the weaker concept of *coupures* in his courses in Paris in 1882, 1883, and 1887. A *coupure*, Casorati quoted Hermite, was a line of discontinuity of the function. It is a line drawn in the complex plane with a view to restricting a many-valued function on the complex plane to a single-valued function defined on the plane with that line removed (that is to say, cut out). It is not the same concept as that of a *Querschnitt* as presented by Riemann in his paper on Abelian functions. That is a line drawn on a surface that is taken as the domain of a function. The surface may be then cut along this line and others and opened out to form a planar domain, if that is helpful. Without rehearsing here all the standard examples of Riemann surfaces, the distinction Casorati was pointing to can be illuminated by saying that a many-valued function, such as $\log$ or $\sqrt{x}$ is not discontinuous at points on the cut, and the way forward is not to restrict many-valued functions to single-valued ones on a cut domain but to pass instead to their Riemann surface.

“What a wonderful thing these discontinuous series of Mr. Weierstrass are that represent absolutely different functions in disjoint areas!”—Hermite had written

⁹⁷The relevant paper, sent to Mittag-Leffler for publication, appeared as (Poincaré 1883c). Poincaré was unaware of (Weierstrass 1880c), and ascribed the “first idea” of functions with “lacunary spaces” to Hermite (1881). Once Mittag-Leffler had pointed out Weierstrass’s priority, Poincaré was quick to recognise his “historical mistake”. See (Nabonnand 1999, 54–69, esp. p. 60).

enthusiastically to Mittag-Leffler on December 24, 1880. In the same letter, in a somehow philosophically inspired passage, Hermite wrote that in his view mathematical analysis was largely a “science of observation”, and mathematicians looked like naturalists, who with the eyes of the mind observe beings belonging to a world as real as the world of nature, a world that they have absolutely not created but whose existence lay in the necessity of things as much as the existence of animals and vegetables. Thus, Hermite went on, the study of subjective world allows a view on the real world, and he believed that

this wide-spread idea of *continuity in the laws of nature* that is at the bottom of Darwinian theories proceeds directly from the first analytical notions on functions that reigned until the time of Riemann. Now, there was no need to have the conceptions of this extraordinary genius for us to see rising up everywhere the discontinuity that imposes itself on us, and manifests itself even in the elements, and consequently for us to avoid the mistaken generalisation that led us to conceive of all functions as mirror images of the ones that are discontinuous only by becoming infinite (quoted in [Dugac 1984](#), 87).

By that time Schering, too, had provided a new proof of Mittag-Leffler theorem ([Schering 1881](#))⁹⁸ so that on February 24, 1881 Hermite wrote to Mittag-Leffler: “you should be very satisfied to have given rise to four proofs of your theorem so far; who will win the prize among the applicants from Germany, France and Italy?” (quoted in [Dugac 1984](#), 105). Not only that, Mittag-Leffler’s theorem quickly entered the classrooms in Paris. On June 29, 1881 Hermite wrote to his Swedish friend that “the students of the École Normale have worked very zealously, as Mr Darboux⁹⁹ has confided to me, in studying your theorem, which, I may say without exaggeration, has now become a classic among us” (quoted in [Dugac 1984](#), 123). On his part, Hermite himself expounded the theorem in his 1881/82 lectures at the Sorbonne ([Hermite 1882](#), 78–86).

6.7.7 Mittag-Leffler’s Final Version

As for Mittag-Leffler, Weierstrass’s (1880b) paper provided him with a method for extending his theorem to functions having an infinite number of poles or essential singularities with only one accumulation point, thus assuming that (6.80) could be entire, transcendental functions. He did this in a series of papers that Hermite published in the *Comptes rendus* of the Paris Académie beginning on February 13, 1882 ([Mittag-Leffler 1882](#)).¹⁰⁰ Inspired by Weierstrass’s (1880b) Casorati had the same idea as Mittag-Leffler, and on February 20 he sent a paper

⁹⁸It is worth mentioning in passing that, like Dini, Schering referred to Betti’s representation theorem under the name of ‘Betti’s convergence factors’ ([Schering 1881](#), 56–58).

⁹⁹By that time Darboux held a teaching position at the École.

¹⁰⁰Some months later Hermite also presented Mittag-Leffler’s extended theorem in his lectures. See ([Hermite 1882](#), 83–86).

to Hermite for publication in the *Comptes rendus*. Obviously it was not accepted, and Casorati reproduced it in the first part of his (1882), before explaining in detail the generalisation of Mittag-Leffler's theorem that he had also expounded in his lectures at the university of Pavia.

Eventually, Mittag-Leffler extended his theorem to the case in which the sequence $\{a_n\}$ has a finite number of accumulation points, and in 1884 he collected all his results into a major paper which appeared in his newly-founded journal *Acta mathematica*.

There Mittag-Leffler began by referring to a number of topological concepts from (Weierstrass's 1880c) and Cantor's theory of infinite point-sets. Having defined the concept of neighbourhood of a point, he introduced the concept of *continuum* as defined in Weierstrass's (1880c, 203). To Weierstrass, a *continuum* was a path-connected region A in $\mathbb{C}$ thought of as the domain of existence of an analytic function $f(z)$ generated by an element *via* analytical continuation. In his words,

Consider an arbitrary point in A , in the neighbourhood of it an arbitrary second one, in the neighbourhood of it an arbitrary third one, and so forth. The totality of points of A that one can get this way is a *continuum* (A_1) represented by a connected piece of the plane of the variable z whose boundary can consist of either isolated points or two or several lines or even by isolated points and lines at the same time.

Mittag-Leffler (1884a, 2) took up this definition adding that a point x' is "on the limit" of A if any arbitrarily small neighbourhood of x' contains always points that belong to A and points that do not belong to A . Then he defined as the complete limit of A the set of all its limit points. That said, he referred to Cantor to define isolated points, isolated sets and derived sets.

He was now able to state and prove his main theorem (Mittag-Leffler 1884a, 8): Let Q be an isolated set in the extended complex z -plane whose points will be denoted by $a_1, a_2, \dots, a_r, \dots$; further let

$$G_1\left(\frac{1}{z-a_1}\right), G_2\left(\frac{1}{z-a_2}\right), \dots, G_r\left(\frac{1}{z-a_r}\right), \dots$$

be a sequence of single-valued, monogenic functions, where $G_r\left(\frac{1}{z-a_r}\right)$ denotes an entire, rational or transcendent function of $1/(z-a_r)$ that vanishes when $1/(z-a_r) = 0$. Then one can always form an analytic expression which is regular everywhere except in the neighbourhood of the points belonging to $Q + Q'$ and which, for every r , can be put in the form $G_r\left(\frac{1}{z-a_r}\right) + P(z-a_r)$ in the neighbourhood of $z = a_r$, where $P(z-a_r)$ is a convergent power series of $z-a_r$.¹⁰¹

As he had shown in his 1877 papers, the theorem is easily proved if Q contains a finite number of points $a_1, a_2, \dots, a_n$, and therefore $Q' = \emptyset$. Instead, if $Q' \neq \emptyset$ one

¹⁰¹ $Q + Q'$ was his notation for the union of Q and Q' .

can associate with any a_r ($a_r \neq 0$ and $a_r \neq \infty$) a point b_r ($b_r \in Q'$) in such a way that $\lim_{r \rightarrow \infty} |a_r - b_r| = 0$. Then, given any $\varepsilon < 1$, and any sequence of arbitrary, positive numbers $\varepsilon_1, \varepsilon_2, \varepsilon_3, \dots$ whose sum is finite, the sought-for analytic expression could be built this way: if either $a_r = 0$ or $a_r = \infty$ one has $F_r(z) = G_r(1/z - a_r)$. Otherwise, if $a_r \neq 0$ and $a_r \neq \infty$, one can expand $G_r(1/z - a_r)$ in a series

$$G_r\left(\frac{1}{z - a_r}\right) = \sum_{n=0}^{\infty} A_n^{(r)} \left(\frac{a_r - b_r}{z - b_r}\right)^n$$

that is convergent for $\left|\frac{a_r - b_r}{z - b_r}\right| < 1$ ($\frac{a_r - b_r}{z - b_r}$ has to be understood as $\frac{z}{a_r}$ for $b_r = \infty$). Then he proved in great detail that one can always determine a whole number $m_r = -1, 0, 1, 2, 3, \dots$ such that

$$\left| \sum_{n=m_r+1}^{\infty} A_n^{(r)} \left(\frac{a_r - b_r}{z - b_r}\right)^n \right| < \varepsilon_r$$

for z satisfying the condition $\left|\frac{a_r - b_r}{z - b_r}\right| \leq \varepsilon$. By putting

$$F_r(z) = G_r\left(\frac{1}{z - a_r}\right) - \sum_{n=0}^{m_r} A_n^{(r)} \left(\frac{a_r - b_r}{z - b_r}\right)^n$$

the series

$$F(z) = \sum_{r=1}^{\infty} F_r(z)$$

provides the required analytic expression. Indeed, given any point $z_0 \notin Q + Q'$, one can always find a $\rho > 0$ such that $z \notin Q + Q'$ for $|z - z_0| < \rho$, and the series converges uniformly for $|z - z_0| < \rho$. In addition, Mittag-Leffler proved that $F(z) + G(z)$, where $G(z)$ is an arbitrary, regular function in the neighbourhood of any point $z \notin Q'$, is also a function satisfying the required condition.

Mittag-Leffler did not fail to remind his readers that he had already given this proof in his lectures since 1879. He also referred to his previous papers published in the *Comptes rendus* in 1882, and in a footnote also referred to the contributions by [Dini \(1881\)](#), [Hermite \(1881\)](#), [Schering \(1881\)](#) and [Casorati \(1882\)](#). Then he summarised some of Weierstrass's results on infinite products, and following the same route as in Theorem A he re-obtained Weierstrass's [\(1876b\)](#) representation theorems. Next he reformulated his Theorem A to deal with the case when Q is an isolated set, but $Q + Q'$ is, in his wording, “the complete limit of a *continuum* A ”, i.e. a path-connected domain of the complex plane. Given this, he stated Theorem B as follows:

Let Q be an infinite, isolated set of points such that $Q + Q'$ forms the complete limit of a *continuum* A . Let $a_1, a_2, \dots, a_r, \dots$ be the points of the set Q , and let $n_1, n_2, \dots, n_r, \dots$ be a sequence of integers (positive, negative, or zero). Further let

$$G_1\left(\frac{1}{z-a_1}\right), G_2\left(\frac{1}{z-a_2}\right), \dots, G_r\left(\frac{1}{z-a_r}\right), \dots$$

be a sequence of functions such that $G_r\left(\frac{1}{z-a_r}\right)$ is a rational or transcendental entire function of $1/(z-a_r)$ vanishing at $1/(z-a_r) = 0$. Then one can always form an analytic expression which represents a monogenic, single-valued, and regular function within the *continuum* A , and which in the neighbourhood of each point a_r of Q can be put in the form

$$(z-a_r)^{n_r} G_r\left(\frac{1}{z-a_r}\right) + (z-a_r)^{n_r} P(z-a_r).$$

This theorem, known as Mittag-Leffler's *Anschmiegungssatz*¹⁰² after [Osgood \(1907, 549\)](#), can be rephrased as follows¹⁰³: To each point $a_r \in Q$ associate the rational function

$$G_r\left(\frac{1}{z-a_r}\right) = \sum_{n=p_r}^{q_r} A_n^{(r)}(z-a_r)^n,$$

where q_r is a non-negative integer and $p_r \leq q_r$ is an integer. Then there exists a single-valued analytic function $f(z)$ that is regular at every point $z \notin Q + Q'$, and it is such that the difference

$$f(z) - G_r\left(\frac{1}{z-a_r}\right)$$

has a zero of order (at least) $q_r + 1$ at a_r .¹⁰⁴

In the remaining part of the paper Mittag-Leffler discussed the consequences of his theorems in great detail, thus reformulating his own previous results in the new topological setting provided by Cantor's theory of infinite, derived sets.

6.7.8 French Responses

Mittag-Leffler's interest in Cantor's theory was quite exceptional at that time. Notoriously, by then the general opinion of Cantor's theory of derived (and transfinite) sets was negative in Germany, with perhaps some major exceptions (Dedekind, Klein, Schottky and Weierstrass among them). Writing to Mittag-Leffler on September 9, 1883 Cantor reported that Kronecker had visited him in July and had not hesitated to state that the idea of different orders of infinity and

¹⁰²The theorem does not seem to have a standard English name; possibilities would be the "clinging" or, nicer, the "snuggling" theorem.

¹⁰³Mittag-Leffler himself rephrased his theorem as Theorem *D* (pp. 53–54).

¹⁰⁴See [Nörlund \(1927, viii\)](#), and also [Burckel \(1979, 1, 393\)](#).

transfinite sets and numbers was a “Humbug” (quoted in (Meschkowski and Nilson 1991, 127)). On the contrary Mittag-Leffler was increasingly convinced of the importance of Cantor’s work, and did his best to promote it. He was confident that the French mathematical world would be “interested at the highest degree in your discoveries”—as he wrote Cantor on January 10, 1883—precisely because they now need such researches and also because in their nice work in function theory they met with difficulties that can be overcome only by your works” (quoted in Dugac 1984, 271). He repeated this claim some days later, on January 19, when writing to Hermite in order to propose a translation of some of Cantor’s papers into French. Mittag-Leffler expressed his confidence that “the French geometers in the front-line—yourself, Poincaré, Picard, Appell, Goursat—now need the ideas that he [Cantor] develops and that will very quickly be extensively developed in France” (quoted in Dugac 1984, 271). However, Mittag-Leffler’s confidence was misplaced. “Mr. Poincaré judges that almost all French readers will have absolutely no inclination for the simultaneously philosophical and mathematical research of Mr. Cantor, where the arbitrary plays too great a part, and I do not think he is mistaken”, Hermite replied on March 5, 1883. In his response three days later Mittag-Leffler apparently agreed, even though he kept maintaining that the mathematical part of Cantor’s paper on infinite, linear set-points was of great importance, and he believed that Poincaré himself “will draw considerable advantages from it. But we will see” (in Dugac 1984, 276).

Hermite’s judgement became even more sharp in a letter he sent to Mittag-Leffler on April 13, 1883: “The impression that Mr. Cantor’s papers produce in us is pitiful. Reading them seems to all of us to be a real chore”. Even though Hermite acknowledged Cantor’s merit in having opened a new field of research, he added that “none of us is tempted to follow him. Among the results that can be understood it is impossible to us to pick up a single one having any actual interest. The correspondence between the points of a line and a surface leaves us absolutely indifferent, and we think that this remark depends on such arbitrary considerations that the author would have done better by keeping it for himself, and by waiting until something will be deduced from it” (quoted in Dugac 1984, 209). Hermite’s sharp opinion was also shared by Appell who, at Mittag-Leffler’s suggestion, had revised the French translation of Cantor’s paper that was eventually published in *Acta* 2, 349–380. This was followed by the French translation of (part of) Cantor’s *Grundlagen einer allgemeinen Mannigfaltigkeitslehre*.¹⁰⁵ While reading this—Hermite wrote Mittag-Leffler one week later—“Picard never stopped cursing the author”. However, Picard began to change his mind after meeting Cantor sometime in spring 1884. On May 6, 1884 Picard reported to Mittag-Leffler that his talks with Cantor have been “extremely interesting”. He went on to recognise that

in the beginning Cantor’s speculations seemed uninteresting to me, except from the philosophical point of view; now I begin to believe that all this could have applications in analysis: some of his theorems on trigonometric series, where it is matter of points of

¹⁰⁵See *Acta* 2, 381–408.

the first kind, have struck me extremely. Are not you going to publish something on these questions soon, that will complete my conversion to point sets? (quoted in [Nabonnand 1999](#), 122).

In 1885 this change of heart by several French mathematicians was completed when Appell and Poincaré proposed Cantor for membership of the *Société Mathématique de France* and he was elected unanimously, ironically on 1 April.¹⁰⁶

Picard's question chimed in well with what Mittag-Leffler was already doing. His (1884) paper actually showed the importance of Cantor's set theory and pioneered its application in analysis. It was hailed enthusiastically by Picard, Appell and Hermite, although they continued to have reservations about the use of concepts which were due to Cantor. In a letter to Mittag-Leffler on October 6, 1884 Hermite expressed the point of view of the French mathematicians in a long passage that is worth quoting in full¹⁰⁷:

Two things should be distinguished in your work: the propositions, the results at which you have arrived, and the exposition that you have made of them, namely the essence and the form. As for the former, I am certain I echo all analysts in recognising that it constitutes, together with Weierstrass's celebrated theorems, the very foundation of the theory of single-valued functions, and Picard, with whom I have talked about these matters, shares my opinion completely. He also does not disagree with me as far as the form is concerned with which you present and expound them. Both of us agree that in proceeding along the route which starts in these entirely new abstract notions in order to arrive at the realities of analysis through a chain of deductions you obeyed the German tendency, a nature of mind that is not ours. What is an imperative need for the French mind is to proceed in the opposite way by taking all possible care to show how a new notion results from the preceding notions, and to make the reader assist at the origin, at the birth of more general propositions as a consequence of known, particular cases, never leaving, if I may say so, the objective reality. [...] The abstraction, which is so attractive for the Germans, bothers us and somehow throws a veil over the consequences that partly conceals them until we have followed a route more suited to us to attain them. Among these intellectual tendencies I do not want at all to establish which one has to be preferred, however your memoir has shown to us how much they are different, if not absolutely opposite.

This notwithstanding, Hermite and his French colleagues engaged themselves in making Mittag-Leffler's and Weierstrass's results widely known in France by proposing them to their students as the subject of their theses, as they had earlier done with Riemann's. This was the case of S. Dautheville who submitted his thesis on power series of several complex variables on July 7, 1885. Its main content was openly based on (part of) the lithographed paper ([Weierstrass 1879](#)) on the preparation theorem and the quotient of power series of several variables. Then the author offered a generalisation of Mittag-Leffler's theorem inspired by [Weierstrass \(1880b\)](#), and in the concluding part he provided a new proof of Hurwitz's theorem ([1883](#)) that a single-valued, analytic function of n variables without essential singularities is rational. In his report Picard recognised that there was nothing

¹⁰⁶See *Bull. SMF* 13 (1885) 87.

¹⁰⁷Quoted in ([Dugac 1985](#), 94).

essentially new in Dautheville's thesis, whose aim was "to collect, and to prove with all the desirable precision, the general theorems that we have on the theory of function of several variables". Perhaps for this reason the thesis was published in the *Annales de l'École Normale Supérieure* (3) 2 (1885), 3–59.

More original was the thesis on the theory of essential, singular points that Claude Guichard submitted on November 13, 1883. There Guichard considered first the case of a single-valued function $f(z)$ having a singular point at a point α and any other singularities within a disk centred at α . Having showed that $f(z)$ can be expressed as the sum (or the product) of two suitable holomorphic functions, he was able to extend both Mittag-Leffler's theorem and Weierstrass theorem on the decomposition as a product of prime functions, and to establish some "general principles" that he applied to the study of simply and doubly periodic functions with arbitrary singular points. Guichard's thesis got a lukewarm, factual report from Appell but, this notwithstanding, as with Dautheville's it was published the *Annales de l'École Normale Supérieure* (2) 12 (1883), 301–394.

Much more interesting was Paul Painlevé thesis on the singular lines of analytic functions (Painlevé 1887). A pupil of Picard, Painlevé devoted the first part of his thesis to the study of a function in the neighbourhood of a singular line (or *coupure*, as he called it). Following the approach of the "French school" he stated that this notion emerged in the discussion of Cauchy's integral, even in the case of real integrands, as Hermite had recently shown. After referring to Weierstrass's and Tannery's results on functions with natural boundaries (see Sect. 6.9) Painlevé established the conditions under which a function can be analytically continued beyond a *coupure* or, in other terms, when the *coupure* is "artificial". He succeeded in doing this "with all rigour", as Picard wrote in his report, then in the second part he tackled the problem of the expansion in series of holomorphic functions. In Picard's view, the most interesting theorem established by Painlevé was that any holomorphic function within a domain S can be expanded in a series of polynomials there. This gives rise to a "very delicate discussion" when one asks about the possibility of such an expansion within S without supposing that the function is holomorphic on the boundary. "By means of a very skilful analysis" Painlevé proved that such an expansion is possible if the domain is convex and the boundary is smooth. In the concluding chapter he considered single-valued functions with a finite (or even infinite) number of singularities, thus referring to Bourguet's and Guichard's theses and re-obtaining Mittag-Leffler's theorem. Picard concluded his report by emphasising Painlevé's "extreme rigour" and "genuine originality".¹⁰⁸ Picard was right, and not only about Painlevé. By this time French mathematicians were emerging at the forefront of the research, and this was felt in Berlin by Weierstrass himself as a threat to the German leadership. "We must collect our forces with cunning so that Paris does not once again become the chief seat of mathematics", Weierstrass is reported to have said in 1882 during a meeting of all the Berlin mathematicians when the talks turned to the French

¹⁰⁸For Picard's quotes above, see (Gispert 1991, 338 and 342–343).

mathematicians. Mittag-Leffler happened to attend the meeting, and he was pleased to quote Weierstrass's claim in German in a letter to Hermite, leaving to Picard the duty of translating it.¹⁰⁹

6.8 Abelian Functions

Weierstrass had lectured for the first time on Abelian functions in summer 1866. He took the subject up again in the summer of 1869 and again in his lectures in the WS 1871/72. On this occasion he wrote to Schwarz on July 7th, 1872 to say that he had “reworked the whole theory fundamentally”. In doing so he had been able to “clarify several essential points completely”, which had always “made difficulties” for him. The whole, Weierstrass added, “has now obtained a form which satisfies me”. This version—the third one Weierstrass had written—was completed in the form of a brouillon or sketch which he hoped to be able to “reduce to a readable work of not too great a size”. This work was being done partly by himself and partly by a very talented woman student of his (Kovalevskaya, without any doubt) who “is fully immersed in the theory”.¹¹⁰ Weierstrass added that he was pleased that he had not published his previous versions from 1855–1856 and 1857–1858.¹¹¹ However, even now he was satisfied with the manuscript Weierstrass did not think of publishing it.

One year later, writing to Kovalevskaya on August 20th, 1873, he quoted an excerpt from a letter he had received from Richelot in which the latter expressed a decisive preference for the route Weierstrass had chosen in the theory of Abelian functions over the ones taken by Riemann and Clebsch. “It seems to me of even greater significance”, Richelot wrote, “that you have followed another route, the natural one for dealing with the main mathematical question of this century, by comparison with Riemann’s, and Clebsch and Gordan’s”. Richelot also expressed his regrets that Weierstrass had never published the continuation of his 1856 paper. “Neither Riemann’s works nor, even less, Clebsch and Gordan’s book should have prevented you from doing it”.¹¹²

Weierstrass was not at all insensible to Richelot’s flattering comments. However, he set the matter aside until the end of 1874 when he discovered the prime functions and realised their importance for the theory of Abelian functions. Then, on January 12, 1875 he wrote to Kovalevskaya that he was thinking seriously of presenting the essentials of his approach to Abelian functions in a series of letters to Richelot where he wanted “to point out the uniqueness of my method without hesitation, and

¹⁰⁹Quoted in (Dugac 1984, 263).

¹¹⁰The Weierstrass–Kovalevskaya correspondence provides evidence of this. See, e.g., (Bölling 1993, 52–56).

¹¹¹There is no hint of lectures on this subject in the list of Weierstrass’s lectures appended to vol. 3 of his *Werke*.

¹¹²For these quotes, see (Bölling 1993, 96).

to criticize Riemann and Clebsch”.¹¹³ On the other hand, the matter of his lectures was pressing, and some days later on January 28, 1875 he wrote to Schwarz that a student of his “who is astonishingly careful” (Hettner, one supposes) had prepared a 700-page manuscript of his lectures, together with an equally bulky manuscript of Kronecker’s algebra that will provide him with “essential help”. All that material had to be reduced to a readable form, and “haste is needed”, if one does not want even Abelian functions be sacrificed to the ever increasing tendency of writing books, Weierstrass added rather surprisingly. In this letter he limited himself to this rather cryptic hint, but it is likely that Weierstrass was thinking of a project of Koenigsberger’s, as is shown by the letter he had written some weeks before, on January 1st, to Kovalevskaya, to whom he used to confide his thoughts, “even the annoying ones”.

“I can’t tell you”, Weierstrass wrote, “how much it displeases me that precisely my oldest student, whom I hold so dear [Koenigsberger], immediately after publishing his bulky book on elliptic functions—in which one can hardly find a single page that does not give rise to censure—has now decided to devote himself to Abelian functions, about which he has worked out nothing by himself” (quoted in Bölling 1993, 173). That said, Weierstrass turned to more general statements about his own approach to mathematics:

I do not at all want to be a scientific pedant, and I do not recognise any, let alone a redeeming, church even in mathematics; what I demand of a scientific work is unity of methods, coherent pursuance of a determined plan, relevant, careful working up of the details, and that it carry the imprint of autonomous research. It is bad enough that here and elsewhere textbooks are written so often by inappropriate people (*Unberufenen*)—here the French must at least be recognised as having the merit of somehow making up for the lack of depth with clarity and elegance of exposition. But the higher, more difficult parts of science, in which only he who devotes all his force to them succeeds in finding something, should not succumb superficial book-writing (quoted in Bölling 1993, 173–174 and Mittag-Leffler (1923b, 155))

6.8.1 Weierstrass’s Algebraic Approach

After he had reduced the manuscript provided by his student to a readable form Weierstrass decided to sketch his algebraic approach to Abelian functions in a letter he sent Schwarz on October 3rd, 1875. The latter had asked his master’s advice on June 21st about a paper dealing with algebraic equations admitting a family of single-valued, birational transformations into each other. Schwarz’s paper dealt with the theorem: let an irreducible, algebraic equation $F(s, z) = 0$ have the property that it is transformed into itself by infinitely many birational transformations, then the number of linearly independent integral functions that are branched like the

¹¹³See (Bölling 1993, 184). These letters were never written because Richelot died in February 1875.

algebraic functions that belong in Riemann's sense to the same class as the given equations, is either 0 or 1. In other words, the given equation is either of rank (or genus) 0 or 1. Rather surprisingly, Schwarz grounded his proof on the idea of a Riemann surface, and he rephrased his theorem in geometrical terms accordingly: let a compact Riemann surface be transformed into itself by a family of single-valued, connected and conformal transformations, then the surface is either simply or triply connected.¹¹⁴ The paper was a interesting medley of Riemann's geometrical ideas and Weierstrass's analytical techniques, ending with the theorem: if two algebraic curves are so related that one can establish a one-to-one correspondence between their points through algebraic equations in infinitely many ways, then the coordinates of an arbitrary point of either curve are either rational or single-valued elliptic functions.¹¹⁵

In his letter Schwarz proudly asked Weierstrass not only about the correctness but also about the originality of his theorem, and observed that the “noteworthy limitation” to the cases $\rho = 0$ and $\rho = 1$ would imply that in these cases an algebraic addition theorem holds. “Among all the theorems whose knowledge I owe to you, this has impressed me the most”, Schwarz wrote, adding maliciously that “Koenigsberger seems not to know it”. Then he commented at length on a fragment full of gaps found in Riemann's *Nachlass* where it was matter of integrating partial differential equations satisfying prescribed boundary conditions for a domain obtained by cutting off three circles on the complex plane.¹¹⁶

Weierstrass is likely to have been disappointed by both the paper and the letter of his former pupil. He let three months pass before answering him, and then in his long letter he began by recognising that Schwarz's result was correct, “but”—he added—“I have an objection to *the method of proof* [Weierstrass's emphasis] that I believe I must communicate to you without fearing that you will misinterpret your old teacher if this time he does not agree with you”. This is why Weierstrass felt it needed to state his “confession of faith” (see 6.4.2). That said, Weierstrass went on by summarising the algebraic theorems he had presented in his last lectures on Abelian functions, knowledge of which would be enough to see that Schwarz's result was “an obvious truth”.

Weierstrass began by defining an algebraic *Gebilde* as the set of points x, y satisfying an irreducible algebraic equation $f(x, y) = 0$. To any point (a, b) of it one can associate a pair of single-valued, injective functions (actually, a pair of suitably convergent power series) $f_1(t), f_2(t)$ such that

$$x - a = f_1(t) \quad y - b = f_2(t)$$

¹¹⁴It is interesting to remark that, perhaps because of this Riemannian approach, the paper was published in Crelle's *Journal* only four years later. See (Schwarz 1879).

¹¹⁵See (Schwarz 1879, 291).

¹¹⁶This is hard to identify, but may be a reference to what was eventually published as Riemann (1876a, nr. XXVI).

satisfy the given equation, and $f_1(0) = f_2(0) = 0$. The set of points (x, y) so defined is an element of the *Gebilde*. It could be proved, Weierstrass added, that the given *Gebilde* could be split into a finite numbers of elements, and also that all the elements could be derived from a single one. This amounts to saying that the *Gebilde* defined by such a function $f(x, y) = 0$ is monogenic.¹¹⁷ Then, in order to answer Schwarz's question Weierstrass went on to explain in some detail what he meant by the concept that two algebraic *Gebilde* can be rationally transformed into each other. Let $f(x, y) = 0$ be the given, algebraic equation, and let $R_1(x, y)$ be a rational function. Putting $R_1(x, y) = \xi$, then to every value ξ (except, possibly, for a finite number of them) there corresponds a definite number, μ say, of pairs (x, y) , i.e. the order of $R_1(x, y)$ in Weierstrass's words. By substituting them in a second, rational function $R_2(x, y)$ one finds that to any ξ there correspond ν , say, values of $R_2(x, y)$ that are all distinct. Thus, from the equations

$$f(x, y) = 0, \quad \xi = R_1(x, y), \quad \eta = R_2(x, y)$$

one gets always an irreducible equation $\varphi(\xi, \eta) = 0$ of degree μ (resp. ν) with respect to ξ (resp. η). Summing up, to any pair (x, y) there corresponds a pair (ξ, η) and vice versa, and x and y can be rationally expressed in terms of (ξ, η) as $x = R_3(\xi, \eta)$, $y = R_4(\xi, \eta)$, where R_3 and R_4 are also rational functions. This means that $f(x, y) = 0$ and $\varphi(\xi, \eta) = 0$ can be rationally transformed into each other, and this applies also to the corresponding algebraic *Gebilde*. That said, Weierstrass went on to present some material which was "more essential and not completely known to you so far". As it summarised the content of (the first part of) Weierstrass's lectures as published in vol. 4 of his *Werke*, we will shortly discuss it in more detail below.

6.8.2 A Course on Hyperelliptic Functions

Weierstrass was right to be worried about Koenigsberger's planned book. Eventually, a book of his (Koenigsberger 1878), on the theory of hyperelliptic integrals appeared in print in 1878 and, all the worse from Weierstrass's point of view, the theory was expounded following Riemann's approach, including the use of Dirichlet's principle. It is interesting to compare this with what Weierstrass was going to teach that very same year on the same subject.

A fresh view of the content of his WS 1877/78 lectures on Abelian functions, or "more properly, on hyperelliptic functions" is offered by the short synopsis that Pincherle offered in a letter to Casorati on April 21st, 1878:

His [Weierstrass's] course can be divided into three parts. The first of these discusses differential equations of the form

¹¹⁷In more modern terms, this amounts to the claim that the Riemann surface of an (irreducible) algebraic function is connected and compact. In this connection see (Ullrich 2003).

$$du_\lambda = \sum_{\alpha=1}^{\rho} \frac{P(x_\alpha)}{y_\alpha} \frac{dx_\alpha}{x - a_\alpha} \quad (\lambda = 1, 2, 3, \dots, \rho),$$

where the u_λ are independent variables and the (x_α, y_α) are pairs of points on the fundamental curve

$$y^2 = (x - a_1)(x - a_2) \dots (x - a_{2\rho-1}).$$

He shows that the x_α satisfying the above system of differential equations are the roots of an algebraic equation of degree ρ , whose coefficients are shown to be single-valued functions of the independent variables $u_1, u_2, \dots, u_\rho$. These functions are denoted by $\wp(u_1, u_2, \dots, u_\rho)$ and he calls them by the name of abelian functions. He then studies the addition theorem for these functions and the relations that obtain between them and their partial derivatives.

Pincherle commented that this part did not differ very much from the first part of Weierstrass's 1856 paper. In fact, the major change with respect to that paper was the introduction of the $\wp$ -functions of several variables instead of the A 's.

Pincherle went on:

In the second part he treats hyperelliptic integrals, the decomposition of a general integral into integrals in normal form, their infinities and moduli of periodicity; then he shows how to write the normal integrals as functions of the $u_1, u_2, \dots, u_\rho$. He does this by means of the sums of logarithms of certain single-valued functions which he calls E and which play a major role in the theory. These functions (which have the property of being zero or infinite at only a single point and discontinuous at infinity) he calls prime functions because of the following property: every rational function of x and y , such as $y = R(x)$, can be regarded as function of the independent variables u , whence, as such, it can always be written as a product of the functions E . I do not know whether there exists already some literature about these functions.

In the third part he finally defines Θ -functions of several variables by means of a differential equation, establishes the monodromy and the periodicity of such functions, and their analytic expression; finally he shows that both the functions $\wp$ and the functions E are expressible as quotients of Θ -functions, and thus finds an analytic expressions for these $\wp$ and E where before he had only proved their existence. This course in Abelian functions is the third in a cycle of courses that Weierstrass gives over three semesters. (Quoted in Bottazzini 1991, 84–86.)

6.8.3 The Preparation Theorem

In his lectures on analytic function theory Weierstrass regularly presented a series of theorems on single-valued functions of several variables that he needed in his lectures on Abelian functions, and in 1879 he collected this material in a paper that he allowed to be lithographed for his students. This seminal paper (Weierstrass 1879) was eventually printed in 1886. The first theorem stated there was his *Vorbereitungssatz* (preparation theorem) which, as Henri Cartan (1965, 39) once said, has become “an indispensable tool in the contemporary developments of mathematics, in analytic geometry and in differential geometry”. It runs as follows:

Let $F(x, x_1, x_2, \dots, x_n)$ be a function, represented in the form of an ordinary power series, such that $F(0, 0, 0, \dots, 0) = 0$. Then there exist infinitely many systems of values of the variables $x, x_1, x_2, \dots, x_n$ within the disk of convergence of the series that satisfy the equation $F(x, x_1, x_2, \dots, x_n) = 0$.

In many researches, Weierstrass remarked, one has to determine only those systems for which $x, x_1, x_2, \dots, x_n$ lies in an arbitrarily small neighbourhood of the origin.

In order to prove the theorem, he began by putting $F(x, 0, 0, \dots, 0) = F_0(x)$ and writing

$$F(x, x_1, x_2, \dots, x_n) = F_0(x) - F_1(x, x_1, x_2, \dots, x_n)$$

so that $F_1(x, 0, 0, \dots, 0) = 0$. Assuming that $F_0(x)$ does not vanish for any x , he observed first that for a suitable $\rho > 0$ one has $F_0(x) \neq 0$ for $0 < x \leq \rho$, and second there exist values $(x_1, x_2, \dots, x_n) \neq (0, 0, \dots, 0)$ for which the series $F_1(\rho, x_1, x_2, \dots, x_n)$ converges. Further, given ρ_0 such that $0 < \rho_0 < \rho$ and $\rho_0 < |x| < \rho$, one can choose a suitable small ρ_1 with $|x| < \rho_1$, $|x_j| < \rho_1$ ($j = 1, \dots, n$) such that $|F_0| > |F_1|$.

Given this, one has the uniformly convergent series

$$\frac{1}{F(x, x_1, x_2, \dots, x_n)} = \frac{1}{F_0} + \frac{F_1}{F_0^2} + \frac{F_1^2}{F_0^3} + \dots = \sum_{\lambda=0}^{\infty} \frac{F_1^\lambda}{F_0^{\lambda+1}}$$

and also, by considering its logarithmic derivative with respect to x

$$\frac{\partial F}{\partial x} \frac{1}{F} = \frac{F'_0}{F_0} - \frac{\partial}{\partial x} \sum_{\lambda=1}^{\infty} \frac{1}{\lambda} \frac{F_1^\lambda}{F_0^\lambda}.$$

Then, supposing that the exponent of the first term in the power expansion of $F_0(x)$ is m one has

$$\frac{F'_0}{F_0} = mx^{-1} + G(x),$$

where $G(x)$ is a power series. Further, because of the uniform convergence of the series $\sum_{\lambda=1}^{\infty} \frac{1}{\lambda} \frac{F_1^\lambda}{F_0^\lambda}$ one can collect all terms containing the same power of x , and rewrite it as

$$\sum_{\lambda=1}^{\infty} \frac{1}{\lambda} \frac{F_1^\lambda}{F_0^\lambda} = \sum_{v=-\infty}^{\infty} G(x_1, x_2, \dots, x_n)_v x^v,$$

where $G(x_1, x_2, \dots, x_n)_v$ is a suitable power series. Eventually one has

$$\begin{aligned} \frac{\partial F}{\partial x} \frac{1}{F} &= mx^{-1} + G(x) - \frac{\partial}{\partial x} \sum_{v=-\infty}^{\infty} G(x_1, x_2, \dots, x_n)_v x^v = \\ &= mx^{-1} + G(x) - \sum_{v=-\infty}^{\infty} G(x_1, x_2, \dots, x_n)_v v x^{v-1}. \end{aligned} \quad (6.81)$$

Thus for values x_j ($|x_j| < \rho_1, j = 1, \dots, n$) the equation $F(x, x_1, x_2, \dots, x_n) = 0$ has always m roots $x', x'', \dots, x^{(r)}$ (counted with appropriate multiplicity) whose modulus is less than ρ . Then, the difference

$$\frac{\partial F}{\partial x} \frac{1}{F} - \frac{1}{x - x'} - \dots - \frac{1}{x - x^{(r)}}$$

can be expanded in a power series $P(x)$ such that

$$\frac{\partial F}{\partial x} \frac{1}{F} = P(x) + \sum_{v=0}^{\infty} s_v x^{-v-1}, \quad (6.82)$$

where $s_v = (x')^v + \dots + (x^{(r)})^v$. By comparing the coefficients of both the expansions of $\frac{\partial F}{\partial x} \frac{1}{F}$ in (6.81) and (6.82) one has $s_0 = m$, i.e. $r = m$ and also

$$P(x) = G(x) - \sum_{v=0}^{\infty} (v+1)G(x_1, x_2, \dots, x_n)_{v+1} x^v$$

for $|x| < \rho$.

By putting

$$f(x, x_1, x_2, \dots, x_n) = \prod_{j=1}^m (x - x^{(j)}) = x^m + f_1 x^{m-1} + \dots + f_m,$$

where $f_1, \dots, f_m$ can be expressed in terms of $s_1, \dots, s_m$ one has

$$\frac{\partial F}{\partial x} \frac{1}{F} = \frac{f'}{f} + P(x)$$

and eventually

$$F(x, x_1, \dots, x_n) = f(x, x_1, x_2, \dots, x_n) \cdot CQ(x, x_1, x_2, \dots, x_n), \quad (6.83)$$

where C is the coefficient of x^m in $F(x, 0, \dots, 0)$ and $Q(x, x_1, x_2, \dots, x_n) = e^{\int P(x) dx}$, and the roots of the equations $F(x, x_1, \dots, x_n) = 0$ and $f(x, x_1, x_2, \dots, x_n) = 0$ coincide. As the coefficients of f and Q are independent of ρ, ρ_1 , the equation (6.83) holds in a suitable neighbourhood of the origin where F, f and Q are all convergent.

Weierstrass proved the preparation theorem under the hypothesis that the function $F(x, 0, \dots, 0)$ does not vanish identically. If it does, he showed how to generate a distinguished polynomial $f(x, x_1, x_2, \dots, x_n)$ by means of linear transformations, a procedure which—as Ullrich (1989, 161) has pointed out—can be already found in Hettner's 1874 lecture notes (pp. 448–452), whereas in both Killing's and Hurwitz's notes “Weierstrass assumes without any commentary that the power series under consideration is distinguished in x ”. Summing up, according to Ullrich Weierstrass

“had the geometrical version of the preparation theorem to its full extent” already in 1874.

In Cartan’s (1965, 155) wording, the preparation theorem can be stated in the following terms (slightly different from Weierstrass’s original formulation):

Let $F(x, x_1, \dots, x_n)$ be a holomorphic function in the neighbourhood of the origin. Suppose $F(0, 0, \dots, 0) = 0$, $F_0(x) = F(x, 0, \dots, 0) \neq 0$ and let p be the integer such that $F_0(x) = x^p G(x)$, $G(0) \neq 0$. Then there exists both a “distinguished” polynomial

$$f(x, x_1, \dots, x_n) = x^p + a_1 x^{p-1} + \dots + a_p,$$

(whose coefficients $a_j(x_1, \dots, x_n)$ are holomorphic functions in the neighbourhood of the origin and vanish at the origin) and a function $g(x, x_1, \dots, x_n)$ which is holomorphic and non-zero in the neighbourhood of the origin, such that $F = f \cdot g$ in the neighbourhood of the origin.

In a footnote Weierstrass claimed he had expounded the *Vorbereitungssatz* in his lectures since 1860. A glance at the surviving lecture notes shows that actually, without naming it that way, the theorem was stated and proved in his 1868 lectures (Weierstrass 1968, 85–90) when tackling the problem of the inversion of analytic functions.

Before dealing with the case of several variables, Weierstrass considered a function $f(x)$ of one variable expanded in convergent power series. In the proof he distinguished between the case of one zero (which can be found also in Jacobi’s work) and multiple zeros. The latter case, one reads in Killing’s notes, has been treated “up to now only in the lectures [of Weierstrass], and with a proof completely different from Jacobi’s” (see Weierstrass 1968, 90). Then he extended this result to a power series $f(x, x_1, \dots, x_n)$ which is convergent in a disk centred at the origin, vanishes at the origin and is of degree m in x , i.e. $f(x, 0, \dots, 0) = x^m G(x, 0, \dots, 0)$ with $G(0, \dots, 0) \neq 0$ (see pp. 90–91). Eventually, as a special case he obtained Lagrange’s series.

The preparation theorem is also expounded and proved in greater detail in Hurwitz’s 1878 lecture notes (Weierstrass 1988a, 153–156) following essentially the same route as in his 1879 paper. In the last of those lectures Weierstrass introduced the concept of an analytical *Gebilde* of the first order (*Stufe*) as defined (locally) by the set of points x, y given by $x - a = \varphi(t)$, $y - b = \psi(t)$. Then, with the technique of analytical continuation, he obtained a global *Gebilde* and explicitly referred to “the analogy between this definition and the analytical–geometrical definition of curves” in $\mathbb{C}^2$ (Weierstrass 1988a, 162). He distinguished between regular and irregular points of a *Gebilde* and pointed out (1988a, 165) that the latter are “branch-points in Riemann’s sense of the analytic function”.

Eventually, he introduced the concept of analytical *Gebilde* of the n th order (*Stufe*). In Hettner’s 1874 manuscript this is defined by a system of equations $x_{n+r} = G_r(x_1, x_2, \dots, x_n)$ where $(r = 1, 2, \dots, v)$, and $G_r(x_1, x_2, \dots, x_n)$ are power series in n variables. By suitable inversion of the power series at regular points he eventually got the “extremely important” final result: “If we replace a certain number $[n]$ of the original independent variables $x_{n+1}, x_{n+2}, \dots, x_{n+v}$ in the equation

with the same number of the original dependent variables $x_1, x_2, \dots, x_n$ then we obtain v new equations that define a second, analytical *Gebilde* of the n th order, which is completely identical in all its parts with the one defined by the original equation” (Weierstrass 1874, 489). In Weierstrass’s view, this theorem represented “the goal of the lectures of function theory”.¹¹⁸

6.8.4 Further Theorems on Functions of Several Variables

The preparation theorem provided Weierstrass with the necessary tool for successfully tackling the problem of divisibility of two power series in several variables. One such series $P_1(x_1, \dots, x_n)$ is divisible by another $P_0(x_1, \dots, x_n)$ if one can determine a third power series $P_2(x_1, \dots, x_n)$ such that

$$P_1(x_1, \dots, x_n) = P_0(x_1, \dots, x_n)P_2(x_1, \dots, x_n).$$

The second theorem Weierstrass presented in his 1879 paper dealt with the conditions of divisibility of two such power series. If $P_0(0, \dots, 0) \neq 0$ the given series are always divisible, on the contrary this is not the case when $P_1(0, \dots, 0) \neq 0$ and $P_0(0, \dots, 0) = 0$. The remaining case, when both series vanish at the origin, is the interesting one. Weierstrass (1886, 146) succeeded in establishing necessary and sufficient conditions for the series $P_1(x_1, \dots, x_n)$ to be divisible by $P_0(x_1, \dots, x_n)$, and also the necessary and sufficient conditions for them to have a common divisor that vanishes at the origin (1886, 147–151).

Then he turned to the theory of analytic functions of several variables. By analogy with what he had done in his (1876b) for functions of a single variable, he began by stating that a power series of several variables that is convergent for every system of values of the variables in some neighbourhood of a given point, represents a single-valued (entire) function of several variables $f(x_1, x_2, \dots, x_n)$ which is regular on that neighbourhood. If it can be analytically continued, then set of regular points of the function is a $2n$ -fold, necessarily bounded domain (1886, 154–155). Every point $(a_1, \dots, a_n)$ on its boundary is a singular point, either a pole or an essential singularity according to whether or not a power series $P_0(x_1 - a_1, \dots, x_n - a_n)$ exists, which vanishes at $(a_1, \dots, a_n)$ and it is such that in a suitable neighbourhood of $(a_1, \dots, a_n)$ the product

$$P_0(x_1 - a_1, \dots, x_n - a_n)f(x_1, x_2, \dots, x_n)$$

can be represented by a power series $P_1(x_1 - a_1, \dots, x_n - a_n)$.

He further distinguished two kinds of poles according to whether $f(x_1, x_2, \dots, x_n) = \infty$ at $(a_1, \dots, a_n)$ could be said to become infinite or did not have a definite

¹¹⁸See (Ullrich 1989, 163) and (Kopfermann 1966, 94–95).

value there. Then, given two convergent power series $P_0(x_1 - a_1, \dots, x_n - a_n)$ and $P_1(x_1 - a_1, \dots, x_n - a_n)$ he established the conditions under which their ratio defines a single-valued function $f(x_1, x_2, \dots, x_n)$ that behaves like a rational function in the overlapping domain of convergence of the given series. On the contrary, he said, the question whether every single-valued function $f(x_1, \dots, x_n)$ which “everywhere in the finite behaves like a rational function” can be represented as a quotient of two convergent power series for any system of finite values of the variables is still “an unresolved question for functions of several variables which seems to present considerable difficulties” (Weierstrass 1886, 163). Related to this, Weierstrass asked a second question he could not answer: given a function that can be represented as a quotient of two entire functions of which one at least is transcendental, does the quotient have the form $0/0$ only for the values for which the function is not determined? In other words, from the values that the dividend and divisor assume at a given point can one immediately deduce whether the function has a definite value at that point, and how it behaves in the neighbourhood of the point? Unable to give a general answer, Weierstrass limited himself to producing some theorems that answered these questions “in many cases”.

Finally he devoted the last part of the paper to the problem of expanding an n -fold periodic, entire function of n variables in convergent series. By means of a linear transformation the given function

$$f(u_1 + 2\omega_{1\beta}, \dots, u_n + 2\omega_{n\beta}) = f(u_1, \dots, u_n), \quad \beta = 1, 2, \dots, n,$$

where the periods ω_{jk} s are constants, can be reduced to $\varphi(v_1 + m_1, \dots, v_n + m_n) = \varphi(v_1, \dots, v_n)$ where the m are arbitrary integers, (so all the periods are 1) and the problem can be easily solved by resorting to the Fourier expansion of functions of several variables. Weierstrass was happy to show that the same expansion could be obtained by means of fundamental theorems of the theory of ordinary power series.

6.8.5 Weierstrass's Last Lecture on Abelian Functions

In WS 1880/81 Weierstrass lectured on Abelian functions for the last time.¹¹⁹ As he had done in WS 1877/78, he limited himself to dealing with hyperelliptic functions. This time, however, he presented the subject “following a new plan”, as he wrote to Kovalevskaya on April 11, 1881, and he was happy to say that a large majority of his listeners had followed the lectures faithfully to the end. So he decided to produce a survey of the theory of Abelian functions, built on the same foundations as he had given in his lectures on the hyperelliptic ones, and in the letter to Kovalevskaya he sketched a short synopsis of his planned paper.

¹¹⁹ A course he announced on this subject in WS 1883/84 was never given.

He began by associating with a given n -valued function $y(x)$ arbitrarily many functions $F(x, y)$ that are rational in y and single-valued functions of x and can be expressed in the form

$$f_0(x) + f_1(x)y + \cdots + f_{n-1}(x)y^{n-1},$$

so that $f_0(x), f_1(x), \dots, f_{n-1}(x)$ are single-valued functions each of which has only a finite number of essential singularities.¹²⁰

Under this assumption, Weierstrass went on, the theory of these many-valued, transcendental functions can be developed along the lines of his (1876b) paper on single-valued functions. Accordingly, prime functions can be introduced that “play the same role as the algebraic units in number theory” so that every function $F(x, y)$ can be represented by means of—in general, infinitely many—prime functions.¹²¹

That said, the integral $\int F(x, y)dx$ can always be expressed as

$$\int F(x, y)dx = F_0(x, y) + \sum_v C_v \log F_v(x, y),$$

where the sum has a finite (resp. an infinite) number of terms according to whether the number of infinities of $F(x, y)$ is finite or not. In the case that $F(x, y)$ is rational, one obtains Abel’s integral “whose essential properties can be derived in the easiest way from the above-presented general theory. In the same way one obtains an easy route to the θ -functions”.¹²² In the paper Weierstrass planned to offer “only the foundations of the theory without entering the detail of calculations” and optimistically concluded that “the paper will appear in a few weeks”.

According to the evidence produced by Mittag-Leffler, apparently Weierstrass worked very hard on it. But the paper, even if it was ever completed, was never published, and in the end it was lost.

6.8.6 The Published Lectures

When Weierstrass was about 70 he felt that time had come to publish a possibly short exposition, but one enriched with critical comments, of the principles of function theory which he had expounded in his lectures for many years, but had never fully worked out. He wrote Schwarz on March 14, 1885 that

¹²⁰As Bölling (1993, 261) has aptly remarked, this would represent a generalisation of Weierstrass’s theory of Abelian functions as published in his *Werke* (see next section), for there he started from the field of rational functions in x, y , where y is an algebraic function of x , which means that $f_0(x), f_1(x), \dots, f_{n-1}(x)$ are rational functions.

¹²¹As we will see in the next section, in the published lectures Weierstrass compared prime functions with Kummer’s ideal prime factors.

¹²²See (Bölling 1993, 257–259) and (Mittag-Leffler 1923b, 180–181).

“To accomplish this work is seen by me as a pressing need if I do not want the fruit of serious, year-long reflections and deep work on various branches of analysis to be wasted. Unfortunately, in my effort to build analysis on a sound basis I do not find any understanding, let alone support, among my closest friends and colleagues”.

Those who had disappointed him included Kronecker, who extended his dislike of Cantor’s work partly to Weierstrass’s, and also Fuchs who, as far as the principles are concerned, “grew up too much under Briot’s and Bouquet’s influence” and, in spite of his important and very influential achievements in the field of linear differential equations, still kept expressing views that Weierstrass thought had been rejected for a long time. This had occurred recently when Fuchs read a paper to the Akademie (Fuchs 1885) where once more he misunderstood the fact that for a function “assuming a value and coming arbitrarily close to a value are two completely different things” (see Sect. 4.3).

Because Weierstrass was still involved in the publication of his *Formeln und Lehrsätze* and in the writing of (Weierstrass 1885), he had no time in the next few months to work out the principles of function theory. Instead he revised four of his previously published papers for publication as (Weierstrass 1886). He had planned to add one more paper to them, containing his “confession of faith in function theory”. But he had great difficulty in writing it, he confessed to Schwarz on May 28, and apparently the paper was never written.¹²³

Some months later, on December 20th, 1885, Weierstrass wrote once more to Schwarz that he had in his hands lecture notes from 1873 on, including very good notes by Weltzien, Hettner, Knoblauch, Hölder, Hurwitz, Deslisle, and many other less complete lecture notes but all in all a satisfying collection of them. Finally, in 1889 he expressed a wish to have his lectures published. His students Hettner and Knoblauch were charged with editing the set of his WS 1875/76 and SS 1876 lectures. The manuscripts of lectures given in previous or subsequent years were occasionally used by the editors to clarify the matter at certain places. In the meantime Weierstrass decided to collect and publish his mathematical works. Therefore the printing of the (already) edited lectures on Abelian functions was postponed to fill up the huge volume 4 of his *Werke*, which Weierstrass was able to check only a small part of before his death.

In spite of the title, the theory of Abelian functions in its proper sense is only sketched there, as Hettner and Knoblauch admitted, while “the basic algebraic research and the theory of Abelian integral is expounded precisely”. They also pointed out that Chapters 26, 28 and the first part of Chapter 29 went back to a manuscript Weierstrass had let circulate among his students.

In the historical introduction to the topic Weierstrass also explained why he decided to withdraw the continuation of his 1856 paper after Riemann published his own in 1857 and claimed he had found “the solution of the general inversion problem” by the end of 1869 and presented it in his lectures thereafter. More

¹²³This was likely replaced by his (1880c) that was also included in the booklet (Weierstrass 1886).

interestingly, he stated that the difficulties one encounters in the theory of Abelian function depend on tackling the theory of the integral at once without having first studied the properties of the integrand function deeply enough. This explained why he felt that extensive preliminary research into the properties of algebraic functions was needed, beginning with the algebraic foundation of the theory.

The version of Weierstrass's theory of Abelian functions presented in the fourth volume of his *Werke* is divided into three parts: the algebraic foundations of the theory; Abelian integrals; and Abelian functions.¹²⁴ The first of these begins with the idea of an “algebraic *Gebilde*” on an irreducible algebraic curve $f(x,y) = 0$. This is a local parameterisation of a neighbourhood of a point (a,b) on the curve: $x = \phi(t)$, $y = \psi(t)$, where $\phi(0) = a$, $\psi(0) = b$, $f(\phi(t), \psi(t)) = 0$, and the power series ϕ and ψ converge for all $|t|$ less than some non-zero amount which may depend on a and b . Weierstrass discussed what happened if the point (a,b) is a singular point of the curve, and how to handle infinitely distant points on the curve.

Then he introduced the idea of rational functions on the algebraic curve and showed that a rational function $R(x,y)$ takes each value on the curve the same number of times (counted with multiplicity); this number he called the order (*Grad*) of the rational function R . Particular attention naturally attached to the points where a given rational function takes the value 0 or ∞ , and Weierstrass considered when a rational function was determined once a certain number of poles were specified (points where it took the value ∞). He looked for sets of points that cannot form a complete system of poles of a rational function, and he defined (p. 69) the rank (*Rang*) of a *Gebilde* to be the smallest number, ρ such that if $n > \rho$ then there is a rational function with n arbitrarily given poles. This number agrees with the genus of the algebraic curve as defined by Riemann, and Weierstrass showed it was unaltered by a birational transformation of the curve. If (a_j, b_j) , $j = 1 \dots \rho$ is a set of ρ points that cannot be the set of poles of a rational function, then given any other point (x', y') there is a rational function $F(x, y, x', y')$ that has poles at precisely the points (x', y') and the (a_j, b_j) , $j = 1 \dots \rho$, and this is a rational function of (x', y') as well as of (x, y) .

From functions of the form F Weierstrass constructed another class of functions H that allowed him to define integrals of the third kind on the algebraic curve, and from them integrals of the first and second kinds. He then showed there were ρ linearly independent integrals of the first kind on the curve, and gave two ways of calculating the rank or genus of an algebraic curve in terms of the degree of the algebraic curve and its singular points. Then he steadily built up to a decomposition theorem for rational functions on the curve, describing any rational function in terms of integrals of the first, second, and third kinds. This allowed him to investigate points which are the only pole of a rational function, and he showed that any point on the curve can be such a point and there is a corresponding number k such

¹²⁴More detailed accounts of Weierstrass's theory can be found in (Wirtinger 1901), (Houzel 1978), and for an extensive account of it based on various sets of Weierstrass's lecture notes, see (Brill and Noether 1894, 403–437).

that there are rational functions vanishing at the given point of order $k, k+1, \dots$. Moreover, for each point (a, b) there are rational functions that have their only pole at (a, b) and these are of every degree except for a certain set of ρ values. Generally these values will be $1, 2, \dots, \rho$ but there are a finite number of points at which the missing values for the degree are not the simplest values; these points have become known as the “Weierstrass points” on the curve and this result as the Weierstrass gap theorem (pp. 224–225). The first section of the volume ended with a proof that, given an algebraic curve, it is only necessary to consider a finite number of *Gebilde* (in modern terms this amounts to saying that an algebraic curve is compact and connected).

The second section, on algebraic integrals, took the reader deeper into the theory of these integrals and their periods (integrals around closed non-contractible paths on the algebraic curve), culminating with Abel's theorem and its converse. Weierstrass's route to these results was via the E -functions and Ω -functions that he introduced. $\Omega(x, y)$ is defined as an integral of the third kind, and $E(x, y)$ from the equation $E(x, y) = e^{\Omega(x, y)}$, but the characteristic properties of the function E are that it has a simple pole at each end point of the integral, an essential singularity at each point where the integrand has a pole and is defined up to a multiplicative factor (the exponential of a period of the integrand) as the path of integration is varied. The key result was that a rational function could be written as a product of E functions, a result that Weierstrass compared explicitly (p. 391) with Kummer's theory of ideal prime factors in algebraic number theory. The factors are determined by the zeros and poles of the rational function. Weierstrass ended this section by showing how the decomposition theorem led to a simple proof of Abel's theorem and its converse (pp. 431–438), which had first been established by Clebsch in 1863.

The final section, on Abelian functions, dealt with the functions obtained by inverting sums of Abelian integrals (pp. 439–624). He swiftly established an algebraic addition theorem for Abelian functions and the fact that they have a system of 2ρ periods. Then he introduced new E -functions, defined as a product of ρ of the earlier E -functions but now regarded as a function on $\mathbb{C}^\rho$ by treating the parameters in the definition of an E -function as variables. The new E -functions are characterised by their simple poles and essential singularities, and Weierstrass showed that they play a role analogous to the σ function in his theory of elliptic functions. Weierstrass finished by giving an extensive treatment of the theory of theta functions in ρ variables.

6.9 Weierstrass's Last Years

Because of his poor health Weierstrass spent the summer 1880 out of Berlin, in the country and eventually in Switzerland (Fig. 6.5). He was therefore unable to attend the meetings of the Akademie, and so it was Kronecker who, on August 12, read a paper of his on function theory that dealt with the properties of infinite series



Karl Weierstraß 1815–1897

Fig. 6.5 Karl Weierstrass (*Acta Mathematica Table générale des tomes 1–3, 1882–1912, p. 176*)

of rational functions ([Weierstrass 1880c](#)). This paper was quickly translated into French and turned out to be one of Weierstrass's most influential works.

6.9.1 *A Paper on Function Theory*

There Weierstrass began by stating some theorems on uniformly convergent series of rational functions, including the so-called M -test for uniform convergence now named after him. He introduced it in a footnote ([Weierstrass 1880c](#), 202) where he repeated his $\delta - n$ -definition of uniformly convergent series in a domain B and remarked that if the series is unconditionally convergent in the domain then it is always possible to remove has a finite number of its terms so that the sum of arbitrarily many of the remaining ones is smaller than δ for any value of the variable. “This condition is certainly satisfied” Weierstrass added, “when there exists a sequence of positive constants $g_1, g_2, g_3, \dots$ such that $|f_v(x)| \leq g_v$ for every point of the domain B , and the sum $\sum_{v=0}^{\infty} g_v$ has a finite value”. In order to characterise

the domain of convergence of such series he introduced the concept of a *continuum* defined as a path-connected region of the complex plane and proved that a series

$$\sum_{v=0}^{\infty} f_v(x)$$

that is uniformly convergent in such a *continuum* represents a branch of a monogenic, analytic function there. However, the domain of (uniform) convergence of a series may be built up of different, disjoint regions as shown by the series

$$F(x) = \sum_{v=0}^{\infty} \frac{1}{x^v + x^{-v}}, \quad (6.84)$$

which is uniformly convergent for $|x| < 1$ and $|x| > 1$.

When this is the case, an “important question” in function theory arises, namely whether the given series represents branches of the same monogenic function or not. The question had a negative answer, as Weierstrass was going to prove. This would imply that “the concept of a monogenic function of one complex variable does not coincide completely with the concept of a dependence that can be expressed by means of (arithmetical) operations on magnitudes”. [Weierstrass \(1880c, 210\)](#) was pleased to add in a footnote that “the contrary has been stated by Riemann” in §20 of his thesis, and also that “a function of one argument, as defined by Riemann, is always a monogenic function”.

Weierstrass said he had found and presented in his lectures “for many years” the result that in either domain $|x| < 1$ and $|x| > 1$ the series (6.84) represents a monogenic function which cannot be analytically continued into the other one across their common boundary ([Weierstrass 1880c, 211](#)).¹²⁵ As he showed in an appendix to the paper, the proof was based on the identity¹²⁶

$$1 + 4F(x) = \left(1 + 2 \sum_{n=1}^{\infty} x^{n^2}\right)^2, \quad |x| < 1 \quad (6.85)$$

that Jacobi had established in the *Fundamenta nova*. (Remember that $|x| = 1$ is a natural boundary for $1 + 2 \sum_{n=1}^{\infty} x^{n^2}$).

This was a particular example of the main theorem he proved in his paper, namely that a series of rational functions converging uniformly inside a disconnected domain may represent different monogenic functions on disjoint regions of the domain ([Weierstrass 1880c, 221](#)).¹²⁷

¹²⁵In fact, Weierstrass had already discussed this series in his (1874, 500–502).

¹²⁶For a detailed account, see ([Ullrich 1997](#)).

¹²⁷This fact had already been discovered by [Hankel \(1870, 105\)](#) who in Note III of his paper (1870, 109–113) provided similar examples, which had been referred to by [Schwarz \(1872a, 241\)](#).

He summarised the content of his paper in a letter to Schwarz on December 20, 1880. After recalling that they had discussed many times the question of whether the concept of a monogenic function $f(x)$ does or does not coincide completely with the concept of a dependence which can be expressed by means of (arithmetical) operations on magnitudes, he referred to the well-known fact that the Fourier series of a function “can represent completely different, and actually arbitrary functions of a real argument in different intervals of the variable” to remark that the same property can be extended even to series whose terms are monogenic functions of a complex variable, as he had shown in his paper. “This, I believe, was hitherto not known”, Weierstrass was pleased to state.¹²⁸

He had discussed this matter with du Bois-Reymond years before, when the latter was preparing the draft of his 1875 paper (see Sect. 6.5.5). In a letter to du Bois-Reymond on December 15, 1874 Weierstrass remarked that the claim Riemann had made in the opening page of his thesis—i.e. that the two concepts of dependence of w on z conceived either as arbitrarily given or as defined by determinate operations on magnitudes are “congruent” as a result of theorems on Fourier series—cannot be accepted any more because of du Bois-Reymond’s example of a function whose Fourier series fails, even if only at a single point, to represent the given function. Such a function is continuous in any interval around the origin, and even if the function has a finite value there its Fourier series expansion is divergent at $x = 0$.¹²⁹

6.9.2 Two Theorems Opposing the Standard View

Having proved his main theorem, Weierstrass took the opportunity to clarify an essential point connecting real and complex analysis. From the very beginning, in his lectures on the elements of function theory he had pointed out two theorems that “did not coincide with the standard view”, namely

1. The continuity of a real function does not imply its differentiability.
2. A complex function defined in a bounded domain cannot always be continued outside it.

The points where the function cannot be defined may be “not simply isolated points, but they can also make lines and surfaces” (Weierstrass 1880c, 221). His reasoning was the following: Let the series $\Sigma A_v x^v$ be absolutely and uniformly convergent for $|x| \leq 1$. Then, by restriction to $|x| = 1$ one obtains the series $\Sigma A_v e^{v\theta}$, which

¹²⁸Borel (1907, 2086–2087) referred to this as one of the “most curious” examples of formulae given in classical texts whose true significance and importance became clear only after they have been attached to a theory. He added that Bertrand had drawn attention to many series like Weierstrass’s in his 1864–1870 *Traité*, taking them from earlier work. “But at that time [Bertrand] did not know of Weierstrass’s and Méray’s ideas in progress and, unaware of the idea of an analytic function, he was happy to transcribe dead formulae without being able to bring them to life”.

¹²⁹See Weierstrass (1923a, 207).

represents a continuous function of a real variable t . Suppose the series is continued analytically outside the disk $|x| = 1$ by considering its expansion in a power series $P(x - x_0)$ centred at x_0 with $|x_0| = r_0 < 1$. An arc of the circle $|x| = 1$ is completely included in the convergence domain of $P(x - x_0)$. There one has

$$\sum_{v=0}^{\infty} A_v e^{v t i} = P(x_t - x_0), \quad x_0 = r_0 e^{t_0 i}, x_t = e^{t i}, \quad (6.86)$$

and $t_0 - \tau < t < t_0 + \tau$.

As $P(x - x_0)$ has derivatives of any order with respect to x , the same holds true for $P(x_t - x_0)$ with respect to t . Therefore, “when one can prove that the function (6.86) in a definite case has no derivatives of any order in any interval of the variable t , one has to conclude that the domain of convergence of the series $P(x - x_0)$, no matter how x_0 is taken, is completely included in the domain of convergence of the given series, which represents a function that cannot be continued outside its domain of convergence” (Weierstrass 1880c, 222).

When a is an odd integer and $0 < b < 1$ the series

$$\sum_{v=0}^{\infty} b^v x^{a^v} \quad (6.87)$$

satisfies the required conditions. However, as Weierstrass had communicated to the Berlin Akademie since 1872, the series

$$\sum_{v=0}^{\infty} b^v \cos a^v t$$

represents a continuous nowhere differentiable function as soon as $ab > 1 + 3\pi/2$, under which condition also the series (6.87) represents a function that cannot be analytically continued outside the unit disk.

Following Weierstrass's hint, the discovery of a continuous nowhere differentiable function seems to have been related primarily to the problem of analytic continuation and not, as Hilbert (1897b, 332) stated, to his critique of the traditional notion of derivative. Contrary to Hilbert's statement, Dugac (1973, 76) claimed that it was the discovery of his counterexample that led Weierstrass to the critique of the “traditional” concept of derivative. “Hilbert's mistake can be understood”, Dugac went on, by taking into account that “he knew Weierstrass's analysis through the lecture notes of Weierstrass's 1878 course” taken by his friend Hurwitz (Weierstrass 1988a) that seems to justify Hilbert's conclusion (see Sect. 6.5.5).

In our view, on the contrary, Weierstrass was primarily concerned with complex analysis. His power series approach naturally led him to a careful investigation of the conditions of analytical continuation of a power series element. In doing this he was also led to a closer investigation of the behaviour of power series on the boundary of their disk of convergence, and to the related discovery of gap series

which in turn, as shown above, was intimately linked to the existence of continuous nowhere differentiable functions via an astute use of uniform convergence.

Referring to the French translation of his (1880c) paper, Weierstrass observed to Schwarz on March 6, 1881, in a nicely understated remark: “My latest paper created more of a sensation among the French than it really deserves; people seem finally to realize the significance of the concept of uniform convergence”. On the same day, writing to Kovalevskaya Weierstrass was happy to observe that his own research on single-valued functions had acquired more value in his own eyes for it had opened the way to the work of young mathematicians, and this was “the most beautiful outcome a teacher and writer could wish for”. Among them Weierstrass listed Mittag-Leffler, Picard, and Tannery who had recently sent him an example “of more elementary nature” than his own (6.84). This was given by the series

$$\varphi(x) = \frac{1+x}{1-x} + \frac{2x}{x^2-1} + \frac{2x^2}{x^4-1} + \frac{2x^4}{x^8-1} + \frac{2x^8}{x^{16}-1} + \cdots,$$

whose sum is $\varphi(x) = 1$ or $\varphi(x) = -1$ according to whether $|x| < 1$ or $|x| > 1$. Weierstrass presented this example to the Akademie on February 21, 1881 and let it printed as an addition to his own paper (see [Weierstrass 1880c](#), 231–233).

Examples of real functions with a pathological behaviour were of interest to Weierstrass as limiting cases of analytic functions. Riemann too, Dedekind reported, found his examples of pathological functions looking at limit case of theta functions. In particular, in one of the relevant fragments later found in his *Nachlass*, Riemann referred to Sect. 40 of Jacobi’s *Fundamenta nova* to study the behaviour of the function $\frac{2K}{\pi}$ when its argument approaches a real rational value. Jacobi had shown this function can be expressed as $(1 + 2\sum_{n=1}^{\infty} q^{n^2})^2$. Commenting on this, Dedekind observed¹³⁰ that Riemann’s calculations were likely to be related to his Habilitationsschrift and his search for examples of functions that in every interval become discontinuous infinitely many times. But Riemann’s and Weierstrass’s attitudes were quite different in this matter. As Klein remarked (1926–1927, 1, 286), “Riemann excluded the appearance of natural boundaries from his considerations. Weierstrass, on the contrary, was led by his systematic manner of thought precisely to look closer at the behaviour of an analytic function in the neighbourhood its natural boundaries”.

In the concluding lines of the paper [Weierstrass \(1880c, 223\)](#) claimed that “it is easy to produce uncountably many other power series of the same nature as the previous ones, and even for an arbitrarily bounded domain of the variable x to demonstrate the existence of functions that cannot be continued beyond this domain; however, I do not enter this here”. In modern terms, this claim amounts to saying that any domain in $\mathbb{C}$ is a domain of holomorphy. However, [Remmert \(1998a, 119\)](#) is right when remarking that Weierstrass “gave no precise statement, far less a proof of this claim”. The proof was to be given by Runge a few years later, in 1885

¹³⁰See Riemann, *Werke*, 498.

(see Sect. 8.6). Some years earlier, however, [Poincaré \(1883c\)](#) and, independently [Goursat \(1882\)](#), re-published in revised form in (1887)) had produced proofs for the case of a continuum bounded by a smooth curve that [Osgood \(1898a\)](#) summarised and generalised in the light of Cantor's set theory.

6.9.3 *The 1884 Lecture at the Mathematical Seminar*

Weierstrass summarised his approach to function theory, and his criticism of Cauchy's and Riemann's, in a lecture he gave at the Mathematics Seminar on May 28, 1884. This was occasioned by recent papers by Appell, Picard, and Poincaré (see Sect. 9.3.1) who extended results of his (1876b) paper on single-valued functions "partly following another way" different from his own. "Actually much can be done more easily by means of Cauchy's theorem", [Weierstrass \(1925, 1\)](#) admitted. Therefore, he felt necessary to explain "what has led me to follow the route I have followed". This was deeply connected with "the fundamental trends" he chose to develop in function theory.

As already mentioned in Sect. 6.2, Weierstrass made a point of building analytic function theory without resorting to the Cauchy integral theorem, a view he had explained in a letter to Schwarz on December 20th, 1874. He first stated Cauchy's theorem without mentioning Cauchy by name, then he added that "nowadays, following Riemann's (or Dirichlet's?) approach the proof is usually obtained from the consideration of a double integral". As we have seen, this had been done for instance by Briot and Bouquet and by Betti. "I do not consider this to be completely methodical"—Weierstrass continued—"because in this procedure the true foundation of the theorem does not comes to light clearly enough". Instead, "in function theory" he gave the following theorem: "Let $F(x|a) = b_0 + b_1(x-a) + \dots$ be a function element, and let it be proved that in a simply connected surface containing the place (*Stelle*) a it can be continued along *every* [Weierstrass's italics] path coming out of a , then through it a single-valued, analytic function is defined for the points of the surface".

Weierstrass considered this theorem as "indispensable" for function theory. The integral theorem was a "direct consequence" of the above-stated theorem that moreover provided "in the very same way" the foundation for functions defined by a linear differential equations with coefficients given by rational or algebraic functions, particularly for all the functions having only a finite number of singular points. To prove this Weierstrass considered simply connected surfaces as defined by Schwarz in his (1870c), namely surfaces built up by overlapping pieces of surfaces whose boundaries are everywhere convex. Indeed, Weierstrass concluded, for any such a piece of surface, the theorem can be easily established. "I have always believed that Cauchy had been the first to equate the contour integral $\int Mdx + Ndy$ to the surface integral $\iint \left(\frac{\partial M}{\partial y} - \frac{\partial N}{\partial x} \right) dx dy$ ", Schwarz replied on February 2, 1875, referring to the relevant notice in [Casorati \(1868a, 78–79\)](#). Then he went on to offer

a proof of the theorem stated by Weierstrass before turning to a problem related to minimal surfaces that he had dealt with from time to time since 1871.

In his lecture at the Mathematical Seminar in 1884 Weierstrass emphasised strongly that he did not start from “a more or less arbitrary definition of an analytic function” but one that related the concept of a function to the dependence of magnitudes on fundamental arithmetic operations in order to get the concept of a single-valued function. A definition of such a function according to which “for every value of the argument one has a well-determined value of the function” is “a definition that says nothing” when the argument is complex. It had once been thought that it was enough to add to this a continuity condition in order to prove the existence of a derivative and, consequently, to get (at least locally) the Taylor expansion of the function. However, his discovery of both continuous nowhere differentiable functions and series having natural boundaries convinced him to have nothing to do with “these old definitions”. But “all the difficulties vanish”, Weierstrass claimed, when one follows his power series approach. Having summarised the method of analytic continuation, including a discussion of the different nature of singular points, in the concluding part of the lecture he addressed his criticism more precisely to Cauchy and Riemann.

According to Weierstrass, Cauchy wanted $f(x)$ to be a function of x if:

1. For any x in a given domain there exists a well-determined value of $f(x)$;
2. The derivative $f'(x)$ exists (herewith the continuity of $f(x)$ is assumed) and is unique and continuous.

This means, said Weierstrass, the derivative is defined as the limit $\frac{f(x+h)-f(x)}{h}$ as h becomes infinitely small, but Cauchy did not tackle the question of when such a limit exists.¹³¹ “In his day one generally assumed that such a derivative always existed, at least for real quantities”. But when x is complex, the value of the limit depends on the way h approaches zero. Cauchy took as a definition the fact that the quotient approaches a well-determined limit $f'(x)$ independently of how h approaches zero. Then Cauchy proved that a function defined that way can be expanded in power series, but his proof was not based on “the first elements of analysis” but on the concept of integral. Hence Weierstrass’s dislike, for he wanted “function theory to be founded by means elementary theorems on basic operations”.

Weierstrass seems to attribute too much to Cauchy, including much of what Riemann had done. And what about Riemann’s contributions? They did not amount to all that much, in Weierstrass’s late reconstruction. Once Cauchy’s definition is given, it is matter of easy calculation to get the Cauchy–Riemann equations that Riemann took as the definition of an analytic function. “At first sight this definition looks arbitrary”, Weierstrass commented, “but Riemann completed it by stating that, as the Cauchy–Riemann equations are satisfied by all the laws of dependence expressed by arithmetical forms, they represent a common property of

¹³¹To tell the truth, [Cauchy \(1823a, 22\)](#) warned the reader by adding the condition “if it [the limit] exists”.

all known function, and therefore he [Riemann] took it as a definition". According to Weierstrass, Cauchy had already both the Cauchy–Riemann equations and their geometrical meaning although he did not put them at the basis of the theory. Riemann's definition is perfectly justifiable, Weierstrass concluded, but it is subject to the same criticism as Cauchy's. It has the property that through it one easily obtains the concept of continuation. But when Riemann wanted to prove the possibility of this continuation he was forced to move to power series, as he did (not in his papers but in his lectures) by resorting to theorems of integral calculus.

Weierstrass further remarked that in Riemann's definition of a complex function one is forced to assume the existence of first order partial derivatives of functions of two real variables, or even of second order partial derivatives when the corresponding Laplace equations are considered. Of course, things get worse when one passes from one to several complex variables. If, on the contrary, one starts from power series only "the first elements of arithmetic" are needed. He himself was strengthened in this view by the remark that the very same approach could "more easily" be followed for functions of several variables. He used to prove this in his lectures on the introduction of analytic function theory, and he also did it in his very last lectures in 1886.

6.9.4 Weierstrass's Last Lecture on Function Theory

Weierstrass's SS 1886 lectures dealt with selected chapters of function theory. The first part of the course was devoted to real analysis, and it is not of our primary concern. There Weierstrass tackled, for the first time in 30 years, a subject he had taught only once at the very beginning of his Berlin career, namely the representation of arbitrary (real) functions in series, and in doing this he stated and proved the approximation theorem today named after him. This part concludes with Weierstrass's critical remarks on Riemann's definition of the definite integral.

As with the Cauchy integral theorem, Weierstrass was also dissatisfied with Riemann's definition of the definite integral. He expressed his concern about that as well as his attempts at extending Riemann's definition in letters to his friends. Writing to du Bois–Reymond he lamented he had always been clear that Riemann's definition was marred by a defect which he had to put up with because he did not know how to avoid it. It had to do with Riemann's integrability conditions: in order to determine the greatest oscillation of the function over an interval one has to take into account the values that the function assumes at the points of discontinuity whereas the integral, if it exists, depends only on the values of the function at points where it is continuous (Weierstrass 1923a, 215–217). In a subsequent letter to him on April 20th, 1885 Weierstrass (1923a, 218) recognised that the definition in question should be modified "more energetically than he himself had done", and went on to claim that Dirichlet's (actually, Cauchy's) concept of definite integral was "in any case too narrow" and can be extended in another way than Riemann's.

On 16th May 1885 he wrote Kovalevskaya that “Riemann’s definition, which is believed to be the most general one one could think of, is insufficient”, and gave some details.¹³² Less than ten days later, on May 28 he repeated this statement to Schwarz, adding that Riemann’s definition “it is neither general enough nor, above all, admissible. It should perhaps be replaced by another, very different one for whose foundation Cantor’s most recent work (not the ones dealing with transfinite numbers) had provided me with essential tools”.

This was connected with the problem of the representation of functions by Fourier series, and with the concluding remarks of Dirichlet’s 1829 paper in particular. Weierstrass had begun to study this subject thoroughly in early 1885, as he announced in a letter to Schwarz on March 14th, 1885. He made it the object of a paper that he presented to the Akademie on July 9, 1885 (Weierstrass 1885), and eventually he expounded it in his summer 1886 lectures. There he presented his approximation theorem and his critical remarks on Riemann’s definition (Weierstrass 1886, 110–112). However, in spite of his efforts he did not succeed in finding the sought-for extension of Riemann’s integral.¹³³

The second, shorter part of Weierstrass’s course dealt with complex functions proper, beginning with a discussion of the nature of single- and many- (even infinitely many-) valued functions. The latter come in when one considers the inversion of a hyperelliptic integral, as Jacobi had done. This gave Weierstrass (1988b, 127) the occasion to clarify his view with respect to “Jacobi’s paradox” concerning the “absurd” nature of infinitely many-valued functions (see Sect. 4.3). Already in 1882, when editing Jacobi’s collected works Weierstrass had commented that Jacobi’s statement could no longer be upheld “from the point of view of present-day function theory”.¹³⁴

As usual, he discussed the matter in his letters to Kovalevskaya and Schwarz. Referring to the inversion of a hyperelliptic integral $u = \int \frac{dx}{\sqrt{P(x)}}$ he claimed that to one value of u correspond countably many x , “as Cantor has proved in a convincing way”. Weierstrass also expressed his regret that this claim could give Kronecker (and Fuchs) further grounds for to criticising his own (and Cantor’s) views. Fuchs had just presented a paper to the Berlin Akademie (Fuchs 1885) where he claimed that there exist linear differential equations which do not define proper “analytic” functions because of this “absurd” behaviour of the solutions (see Sect. 4.3). Were he right, Weierstrass commented to Kovalevskaya on March 24, 1885, then he [Fuchs] “would put the stamp of absurdity on the huge majority of functions whose properties he himself had tried to research with such great success”.¹³⁵

Fuchs’s paper provoked the immediate reaction of Casorati, who republished his old results on the periodicity of analytic functions in two terse papers in Mittag-

¹³²See Bölling (1993, 338–340) and also Weierstrass (1923b, 196).

¹³³See Hawkins (1975, 67).

¹³⁴See Jacobi (1881–1891, 2, 516).

¹³⁵See Bölling (1993, 329–330).

Leffler's *Acta* (as is also discussed in Sect. 4.3). Writing to Schwarz on March 14, 1885 Weierstrass asked the question: "given an arbitrary analytic function $f(u)$ could one prove that for each value of u that the corresponding values of $f(u)$ form a countable set so that they could be arranged in a series? No doubt the answer is affirmative", Weierstrass continued. He also claimed to be able to give a proof by resorting to minimal surfaces. By means of such a surface he made the whole family of the pairs $(z, f(z))$ —what he called a monogenic *Gebilde* of the first order—"much more intuitive to my mind than through the Riemann's surface", but, he went on, "certainly it will be possible" to prove it without such a geometrical help, for example by assuming Poincaré's uniformisation theorem of 1883 to be correct. Weierstrass had an objection to Poincaré's proof concerning what happened at points where the function ceases to be regular (see 8.3), and in his 1886 lectures Weierstrass (1988b, 130) limited himself to stating that the proof was "probably correct". The possible connection with the uniformisation theorem, pointed out by Weierstrass, was later to be emphasised by both Poincaré and Koebe in their papers in 1907 (see Sect. 8.3.2).

The above statement, generally known as Volterra–Poincaré theorem, seems to support the fact that Weierstrass had the theorem as early as 1885. As Ullrich (2000) has shown, the question answered by the theorem arises naturally in the context of Weierstrass's function theory, and of analytical continuation along "chains of discs" in particular, when one asks "how large" is the set of values a function $f(u)$ attains at a point $u = a$ after analytic continuation along all the different chains. Apparently the theorem had first been communicated by Cantor to Weierstrass "many years before", as Cantor explained in 1888 in a letter to the Italian mathematician Vivanti¹³⁶, who had just published the theorem (although with an incorrect proof¹³⁷) in the *Rendiconti del Circolo matematico di Palermo*. Vivanti (1888b, 150) recognised that the theorem "had recently been communicated" to him by Cantor who invited him to produce a proof. Vivanti attempted to prove the theorem by resorting to Riemann surfaces. The crucial point was the assumption that the sheets of the Riemann surface of a monogenic function that meet at a branch point are countably many in number and, "on the basis of the principles of set theory" also that the set of branch points of any sheet of the surface is in one-to-one correspondence with only countably many other sheets. Vivanti's 1888 paper, which connected the theorem with Poincaré's uniformisation theorem, produced a vivid reaction from Poincaré, and another from Volterra.¹³⁸ Poincaré sent a letter to the editor of the *Rendiconti*

¹³⁶Cantor's letter is published in (Ullrich 2000, 381–382). Some months later, on August 26, Cantor repeated the same claim in a letter to Volterra (published in (Israel and Nurzia 1984, 176)). According to Cantor, the theorem was "new" to Weierstrass. In both letters Cantor also added that "some years later" Weierstrass told him that he had been able to prove the theorem by resorting to minimal surfaces.

¹³⁷As immediately pointed out by Hurwitz in his review in *Fortschritte* 20, 393–394.

¹³⁸A former student of Betti's at the Scuola Normale in Pisa, Vito Volterra graduated in 1882 and was appointed professor of mechanics at Pisa the next year. In 1892, after Betti's death, he switched to the chair of mathematical physics. Then he moved to the University of Turin and was eventually

that was published in the same volume of the journal as Vivanti's paper (Poincaré 1888) in which the proof was based on Weierstrass's definition of analytic functions by means of power series and chained discs. Independently, but following the same Weierstrassian approach Volterra (1888) eventually produced a detailed, rigorous proof of the theorem.¹³⁹

In his 1886 lecture Weierstrass pointed out some essential features of his power series approach to the theory of functions of several complex variables, strongly emphasising the role of uniform convergence. He explicitly refused to take the n -sheeted Riemann surface as the proper foundation of function theory, he explained, because "it would be difficult to deal with them when there are infinitely many sheets". On the other hand, Weierstrass continued, Riemann himself—who "had a mathematical imagination like no-one else he [Weierstrass] had ever known"—had met difficulties he himself was not able to overcome in the case of manifolds of several dimensions (Weierstrass 1988b, 144). The lecture concluded with a sketch of some basic concepts and results of the theory, keeping in mind that "the ultimate aim is always the representation of a function".

6.10 Weierstrass's "School"

Among the founders of complex function theory, Cauchy, Riemann and Weierstrass, only Weierstrass felt acutely the need to build a "school" around him inspired by his own approach to analysis. This was a task that he pursued with increasing awareness during the many years in which he was shaping his *Funktionenlehre* in a more and more satisfactory way.

Although in the early period of his teaching in Berlin (up to 1864) Weierstrass was not entitled to be the supervisor of a doctoral dissertation, he always called Koenigsberger his "oldest student". Indeed, Weierstrass had suggested the subject of his thesis to Koenigsberger—express the movement of a point attracted by two centres by means of Jacobi's theta functions—and provided him with the methods for solving the problem. Koenigsberger graduated in 1860 with Kummer as supervisor, as Lazarus Fuchs had done in 1858 with a thesis on the lines of curvature of surfaces. In 1859 Kummer also supervised du Bois-Reymond's thesis on the equilibrium of fluids as well as Gordan's on geodetic lines in 1862. It is also worth

appointed to the chair of mathematical physics at Rome in 1900. In 1904 he was named a Senator of the Italian Kingdom. Volterra contributed to many areas of mathematics, including functional analysis where he played a pioneering role. He became increasingly involved in academic and institutional work until 1932 when he was forced to resign all of his academic positions because of his refusal to sign an oath to the Fascist regime. For Volterra's biography, see Goodstein (2007). For an account of his scientific work, see Somigliana's obituary in (Volterra, *Opere*, 1, XV–XXXIII).

¹³⁹For a detailed account of the story, see Israel and Nurzia (1984) that includes the relevant letters exchanged between Volterra, Vivanti, and Cantor.

noting that Riemann's student Prym graduated from Berlin in 1863 with a thesis on hyperelliptic functions inspired by Riemann's geometrical approach. His thesis got a positive report from Kummer, and Prym graduated with the highest grade, *eximia cum laude*.

A decisive impulse for the creation of a mathematical school in Berlin was given by Kummer and Weierstrass in 1860 when they established a Mathematical Seminar aiming at "the scientific education of the students of mathematics through exercises and training in their own work" (quoted in [Biermann 1973](#), 73). The activity at the Seminar joined the lectures at the University to make Berlin the most attractive center for mathematical students coming there from Germany and abroad.

As [Biermann \(1973, 75\)](#) has rightly stated, the period of time from 1864 to 1892 represented a high point in the history of mathematical teaching and research at the Berlin university. As Weierstrass himself proudly stated in later years (quoted in [Biermann 1973, 75](#)):

Through the combined work of Kummer, Weierstrass, Kronecker it has been possible to organize the mathematical teaching according a comprehensive plan, which willing and successful younger forces later joined in such a way that students were given the opportunity to attend a relevant series of lectures on the most important mathematical disciplines in an appropriate sequence for a 2-year course, not a few among these being absolutely not, or at least not regularly, taught at other universities. This had the effect that for several years now ... not only those mathematical students from all parts of Germany who aim at a higher education in their science, but also not a few young, foreign people wanting an academic career, sometimes being sent by their governments from Austria, Hungary, Italy, Switzerland, Sweden, Denmark, Russia, America, and in recent years even from France, gather here in Berlin and most of them follow a complete course.

Once he was appointed Ordinarius in 1864 Weierstrass was officially entitled to supervise doctoral dissertations. Beginning with Wilhelm Biermann in 1865, there is an impressive number of students who may be properly included in Weierstrass's "school" (see [Biermann 1973](#), 84–100). Biermann's thesis dealt with the solution of mechanical problems by means of elliptic functions, and in the introductory pages he provided an early summary of Weierstrass's theory of elliptic functions, including the function $\wp$, the differential equation it satisfies, the invariant g_2, g_3 and so forth. This was followed by a thesis on the expansion in series of spherical functions by Wilhelm Thomé, who also graduated in 1865 and later became the first professor of the Weierstrassian "school" to teach at Berlin University.

The list of Weierstrass's graduate students includes such first-rate mathematicians as Georg Frobenius, Eugen Netto, Ludwig Kiepert (all of them in 1870), Wilhelm Killing (1872), Friedrich Schottky (1875), Arthur Schönflies (1877), Hans von Mangoldt (1878), and Friedrich Schur (1879), not to mention the large number of German and foreign mathematicians who at various times attended some of Weierstrass's lectures. Somewhat ironically, however, the list does not include those whom Weierstrass himself considered as his closest pupils, Schwarz and Kovalevskaya. To them Mittag-Leffler may be added, for he considered himself to be a faithful student of Weierstrass and did his best to spread his teacher's

Fig. 6.6 Hermann Amandus Schwarz (Bölling *Das Fotoalbum für Weierstrass*, Vieweg ©1994, p. 16.4)



methods and views in analysis. A closer look at Weierstrass's relationships with all three of them will shed some light on the character of the latter's mathematical "school".¹⁴⁰

6.10.1 *The "Dear Friend and Colleague" Schwarz*

As we have seen (Sect. 6.3.3) in summer 1861 Schwarz had attended the first course by Weierstrass on differential calculus (Fig. 6.6). Later on, in 1863–1864, he attended the latter's lectures on the theory of analytic functions. As he himself later remembered, it was when attending that course that his classmate Mertens remarked on the peculiar fact that "the existence of a conformal mapping of a triangle, for instance, onto a disk had already been proved by Riemann whereas the actual determination of such a map, because of the discontinuities of the boundary line which lie at the angles, still seemed to surpass the power of analysis". At that time, Schwarz commented, he himself knew no case of surface with prescribed boundary that was mapped conformally on a disk.

Under Kummer's supervision Schwarz graduated in 1864 with a thesis on developable surfaces but, at Weierstrass's suggestion, from that time he became more and more interested in Riemann's work. One might say that Schwarz set

¹⁴⁰We note also Cantor's opinion, expressed in a letter to Klein of 8 December 1895, that "One must distinguish with Weierstrass between what he really did and the myth in which his pupils have enveloped him, so to speak like a thick fog, in order to strengthen and elevate their own reputations". We agree with Dugac that this opinion merits serious consideration. Cantor's comment and Dugac's addition are in his edition of the Poincaré correspondence (Dugac 1986, 111).

himself the task of re-deriving with Weierstrass's analytic methods what Riemann had done in a geometrical manner. His correspondence with Weierstrass, quoted in the previous sections, provides considerable evidence of this.

In his letters to Schwarz Weierstrass occasionally took the opportunity to ask his "dear friend and colleague", the "master of representation", for advice on relevant matters, as he did on April 21, 1875 with Schottky's thesis. This dealt with "the necessary and sufficient conditions for a $(n + 1)$ -connected, plane surface to be represented on a similarly shaped surface, and it contains a series of nice, and as far as I know, completely new theorems, so that for a long time I doubted whether all of them were well founded (which was actually not the case without exception so that the dissertation must be partly re-worked before being printed)". After stating that he was very curious about Schwarz's advice, Weierstrass went on with a pleasing picture of the author, "a sensible, somewhat dreamy person who sometimes is able to find the right idea only by instinct ('I cannot prove it, however it's right', is a usual way of speaking with him), thus ill-suited for practical life so far".

As the leader of a school should be, Weierstrass was always concerned with the academic career of his former students. For Schwarz, Weierstrass, jointly with Kronecker, successfully managed to ensure that his former student was hired at Göttingen in 1875. Weierstrass was very happy that the hope he had had for years—to have Schwarz teaching at a German university—had eventually been fulfilled. "Göttingen is now the right place for you", he wrote to Schwarz on June 5, 1875. "The mathematical sciences are considered there as hardly anywhere else, and there will never be lack of good students if the right teachers are there". In fact, Weierstrass saw Schwarz's hiring at Göttingen as an opportunity to expand his own influence in Germany beyond Berlin.

You have told me often—he went on—that you wanted to remain a student of mine even after the university years were over. I accept this in the sense that you are one of the few who in their later development have not repudiated the principles that I deemed it my principal task to instill in my students, whose essence lay in the requirement to regard clarity and truth as the most necessary things in science, and to shun and hate nothing more than empty talk about half-understood things, which unfortunately tries to make itself appreciated even in the most serious and pure of the sciences. Of course, one has to be led in one's own work by the conviction that the attainment of general results is the final aim, which, however, can be attained only by following the path of deep research.

With these flattering words Weierstrass seemed in fact to describe himself and his own approach to mathematics. This passage also reminds us of the "confession of faith" he made to Schwarz on October 3, 1875.

Academic matters were frequently discussed in their correspondence. Interestingly enough, there is, however, no mention in it of the important change that occurred at Berlin in 1882 when Kummer surprisingly announced to the Faculty that he was going to retire. Schwarz was Kummer's son-in-law, and as Mittag-Leffler maliciously commented in a letter to Hermite on August 3, 1882 "he [Schwarz] has always held it as a natural right to become his [Kummer's] successor, all the more so because Kummer's influence on the government is very high, so high that it is a *cancan* spread all over Germany that whoever marries a daughter of Kummer—

and they are so ugly and clumsy that you can not imagine—also marries a chair in mathematics”.¹⁴¹ However, things went differently.¹⁴² Kronecker was appointed Ordinarius, and Weierstrass energetically managed to arrange that a third chair of mathematics be created for Fuchs. He succeeded, and eventually Fuchs was hired at Berlin in 1884. Thus, [Biermann \(1973, 90\)](#) commented, Weierstrass had achieved the goal he had pursued for a long time.¹⁴³ But, with Kummer’s retirement and the hiring of Kronecker and Fuchs as professors at Berlin “this glorious period came to an end. The following years [up to 1892] still belong to the same era, but they are not at the same high level any more”.

In his letters to Schwarz, Weierstrass did not withhold remarks and suggestions about the policy of appointments of professors at German universities, even when that possibly involved his former students. Of particular interest here is the exchange of letters that occurred between them in the late 1885, when Klein left Leipzig for Göttingen. The start is amusing. It was matter of who would obtain Klein’s chair, and on December 7 Schwarz wrote to Weierstrass that he had received a short, hasty letter from Klein who said confidentially that in order to replace Klein himself the Faculty had proposed the following list to the ministry: (1) Lie; (2) Lindemann; (3) Voss. Because of Klein’s hardly legible handwriting Schwarz guessed at first that at number 1 in the list one had to read “Sie” (you) instead of Lie, and was “very pleased” by that. Reading it as “Lie” was also possible, he added, but the proposal of Lie seemed to him “not very likely, although possible”.

But Schwarz’s guess was wrong. “I’m very sorry that Klein’s hasty handwriting has caused you a great delusion”, Weierstrass replied on December 20. He knew of the proposal made by the Faculty at Leipzig and added that “Kronecker did not want to believe it”. Had Leipzig been a Prussian university, Weierstrass would have felt his duty to take a position openly about “such an unheard-of procedure” that was “an offense to all presently active German mathematicians”. Weierstrass’s opposition—at first sight grounded on the defence of his young students and colleagues—was in the end grounded on nationalist reasons that led him to a surprisingly unfair, and above all historically unfounded, evaluation of Lie’s stature combined with a deep underestimation of Klein’s. “I will not deny—Weierstrass went on—that Lie has produced noteworthy works, but he is not a man of such great significance, either in a scientific respect or as a teacher, that he, the foreign one, should be preferred to all the German nationals whom one could take into consideration. Now it is said he is a second Abel worthy of this prize. A nice start of the new era that is going to begin under Klein’s presidency! Du Bois–Reymond, who sometimes hits the nail

¹⁴¹See ([Dugac 1973, 159](#)).

¹⁴²For a detailed account, see ([Biermann 1973, 98–99](#)).

¹⁴³Mittag-Leffler wrote to Cantor in 3 May 1883 that “Schwarz has been to Paris . . . It goes without saying that he spoke ill of his colleagues. . . . The point of his trip was to swing the sympathy of the French towards him and away from Fuchs in a struggle that he thinks he will be engaged in after the death of Kummer or Weierstrass. He has greatly flattered the young French and above all has sought to win the goodwill of Poincaré”. See ([Nabonnand 1999, 126](#)).

on the head, had already labelled the trio Klein–Lie–Mayer for years as the ‘*société thuriféraire*’ [society of sycophants]”, Weierstrass concluded.

Schwarz was not an easy man at all, and often took the opportunity to argue with colleagues, in Germany and abroad. Weierstrass was very much concerned about this, particularly when it involved some of his closest students. Such an argument occurred with Mittag-Leffler. Being well aware of it, when Mittag-Leffler planned to visit Weierstrass in summertime in 1888 to work jointly on the evaluation of the papers submitted for the Prize to be awarded by King Oscar the Second of Sweden, Weierstrass wrote a long letter to Schwarz on August 16 to express his wish that he (Schwarz) and Mittag-Leffler could find a suitable *modus vivendi*. It was not just a matter of personal relations. Weierstrass was clear in stating that a suitable style of life had to be established “among all the people on whose mathematical education [he] had some influence”, and that possible arguments among them “should not be aired in public”. It was no secret that he did not want the achievements he had obtained in the field of science through unceasing work to sink before the further, requisite research be completed and put in the right place. Now, he went on, “I must endure the experience that my methods, and the results obtained through them, are not only recognised abroad—in France, Italy, and Sweden, but also—what is much more important, further developed while in our country they are systematically attacked, and presented as aberrations”. There is no doubt that Weierstrass was tacitly referring to Kronecker. To his “greatest regret”, the fact that those attacks were not rendered ineffective was largely the fault of many who “call themselves my students. Were they soundly together they would build a power that would have to be respected”.

Weierstrass was above all concerned about the future of his “school”, as he stated in a passage that is worth quoting in full:

However, as things now stand, I fear—and I have every reason to do so—that in a short time the most influential chairs at our universities will be taken by people about whom nobody thinks at all today. The younger mathematicians who call themselves my students, Hettner, Knoblauch, Hölder . . . will come to nothing; the older ones, to whom you belong, will stay isolated, and will feel limited in their efficacy. And with all the authority of education the opinion will become that ‘Weierstrass himself achieved really noteworthy things, but only second-rate people came out of his school so that one has to conclude that his road was not the right one’.

Rather prophetic, one would say! Weierstrass, who in 1883 had preferred Fuchs to Schwarz as Kummer’s successor, managed to overcome Kronecker’s opposition to Schwarz when he retired in 1892, and it was matter of his own succession, and to convince him that his former, faithful student was the right person to be hired at Berlin. Weierstrass was successful, but when Schwarz arrived at Berlin the parabola of his productive work had already turned downwards, so much so that he had already published his collected works.

A vivid picture of the mathematical school at Berlin in the early twentieth century is provided by Volterra, who visited Berlin in 1904. Commenting on the former students of Kronecker and Weierstrass who were now the professors at Berlin Volterra wrote to his wife on February 18th: “At any rate, they are suggestive figures because they make us think, and there are certainly great ruins. They make us

think because they show that Weierstrass and Kronecker with all their genius did not succeed in anything that they hoped to accomplish. They created neither a new mathematics nor a school of their own. Their genius has been more advantageous to France than to their own pupils” (Goodstein 2007, 141–142; transl. corrected). Some days later, after a meeting of the Berlin Mathematical Society he commented to his wife: “This poverty of the current Berlin mathematicians! So last night at the meeting of the Berlin Society: there was not one distinguished personality and the papers presented were old stuff pulled out, which showed the absence of a modern guiding mind” (Goodstein 2007, 141).

6.10.2 The “Dear, Faithful” Sonya

The academic and scientific character Weierstrass always maintained in his relationship with Schwarz turned to a more intimate one when he dealt with Sonya Kovalevskaya. To begin with, in his letters—and one may presume in their conversations—he switched very soon from the more official “Sie” with which he used to address all of his colleagues to the more familiar and intimate “Du”. Not to mention expressions like “my dear heart” that Weierstrass occasionally used in his letters, and which seem to suggest some even more intimate relation, at least on his side. Be that as it may, as a matter of fact Weierstrass followed Kovalevskaya’s mathematical education step-by-step, and her later achievements with a care that he reserved for nobody else among his protégés. He also arranged with Fuchs, by then teaching at Göttingen, to let her graduate *in absentia* from Göttingen in 1874.

In turn, Kovalevskaya provided new hints for Weierstrass’s research, as occurred with her thesis. There she had produced the following example: consider the function $f(x) = \frac{1}{1-x}$. The power series $\sum_{n=0}^{\infty} \frac{(2n!)}{n!} \frac{(t^n)}{(1-x)^{2n+1}}$ satisfies the differential equation $\frac{\partial \varphi(x,t)}{\partial t} = \frac{\partial^2 \varphi(x,t)}{\partial x^2}$ formally and reduces to $f(x)$ for $t = 0$. However, the series is not convergent for any $t \neq 0$. Writing to her on May 6, 1874 Weierstrass commented that this provided him with “the starting point for some very interesting research”. He gave a hint of them in the letter¹⁴⁴ and referred to this when writing to du Bois-Reymond some months later: “I was of the opinion—Weierstrass (1923, 204) stated—that a power series of several variables that satisfies a given partial differential equation formally should also be always convergent within a given domain, and therefore should represent a function that really satisfies the differential equation. That this is not the case has been discovered to my great surprise by my woman student by herself”.¹⁴⁵

¹⁴⁴See (Bölling 1993, 121–126). Weierstrass returned to this on May 9, by proposing her “a little exercise” concerning the same differential equation. See (Bölling 1993, 126–130).

¹⁴⁵For Kovalevskaya’s mathematical work, see Cooke (1984). Some ten years later Weierstrass resumed the subject in his (1885).

Eventually, in 1883 Weierstrass recommended Kovalevskaya to Mittag-Leffler for a teaching position (see Bölling 1993, 413–429). By the end of 1883 she had moved to Stockholm from where she wrote Weierstrass that she planned to give her first course on partial differential equations (see Bölling 1993, 303). Interestingly enough, in his answer Weierstrass objected that it would not be easy for Kovalevskaya to explain to her students the “true goal of her research” were they are not “suitably familiar with the foundations of function theory in his sense”. Indeed, it was matter of showing how analytical *Gebilde* could be determined by algebraic, partial differential equations (see Bölling 1993, 305).

In his letters to Kovalevskaya Weierstrass used to express not only his views on mathematical subjects but also his more intimate thoughts, which he avoided doing with Schwarz. Thus, for instance, on August 27, 1883, writing about Kronecker, he dropped into an anti-Semitic tone by stating that the latter, like “many highly intelligent people, namely the ones from the Semitic race, does not posses sufficient phantasy (intuition I should better say), and it is true, a mathematician who is not somehow a poet, will never be a complete mathematician”. (Be it said in passing that it is difficult to discover traces of poetry in Weierstrass's works!) To support his claim Weierstrass went on with a sort of racial comparison that in his view showed “in fully runaway manner that Abel stands over Jacobi, Riemann over all of his contemporaries (Eisenstein, Rosenhain), Helmholtz over Kirchhoff (although in the latter there is no drop of Semitic blood)” (see Bölling 1993, 292).

Kronecker was also the subject of a long, sad letter on March 24, 1885 where Weierstrass complained that his colleague exerted all his authority to sink Weierstrass's “so-called analysis” (see Bölling 1993, 329–331).

On the other hand, Weierstrass did his best to support Kovalevskaya's mathematical work. Thus, for instance, in a long, detailed letter on August 16, 1888 he explained the content of Kovalevskaya paper on the motion of the top (see Sect. 7.10) so that, when she got the Bordin Prize, Weierstrass proudly wrote her on February 1, 1899 that he was particularly happy that “competent judges” have established that his “faithful woman student” (his “senile weakness”—as Schwarz used to call her, according to Mittag-Leffler) was not at all “nothing else than foolishness” (see Bölling 1993, 395).¹⁴⁶

6.10.3 Mittag-Leffler's Network

An interesting, and perhaps more balanced, picture of Weierstrass's “school” was provided by Mittag-Leffler, who in his letters to the French mathematicians (and Hermite in particular) used to compare what was going on in Germany with the French mathematical milieu.

¹⁴⁶Kovalevskaya's paper was published as her (1889), and republished as her (1890).

After looking at the mathematical schools in Paris and Berlin Mittag-Leffler wrote to Hermite on May 17, 1881: “You are very happy, dear maître, to have three pupils like Picard, Appell, Poincaré. Weierstrass has been a Professor in Berlin for 30 years, and has not a single pupil who could be compared with one of those three—excepting Mr. Fuchs, as goes without saying” (in [Dugac 1973](#), 156).

Even more crude was the portrait of Schwarz he painted to Poincaré on July 18, 1882 ([Dugac 1984](#), 162):

M. Schwarz is a talented mathematician who has studied Weierstrass’s ideas very thoroughly and edited his works with an admirable clarity and exactitude, but he has produced scarcely any work of importance. He is too busy with his numerous lectures. He wants to make a lot of money and so he gives a lot of lectures, 14 a week. This is the way German professors enrich themselves” ([Nabonnand 1999](#), 99).

One week later Hermite replied to Mittag-Leffler that “M. Schwarz is an uhlan of the advance guard, eager to begin the battle”. ([Dugac 1984](#), 162)

By 1881 Mittag-Leffler had moved to a chair at the Stockholm College that would later become Stockholm University. In the late spring of that year he met Lie for the first time, who came up with the idea for founding a Nordic mathematical journal and suggested that Mittag-Leffler should be the editor (see [Domar 1982](#)). Mittag-Leffler seized on the idea and acted quickly and energetically to realise it so that in December next year he was able to present the King with the first issue of the journal, *Acta mathematica*.

In editing *Acta* Mittag-Leffler showed all of his political skills. His success in establishing an extensive network of personal relationships enabled him to ensure the contributions of outstanding French and German mathematicians, and *Acta mathematica* very soon became one of the leading journals in the field of analysis. Even Weierstrass, the editor of the oldest surviving mathematical journal, had to recognise in a letter to Mittag-Leffler that he had succeeded from the very beginning in having as collaborators for his new journal both many old masters and young, emerging talents.¹⁴⁷

Among them there was the most talented French mathematician, the rising star in the Paris mathematical milieu, Poincaré, who promised him “never to leave *Acta*”. When Poincaré began working on automorphic functions, and was in competition with Klein, Hermite confidentially asked Mittag-Leffler about the views of the Berlin mathematicians. Answering Hermite, on July 7, 1881, Mittag-Leffler began by pointing out that he himself had very good relations with Klein. To tell the truth, he went on, Weierstrass “thinks that Mr. Klein is a man who is not lacking in talent but who is very superficial, and who is even something of a charlatan. Kronecker thinks that he is merely a charlatan without real merit. I believe—Mittag-Leffler added—that this is also Kummer’s opinion. Klein had studied at Berlin, but he has been a not very grateful student who profited from neither Weierstrass’s nor Kronecker’s lectures” (quoted in [Dugac 1984](#), 250). On his part, [Klein \(1926–1927](#),

¹⁴⁷See ([Behnke 1966](#), 31).

1, 284) confessed later with some regret that during his stay in Berlin in 1868 he, like Lie, never followed a course by Weierstrass “out of a spirit of contradiction”. Instead, he gave a talk at the mathematical seminar on non-Euclidean geometry and limited himself to transcribing a set of lecture notes from a course of Weierstrass on elliptic functions.

On the other hand, it is interesting to have a look at the picture of Weierstrass that Klein provided many years later:

According to my recollection, Weierstrass's position was that of an absolute authority whose teaching was accepted by his listeners as an indisputable rule, often without having rightly understood it in a deep sense. No doubt could be raised, any checking was difficult to do because Weierstrass was extraordinarily lacking in quotations. In his lectures he had set himself the goal of expounding a system of well-ordered, connected thoughts. Thus he began with a methodical construction from bottom up, and pursuing his ideal of completeness he set out the path in such a way that afterwards he needed to refer only to himself (Klein 1926–1927, 1, 284)

After referring to Weierstrass's complaints as expressed in his letter to Kovalevskaya quoted above, Klein added that Weierstrass was wrong in being too much concerned with the critiques of his closest colleagues, for “earthly things are subject to the eternal law of change”, to quote his rather philosophical conclusion.

6.11 The Legislator's Heritage

In Remmert's forceful opinion “Weierstrass certainly was overestimated in his day”.¹⁴⁸ The theory of Abelian functions, which Weierstrass considered the “true content” of his mathematical work, did not have the influence he hoped for it. Instead, “Riemann's ideas turned out to be more fruitful”. On the other hand, Remmert added, it would be unfair to consider Weierstrass as a mere school teacher only able to bring rigour and clarity into mathematics. Thus, “what made Weierstrass famous?”, as Behnke (1966, 26) asked. No doubt his epoch-making solution of Jacobi inversion problem in the hyperelliptic case greatly contributed to it. It was because of that work that the 40-year-old Weierstrass was recognised as “our teacher”, as Richelot rather emphatically hailed him. In Hilbert's words, this “rightly remains regarded as one of the greatest achievements in analysis” (Hilbert 1897b, 331) “Unfortunately, everything in life comes too late”—which seems to have been Weierstrass's comment.

In his Berlin years Weierstrass tried to extend this result to the more general Abelian integrals. According to Mittag-Leffler (1923a, 53), “the real reason why Weierstrass decided so late” to publish his theory—or, better, to let his theory be published by Hettner and Knoblauch—lay in the fact that “he succeeded only in very old age in finding the keystone to the harmonious, self-contained building on which

¹⁴⁸See (Weierstrass 1988a, ix) .

he wanted to erect Abelian functions". He wanted to derive the properties of the most general Abelian functions from three different points of view: the addition theorem, periodicity, and the theory of algebraic differential equations, so that, starting from any of them one would have to reach the same class of functions. Once this had been done, Weierstrass argued, he would have succeeded in "bringing a whole chapter of science to an effective conclusion". However, the first two viewpoints revealed themselves to be more general than the third one.

This is because, as Mittag-Leffler went on to explain, the "Rang"¹⁴⁹ of algebraic curves of genus $p > 1$ is $3p - 3$, whereas the number of arbitrary constants of possibly relevant theta functions in p variables is $p(p + 1)/2$, and these two numbers agree only if $p = 2$ or $p = 3$. When $p > 3$ the second number is greater than the first one, thus there are periodic functions that are more general than the ones defined by algebraic equations. "Now", Mittag-Leffler commented, "what should come in in place of the differential equations? Weierstrass struggled almost all his life with this question".

In Weierstrass's view, even Riemann's theory failed in this respect. Being unable to overcome this difficulty Weierstrass was forced to withdraw the continuation of his 1856 paper, and eventually to give up to its planned replacement. Hints at the results of his research are provided by the papers he published in 1869 and 1876. Mittag-Leffler (1923a, 55) reported that in summer 1888, while he visited Weierstrass, the latter finally found the solution of the problem that had occupied him his whole life. "The rank of an algebraic equation played no role any more. The true class number was something completely different, and through its introduction the whole theory acquired a completion and, at the same time, a simplification that surpassed anything Weierstrass had dared to hope". Because of his poor health, Weierstrass was only able to produce the fragments of his planned work that were eventually published posthumously in his *Werke* (Weierstrass 1903), and in the end, he was unable to reach the goal. The theory of analytic functions of several complex variables required a new sophisticated geometrical approach that was to be created in the twentieth century. So, in spite of his inconclusive efforts, what assured Weierstrass's fame were the results he found on the way towards setting up a satisfactory theory of Abelian functions.

Notoriously, Weierstrass published much less than he had actually discovered. Mittag-Leffler described this attitude to Hermite in these touching words: "maybe you know that Mr. Weierstrass has the greatest difficulty in writing. The blood rushes to his head as soon as he puts pen to paper. This unfortunate mood is one of the reasons why he published so little".¹⁵⁰

Be that as it may, as a matter of fact Weierstrass presented many of the results he achieved over the years in his lectures, where he built the theory of analytic functions from the very foundations according to his "confession of faith"

¹⁴⁹By Rang (rank), Mittag-Leffler meant the dimension of the space of algebraic curves of a given genus.

¹⁵⁰See (Dugac 1973, 155).

that function theory has to be built on the foundations of algebraic truths. By rejecting Riemann's global, geometrical approach Weierstrass reconceived the study of analytic functions from a local point of view. This gave a privileged role to power series expansions converging in a disk or in an annulus around a point. Accordingly, he developed a power series approach to all of complex function theory, placing a particular emphasis on the concept of uniform convergence. As [Boutroux \(1908, 2\)](#) summarised it, "for Weierstrass's school, to define a function is essentially to give a Taylor series, since from this series one can *theoretically*, by the method of analytical continuation, deduce the value of the function at every point where it is defined".

All the subtleties he propounded, including the $\varepsilon - \delta$ -definitions, the Bolzano–Weierstrass theorem, the sharp distinction between a function attaining a given value at a point and getting arbitrarily close to it, upper and lower bounds and so forth, as well as his striking, celebrated counter-examples to commonly held views, were introduced as part of this programme. This rigorous approach made of Weierstrass "the great legislator of mathematics" as Hermite called him. His epsilonotics—as later critics called his $\varepsilon - \delta$ -approach—may have had "a sobering effect on mathematical fantasy", but certainly revolutionised mathematics, as Remmert has said (in [Weierstrass 1988a](#), x). As [Hilbert \(1926, 161\)](#) later stated: "Weierstrass provided a sound foundation for mathematical analysis through his criticism carried out with masterly insight", adding that he himself conceived of his work on the foundations of mathematics "directly as a necessary continuation of Weierstrass's work".

Combined with the new, topological results provided by Cantor's set theory, Weierstrass's subtleties helped shape the modern form of real analysis, and the theory of Fourier series in particular—fields that were not particularly close to Weierstrass's heart. But they were of crucial importance even in analytic function theory. His distinction between poles and essential singularities, the Casorati–Weierstrass theorem, the concept and analysis of natural boundaries, the Weierstrass representation theorem, the concept of prime factors, the preparation theorem, remain to this day important features of the theory of complex functions of a single (and several) variable(s), not to mention that his $\wp$ and σ -functions provide the nowadays standard approach to elliptic functions.

As we document more fully in Chap. 10, parallel with the growing tendency towards an arithmetical rigour in analysis—the "arithmetisation of mathematics", as [Klein \(1895\)](#) called it—Weierstrass's arithmetical approach to analytic function theory reached the peak of its influence in the years 1890–1900, when it was also successful in the English-speaking world. In those years the German term *Funktionenlehre* and its English cognate "function theory" became almost synonymous with analytic function theory on Weierstrassian principles. But of course there were always other voices, even in Germany. In a polemical move intended to signal a shift away from Berlin and towards Göttingen, Klein chose to mark the 80th birthday of Weierstrass with an address that, we would say, came "to bury Caesar, not to praise

him”.¹⁵¹ While commending the “arithmetisation of mathematics” for its rigour, and urging that it be extended across mathematics, Klein argued that intuition was the source of creativity and discovery in mathematics, reminded his listeners of Riemann’s geometrical approach, firmly stressed that “mathematics will never be completed by logical deduction” and so that “even today *intuition* (*Anschauung*) also retains its full, specific significance” (Klein 1895, 234). The “Weierstrassian rigour” that Klein praised was also, he suggested, not the way to the future of the subject.

But if Klein’s remarks hint at a potential weakness in Weierstrass’s approach, or perhaps, and more fairly, at a legitimate concern that rigour in mathematics should proceed *with* intuition and not seek to replace it, it remains true that Weierstrass was throughout his long and productive life a creative mathematician. It is not given to many to introduce as many ideas, insights, and clarifications into mathematics as he did, and fewer still shape their contributions into a genuine, systematic theory that occupies a central place in the mathematics of its time. Weierstrass’s life’s-work has a grandeur to it, and even a hint of pathos. It is markedly eccentric in its disdain for the integral, however plausible that was in the context of his great hope for a complex function theory in several variables, but its foundations in the function theory of a single variable stand to this day.

¹⁵¹Weierstrass’s birthday fell on 31 October, Klein’s address was given on 2 November.

Chapter 7

Complex Function Theory and Differential Equations

Nonetheless, every time I can I seek absolute rigour, and that for two reasons; in the first place it is always hard for a geometer to tackle a problem without solving it completely; in the second place the equations that I would study have applications not only in physics, but also in analysis. It is on this possibility for the Dirichlet problem that Riemann founded his magnificent theory of abelian functions.

Poincaré

7.1 Introduction

The second half of the nineteenth century saw a growing systematic recognition of the importance of complex function *theory*. The recognition that there was a coherent body of ideas was enhanced by the discovery that it could be fruitfully adapted to other domains of mathematics, and this in turn did much to strengthen mathematicians' attraction to complex analysis. In this chapter we consider the impact of complex function theory on the theories of ordinary and partial differential equations and topics in such areas as potential theory and mechanics.

The influence of Riemann can be felt in various ways in these developments. His account of the hypergeometric equation ([Riemann 1857a](#)) was the first treatment of a differential equation from a thoroughly complex point of view (see Sect. [5.8.1](#) above). It directly influenced the work of Fuchs in 1866, as we shall shortly describe, and was surely known to C.A. Neumann when he took up the study of the spherical functions in 1867. And, as is well known, Riemann's provocative use of Dirichlet's principle, and its refutation by Prym and Schwarz (discussed above, see Sect. [5.11.1](#)) stirred up attempts to solve the Dirichlet problem and thus rescue the theory of harmonic functions.

Specifically, we consider:

1. The special functions.

2. The theory of linear ordinary differential equations in the complex domain.
3. Stokes' phenomenon and the first complex oscillation theorems.
4. Potential theory in simply connected domains.
5. Minimal surfaces.
6. Conformal transformations and conformal structures.
7. The later history of the motion of the top.
8. The three-body problem, and Poincaré's theorem on complex partial differential equations.

The range of topics that complex analysis transformed is large. Some have already been mentioned, for example Kepler's problem and the reversion of series (in Sect. 3.5), Poncelet's theorem (Sect. 1.4), Dirichlet's work in number theory (Sect. 4.7), and the Riemann zeta function (Sect. 5.10). The way that the theory of modular functions led to the discovery of real functions which are continuous everywhere but differentiable nowhere was discussed above (in Sect. 6.5.5). The study of cubic curves was likewise transformed by Clebsch's parameterisation of them by elliptic functions (see Sect. 5.11.3) and the idea that algebraic curves may intersect in complex points was also mentioned in our analysis of the reception of Riemann's work (Sect. 5.11.6). There are complex algebraic integers in Gauss's number-theoretical work (see Sect. 4.7.2), and complex function theory entered the discussion of quadratic reciprocity and Gauss sums (Sect. 4.7.1). After Legendre had shown how real elliptic integrals could be used in a number of problems in mechanics, such as the true motion of a pendulum and the study of orbits under the action of a central force, Jacobi was the first to show how complex elliptic functions could be used to investigate the motion of a top (see Sect. 4.6); he was followed by Hermite and a number of others. The theory of the solution of algebraic equations, culminating in Jordan's observation that the division theory of elliptic functions provides the solution of every algebraic equation, will be discussed in the next chapter (see Sect. 8.2.1).

In fact, a look at the slowly changing list of subdivisions of mathematics used by the editors of the *Jahrbuch* suggests that most major areas of mathematics were affected by the advent of complex function theory. Topics that were deliberately kept elementary were not, but even the trade in problem solving was not immune. "It is amusing to note", Weil (1974, 293) once observed, that the determination of the functional equation of the L -series

$$L(s) = 1 - \frac{1}{3^s} + \frac{1}{5^s} - \frac{1}{7^s} + \dots$$

was proposed as an exercise by Schlömilch in his *Archiv*, and Thomas Clausen, an astronomer at Dorpat observatory, solved it in 1858 by using Cauchy's calculus of residues. Weil's comment "Clearly they all regarded such matters as routine" shows just how successfully complex function theory was advancing.

So successful was this advance that it even can be argued that the theory succeeded too well; later generations had to prise the study of differentiable, and indeed merely continuous, phenomena from under a thick coating of analytic

methods. Since our aim is to establish that complex function theory came to play a central role, we have not attempted to give a complete description of the ways in which complex analysis entered mathematics but have contented ourselves with a range of examples. Nor is any topic dealt with exhaustively. In some cases there are book length treatments of what we have surveyed in a few pages; in others, we are aware of work in progress. There is much more to be said about many of these topics, but extensive treatments belong in another place; readers are invited to take up the task.

We turn now to the two important themes in the theory of ordinary linear differential equations that dominated research in the first half of the nineteenth century and were to deepen and eventually come together in the complex domain: special functions and the hypergeometric equation.

7.2 The Special Functions

The so-called special functions of mathematical physics are functions which arose in the eighteenth and nineteenth centuries in the study of physical problems, such as the oscillations of the hanging weighted chain, heat diffusion, and gravitational attraction.¹ They include many classes of functions with specific names (Bessel functions, spherical harmonics, the Gamma function, Lamé functions, and cylinder functions) while by convention the trigonometric and elliptic functions are excluded from their number. Nonetheless, the interaction between the special functions and these other distinguished classes of function is numerous and deep, as are their links to certain important functions with more abstract origins, such as the Eulerian Beta functions. The theory of the special functions formed a domain in its own right throughout the nineteenth century; our concern is to investigate how the rise of complex function theory affected it, and how it stimulated the theory of analytic functions.²

Some of these interactions go back to the very origins of complex function theory. It is not just that d'Alembert's 1752 paper on hydrodynamics (see Sect. 2.2.2) presented functions of a complex variable and the Cauchy–Riemann equations, although the connections made there have proved lasting. More substantial use of the idea of a complex function of a complex variable came with the beginnings of the modern theory. The Gamma function is at least implicitly complex in Gauss's published memoir of 1812 on the hypergeometric series, which is itself explicitly

¹For a detailed historical account of this and many other subjects that we treat in this chapter, see the relevant chapters of [Burkhardt \(1908\)](#).

²Many interesting questions in the purely real theory of the special functions, in particular many ingenious integrals, are therefore not described; for example, the paths introduced by Hilbert in his proof that π is transcendental in ([Hilbert 1893](#)). For that matter, many clever moves in the truly complex theory have also been left undiscussed once the first phase of the development is over, around 1900.

defined for a complex variable (see Sect. 1.5.2). The origins of the method of stationary phase lie in Cauchy's notes to his memoir on waves in which, as we saw in Chap. 3, some of Cauchy's first ideas about complex functions were first published in 1827. Yet, as we shall describe below, the first deep appreciation of the value of treating the special functions as complex functions had to wait for the 1850s. Doubtless this is due in large part to the conceptual difficulties inherent in letting variables or parameters in physical problems become complex. If natural science seemed to offer no reason to pass beyond the scope of the real numbers, naturally functions would remain real-valued, and complex quantities enter only formally, perhaps as a trick to evaluate a difficult integral. A further reason for the delay would have been the absence of a worked-out theory of complex functions which could be either taught to advanced students or taken off the peg by scientists and readily put to use. These phases are well illustrated by the example of the Bessel equation.

7.2.1 Bessel's Equation

The Bessel equation can be written as

$$x^2 \frac{d^2 y}{dx^2} + x \frac{dy}{dx} + (x^2 - n^2) y = 0. \quad (7.1)$$

Following the lead of some mathematicians of the nineteenth century, many historians have observed that this equation and its solutions, the Bessel functions, are not well named.³ Honour for discovering the first example of a Bessel function probably lies with Jakob Bernoulli, who described it in 1703 in a letter to Leibniz.⁴ In 1733 Euler and Daniel Bernoulli discussed the problem of determining the shape of the hanging weighted chain together in the St Petersburg Academy before Bernoulli left to go to Basel; Bernoulli's papers on the subject (published in 1738 and 1740) then drew a response from Euler.⁵ The problem led Euler and Bernoulli to discover the Bessel function $J_0(x)$ and they showed that it had infinitely many

³See [Wagner \(1894\)](#) for a history of Bessel Functions from Fourier to 1858. It stops short of discussion complex function theory. Schlömilch and Lipschitz proposed the name Bessel functions in the late 1850s; Heine countered with Fourier–Bessel functions, but this never caught on. The notes and corrections in the second edition (1878–1881) of Heine's *Handbuch der Kugelfunktionen* (1861) are very informative historically and are probably the source of many later historical passages, such as those by [Watson \(1922\)](#).

⁴See [Leibniz \(1849–1863, 3, 75\)](#).

⁵See [Cannon and Dostrovsky \(1981, 7 and 69\)](#) who find amongst other riches in these papers of Euler's an integral representation for $J_n(x)$ usually attributed to Poisson and Lommel, see (Sect. 7.2.3) below. This had been already pointed out by [Watson \(1922, 24\)](#) who added that, compared to Euler's, "Poisson's forms" of the integrals "are more elegant, and his study of them is more systematic".

real zeros. Bernoulli then began, and Euler continued, the study of the functions $J_n(x)$. Euler published a paper (1762a) on the vibrating drum. For the problem of the circular drum, he expressed the wave equation in polar coordinates, and looked for a solution by the technique of separation of variables; this led him to Bessel's equation, for which he gave power series solutions.⁶ Euler found only one solution to the differential equation for each integer value of the parameter n ; his attempt to find a second linearly independent solution failed, but later, in 1774, he discovered a second solution in the special case when $n = 0$, eventually published in 1784 as (Euler 1774).

As we have seen in Chap. 3, by that time Bessel's functions had reappeared in the solution of an astronomical problem. In his paper on Kepler's problem Lagrange (1771) showed that the solution of Kepler's equation $t = x - c \sin x$ can be expanded in series as $x = t + \sum_{n=1}^{\infty} A_n \sin nt$ where the A_n can be expressed in modern notation as the Bessel functions $nA_n = 2J_n(nc)$. Analogously, he also showed that the radius vector r can be expanded in series as $\frac{r}{a} = 1 + \frac{1}{2}c^2 + \sum_{n=1}^{\infty} B_n \cos nt$ where a is the semi-major axis of the elliptical orbit of the planet, and $B_n = 2(c/n)J'_n(nc)$. Lagrange limited himself to giving the expressions of A_n and B_n for $n = 1, 2, 3$.

A generation later, and apparently without knowledge of Euler's work, Fourier encountered Bessel's equation in the course of his study of heat, by choosing cylindrical coordinates for the study of a problem in heat diffusion in an infinitely long circular cylinder. In their study of Fourier, which centres on his monograph of 1807, Grattan-Guinness and Ravetz summarise his achievements by saying that Fourier achieved "Nothing less than the basic theory of the "Bessel function" $J_0(x)$: its series and integral forms, the reality of its roots, orthogonality and the consequent capability of representing a function over an interval, and a form for the function of the second kind" (Grattan-Guinness and Ravetz 1972, 375–376).

As we also noted in Chap. 3, a remarkable study of the approximate value of A_n for large n was published by Carlini in 1818 (whose mistakes were corrected by Jacobi in 1850) while the corresponding approximation of B_n was given by Laplace (1828) in a supplement to the vol. 5 of his *Mécanique céleste* published in 1827. Laplace's work is worth mentioning here for he considered purely imaginary values of the variable c . As usual for him, he warned that in general this "passages from the real to the imaginary" required "a great circumspection" but in the present case, as c is indeterminate, such a passage may be used "without fear" (Laplace 1828, 489). As Watson commented dryly (1922, 7): "To anyone who is acquainted with the modern theory of asymptotic series, the fallacious character of such reasoning will be evident".

Bessel came to the equation that bears his name in 1824, in the course of a successful attempt to separate two ways in which one planet disturbs the orbit of

⁶See Lützen (1987).

another.⁷ One is by direct gravitational attraction, the other is by perturbing the motion of the sun. He expressed the effect of the motion of the sun by this series:

$$J_n(x) = \frac{x^n}{2^n \Gamma(n+1)} \left(1 - \sum_{k=1}^{\infty} \frac{(-1)^k x^{2k}}{2^{2k} k! (n+1)(n+2) \dots (n+k)} \right). \quad (7.2)$$

In his notation, the Bessel function of order h was defined as this function I_k^h of the real variable k :

$$2\pi I_k^h = \int_0^{2\pi} \cos(h\varepsilon - k \sin \varepsilon) d\varepsilon.$$

Straightforward manipulations showed that

$$kI_k^{i-1} - 2iI_k^i + kI_k^{i+1} = 0$$

and that

$$\frac{dI_k^i}{dk} = \frac{i}{k} I_k^i - I_k^{i+1}$$

from which the eponymous equation was readily derived. In Bessel's work these functions enter as coefficients in various power series, so the exponents i and h are naturally integers, while the variable k has astronomical significance, so it is naturally real.⁸ It is interesting to see how and when these restrictions ceased to apply.

7.2.2 Laplace's Equation and Legendre's Polynomials

Motivation came from the theory of spherical harmonics, which grew up in the rapidly developing field of potential theory. In 1785 Laplace (1785b, 363) had published this equation for the attraction due to a spheroid⁹:

$$\frac{\partial}{\partial \mu} \left((1 - \mu^2) \frac{\partial V}{\partial \mu} \right) + \frac{1}{1 - \mu^2} \frac{\partial^2 V}{\partial \phi^2} + r \frac{\partial^2 (rV)}{\partial r^2} = 0. \quad (7.3)$$

⁷The paper built on an earlier one, written in 1818 on Kepler's problem where Bessel re-obtained Lagrange's expansion of r/a in series. We cannot find that it supports Kline's (1972, 710) claim that in 1818 Bessel showed that $J_0(x)$ has infinitely many real zeroes.

⁸In the special case $h = 0$ Bessel's integral is commonly named after Parseval, for it had been obtained in 1806 by the latter in the memoir (Parseval 1806b) that we have considered in Chap. 2.

⁹The corresponding, more familiar equation in orthogonal coordinates $\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} + \frac{\partial^2 V}{\partial z^2} = 0$ was published by Laplace in (Laplace 1789, 278).

He expressed his solution in the form $V(r, \theta, \phi) = \sum_{n=1}^{\infty} \frac{U_n}{r^n}$, where $\mu = \cos \theta$ and the functions $U_n = U_n(\theta, \phi)$ satisfy the equation

$$\frac{\partial}{\partial \mu} \left((1 - \mu^2) \frac{\partial U}{\partial \mu} \right) + \frac{1}{1 - \mu^2} \frac{\partial^2 U}{\partial \phi^2} + n(n+1)U = 0. \quad (7.4)$$

[The functions U_n were later called spherical harmonics by Lord Kelvin in (Thomson and Tait 1879, 171)].

Without mentioning Legendre, Laplace expressed them as integrals involving the polynomials $P_n(x)$ defined as

$$\frac{(2n-1)(2n-3)\dots 1}{n!} \left(x^n - \frac{n(n-1)}{2(2n-1)} x^{n-2} + \frac{n(n-1)(n-2)(n-3)}{2 \cdot 4 \cdot (2n-1)(2n-3)} x^{n-4} - \dots \right) \quad (7.5)$$

that had recently been defined explicitly for even n by Legendre (1785) in the course of his investigation of the attraction due to a spheroid. In fact, the equation for U as a function of μ , holding ϕ constant, is

$$\frac{d}{d\mu} \left((1 - \mu^2) \frac{dU}{d\mu} \right) + n(n+1)U = 0, \quad (7.6)$$

nowadays called Legendre's differential equation, and which is satisfied by the Legendre polynomials. The Legendre polynomials P_n satisfy this form of the equation

$$(1 - z^2) \frac{d^2 U}{dz^2} - 2z \frac{dU}{dz} + n(n+1)u = 0. \quad (7.7)$$

The equation reduces to the hypergeometric equation on writing $z^2 = \eta$, see (Whittaker and Watson 1937, 304).

Laplace's researches on the attraction of spheroids overlapped with Legendre's, who was moved to assert his originality in the field in a second memoir read at the Académie on July 7, 1784 (and printed in 1787). He allowed that Laplace (1785b) had provided a much more scholarly and general proof of the same propositions that he himself was going to present, but he claimed priority over Laplace on the grounds that his own paper (Legendre 1785) was published before Laplace's, and asserted that it had provoked Laplace to deepen the subject and so to provide the geometers with a complete theory of these functions (Legendre 1787, 370).

Legendre (1787, 371) introduced the polynomials $P_{2n}(x)$ as the coefficients of t^n in the expansion of $(1 - 2xt + x^2)^{-1/2}$, and he proved (1787, 373–374) that for positive integers m and n they satisfy the orthogonality relationship:

$$\int_0^1 P_{2n}(t) P_{2m}(t) dt = \begin{cases} 0 & m \neq n \\ \frac{1}{4n+1} & m = n \end{cases}$$

Then he stated that $P_{2n}(x)$ can be split into the product of n factors like $(x^2 - \alpha_i^2)$, i.e. the $2n$ zeros of $P_{2n}(x)$ are real, different from each other, symmetric with respect to zero, and less than 1 in absolute value. He limited himself to proving this in the case $n = 2$. Then he proved that for $0 < x < 1$, $P_{2n}(x) < 1$.

In a subsequent paper presented to the Académie in 1789 but published in 1793¹⁰ Legendre proved that the given expression (7.5) for $P_n(x)$ also holds for odd n , thus establishing it for any positive integer. He also proved a nice orthogonality relationship for his polynomials:

$$\int_{-1}^1 P_n(t)P_m(t)dt = \begin{cases} 0 & m \neq n \\ \frac{2}{2n+1} & m = n \end{cases}$$

reminiscent of the sine and cosine functions, so that they were soon used to express a wide class of functions as series in the P_n 's, analogous to the expansion of an "arbitrary" function as a Fourier series.

Legendre collected his results in Sect. 5, § 10 of the *Exercices de calcul intégral* (Legendre 1811–1817). Following this, in 1816 Rodrigues gave the compact formula for Legendre's polynomials:

$$P_n(x) = \frac{1}{2^n n!} \frac{d^n(x^2 - 1)^n}{dx^n}. \quad (7.8)$$

The relevant section of Legendre's *Exercices* caught Jacobi's attention who, full of youthful enthusiasm, and apparently unaware of Rodrigues's work, boldly stated (Jacobi 1827b, 22) that "Legendre has missed the fundamental property" of the polynomials $P_n(x)$. This property was given by the same equation (7.8) that Jacobi obtained directly as the coefficient of z^n in the expansion in Lagrange series of $y - x = zF(y)$, where $F(y) = \frac{1}{2}(y^2 - 1)$. To this he added the following "remarkable relation"

$$\frac{1}{(n-r)!} \frac{d^{(n-r)}(x^2 - 1)^n}{dx^{(n-r)}} = \frac{(x^2 - 1)^r}{(n+r)!} \frac{d^{(n+r)}(x^2 - 1)^n}{dx^{(n+r)}} \quad (r \leq n)$$

that he obtained with a skillful application of Taylor series.

Complex variables entered the subject in Livre XI, Chap. 2 of Laplace's *Mécanique céleste*, devoted to the shape of Earth. There¹¹ Laplace showed that the polynomials $P_n(z)$, when $|\arg z| < \pi/2$, can be expressed by the integral (*Laplace's first integral*)

$$P_n(z) = \frac{1}{\pi} \int_0^\pi \left(z + (z^2 - 1)^{1/2} \cos \varphi \right)^n d\varphi. \quad (7.9)$$

¹⁰See *Hist. Acad. Sci. Paris* 1789, 372–454.

¹¹See his *Oeuvres* 5, 40–41.

Inspired by Laplace's "deep researches", in 1843 Jacobi published a paper where, under the same hypotheses as Laplace's, he showed that the polynomials $P_n(z)$ could be represented by the integral (*Laplace's second integral*)

$$P_n(z) = \frac{1}{\pi} \int_0^\pi \frac{d\phi}{(z + (z^2 - 1)^{1/2} \cos \phi)^{n+1}}. \quad (7.10)$$

An integral formula for Legendre polynomials was eventually obtained in 1881 by Schläfli, who combined Rodrigues's formula (7.8) with Cauchy's integral formula for the derivatives of an analytic function to get the expression

$$P_n(z) = \frac{1}{2\pi i} \int_C \frac{(t^2 - 1)^n}{2^n (t - z)^{n+1}} dt,$$

where C is a contour encircling the point z . This expression satisfies the particular form of the hypergeometric equation given above even when n is not a positive integer provided that the contour C is such that $(t^2 - 1)^{n+1}(t - z)^{-n-2}$ takes its original value after describing C .

However, prior to Schläfli, C.A. Neumann had had the idea of applying Cauchy integral formula to the solutions of the equation (7.7). Indeed, Schläfli's 1881 booklet was explicitly inspired by Heine's *Handbuch der Kugelfunctionen*, as was C. A. Neumann's work to which we now turn.

7.2.3 Spherical and Bessel Functions

Spherical functions were given an influential treatment in Heine's *Handbuch der Kugelfunctionen* of 1861, where they were also intermittently regarded as complex functions. Inspired by the formula

$$\frac{1}{z - z_1} = \sum_{n=0}^{\infty} (2n+1) P_n(z_1) Q_n(z)$$

that he found on p. 39 (see also p. 104) of that book,¹² C.A. Neumann (1867) set out to generalise Cauchy's derivation of the Taylor series expansion of a function on a disc to expansions in terms of functions $P_n(z)$ and $Q_n(z)$ that are a basis of solutions for the differential equation (7.7) that he rewrote as

$$\frac{d^2 F}{dz^2} - \frac{2z}{1-z^2} \frac{dF}{dz} + \frac{n(n+1)}{1-z^2} F = 0.$$

¹²The functions $Q_n(z)$ that Heine called spherical functions of the second kind are commonly called Legendre functions of the second kind. Heine (1861, 41) pointed out that they were first introduced by Gauss in 1816 under the form of hypergeometric series, see Gauss, *Werke* 3, 188–189.

Specifically,

$$P_n(z) = \frac{1}{\pi} \int_0^\pi \frac{d\omega}{(z + (z^2 - 1)^{1/2} \cos \omega)^{n+1}}$$

and

$$Q_n(z) = \frac{1}{\pi} \int_0^\infty \frac{d\omega}{(z + (z^2 - 1)^{1/2} \cos i\omega)^{n+1}},$$

where the sign of the square root is chosen so that $|z + (z^2 - 1)^{1/2}| > 1$.

Central to Cauchy's approach, Neumann argued in the opening pages, was the Cauchy integral theorem

$$2\pi i f(z_1) = \int \frac{f(z) dz}{z - z_1},$$

where the integral is taken positively around any circle containing the point z_1 . He then stated the orthogonality results:

$$\int P_m(z) Q_n(z) dz = \begin{cases} 0 & m \neq n \\ \frac{2\pi i}{2n+1} & m = n \end{cases}$$

so

$$\int P_m(z) P_n(z) dz = 0 = \int Q_m(z) Q_n(z) dz,$$

for all m and $n, m \neq n$, where the integrals are taken positively around an ellipse with foci at ± 1 , and he stated them again in his (1867, 3), where he added that the integral can be taken “any other closed curve into which the ellipse can be deformed by stretching and bending without coming into contact with the segment $-1 \dots +1$ ”. He then deduced, by an appeal to the Cauchy integral theorem, that functions defined inside such an ellipse have expansions in terms of the functions $P_n(z)$ and functions defined in the annular region between two such an ellipses have expansions in terms of the functions $P_n(z)$ and $Q_n(z)$.

In 1848 F. Neumann (1848, 24) had generalised these ideas to the complex setting, by means of the integral $Q_n(z) = \frac{1}{2} \int_{-1}^1 \frac{P_n(t)}{z-t} dt$, defined on the complex plane cut along the interval $(-1, 1)$. Functions analytic inside an ellipse with foci at -1 and 1 are expressible as a series of these complex Legendre functions (called Legendre–Fourier coefficients).¹³

C. Neumann's short pamphlet (1862), and still more his book (1867), marked one advance over Heine's successful book of 1861 on the spherical functions. Whereas in Heine's treatment the functions are only formally made complex, in Neumann's treatment they are handled in a way that takes good account of Cauchy's theory of complex functions. These “Recollections of spherical functions”, as Neumann called them, introduced the proper treatment of Bessel functions that constituted

¹³See the 2nd ed. of Heine's *Handbuch*, vol. 1, 141–143. See also (F. Neumann 1878).

the core of Neumann's book. The same care that he had taken with the spherical functions now distinguished his complexification of the Bessel functions. Once again he sought two basic families of functions to use as the basic terms of infinite series expansions. By analogy with the spherical functions he considered Bessel functions of the first kind, $J_n(z)$, and of the second kind $O_n(z)$ (satisfying not Bessel's equation but another differential equation) produced for the occasion, and nowadays commonly called Neumann's polynomials.¹⁴ By analogy with the functions $P_m(z)$ and $Q_n(z)$, he established for the functions $O_n(z)$ orthogonality properties such as

$$\begin{aligned}\int_C O_m(z)O_n(z)dz &= 0 \quad (m \neq n) \\ \int_C J_m(z)O_n(z)dz &= 0 \quad (m^2 \neq n^2),\end{aligned}$$

C being any closed contour not through the origin. He then showed how this new class of functions was also capable of yielding series expansions like the trigonometric and spherical ones.

A fuller treatment of Bessel's equation, which was also valid for fractional values of the parameter n was provided the next year in the book by Eugen Lommel (1868). He explicitly avoided Neumann's analogy with spherical functions, and instead defined a Bessel function of argument z and order ν as the integral

$$J_\nu(z) = \frac{1}{\sqrt{\pi}} \frac{z^\nu}{2^\nu \Gamma(\nu + 1/2)} \int_0^\pi \cos(z \cos \omega) \sin^{2\nu} \omega d\omega$$

thus generalising an integral investigated by Poisson (1823a, 300 and *passim*) in a memoir on the conduction of heat in solid bodies. Lommel considered only real values of ν . For negative values of ν the integral is convergent for $\nu > -1/2$, and so he defined functions of order less than $-1/2$ by means of a suitable recurrence formula (Lommel 1868, 9–10).

In some ways his task was actually easier, because for non-integral ν a second linearly independent solution of the Bessel differential equation is $J_{-\nu}(z)$, while for integral ν this is not so (if n is an integer $J_{-n}(z) = (-1)^n J_n(z)$). We cannot find that Lommel gave two linearly independent solutions for integral n ; the second solution found by Neumann (1867), that he called Bessel's associated function, was supplanted by another one due to Hankel (1869) and commonly adopted: $Y_n(z)$, called a Bessel function of the second kind (of order n). Hankel defined it as

$$Y_n(z) = \lim_{\nu \rightarrow n} \left[\frac{J_\nu(z) - (-1)^n J_{-\nu}(z)}{\nu - n} \right].$$

¹⁴They are polynomials in negative powers of z . For an account of their properties, see Watson (1922, 271–280).

and gave its expansion in ascending powers of z . (Believing that this result by Hankel was poorly known, Bôcher derived it in full detail in his (1892)). In the same paper [Hankel \(1869, 473–485\)](#) extended Lommel's integral representation of $J_\nu(z)$ to complex values of ν , and investigated its properties. Without mentioning Hankel's work, Lommel returned to his theme in two papers bearing the same title, and published in early issues of the *Mathematische Annalen* ([Lommel 1871](#)). In particular, in the first of these he unified his treatment, which had been based on three equations, around one, which may, he pointed out, now have a complex parameter.¹⁵

7.2.4 The Gamma Function

Explicit recognition that the Gamma function could be complex seems not to have been a tidy process. In his papers (1812a,b) Gauss equivocated over the nature of the argument of the Gamma function Γ ; it is hard to see that he anywhere insisted that it be a function of a complex argument. It tended to be a function of the α , β , and γ , and Gauss nowhere stipulated that they be real or suggested that they could be complex. Credit for opening up the complex theory is therefore usually given to [Schlömlich \(1844\)](#) and [Newman \(1848\)](#). Then for the first issue of the *Quarterly Journal of Mathematics* in 1857 Enneper wrote on Γ as a function of a complex variable.

But by then the story had restarted with Weierstrass's paper (1856a), in which he gave a new definition of Γ and showed how to treat it as a complex function starting from its functional equation. Analyses of the complex Gamma function slowly followed Weierstrass's pioneering investigations. In 1864 Hankel obtained an integral representation for Γ involving a contour integral in the complex plane¹⁶, and in 1886 Hölder showed that Γ satisfies no algebraic differential equation.

He had been led to try to obtain this result, he said, after he heard that in a conversation Weierstrass had put it as an exercise, and in his paper ([Hölder 1886](#)) he argued as follows. He began with the function $\varphi(x) = \frac{\Gamma'(x)}{\Gamma(x)}$, which satisfies the functional equation $\varphi(x+1) = \frac{1}{x} + \varphi(x)$. He then supposed, for a contradiction, that φ satisfies an algebraic differential equation of degree m and used the property of φ just established to show that it then satisfies an algebraic differential equation of degree $m-1$. By iteration, this shows that φ satisfies no algebraic differential equation. Then he turned to the function Γ and showed that if it satisfied an algebraic differential equation of degree m , then there was an algebraic differential equation of degree m involving only Γ , φ and derivatives of φ but not of Γ . Because there could be no algebraic equation in the derivatives of φ he treated these derivatives as

¹⁵For a detailed, technical account, rich in historical references, see ([Watson 1922](#), Chap. III).

¹⁶An integral formula substantially equivalent to it was given in 1812 by [Laplace \(1812a, 135\)](#).

arbitrary, independent coefficients in the differential equation for Γ . He finished his proof by invoking Kronecker's theory of irreducibility in a domain of rationality and the equality of Γ' and $\Gamma\phi$ to show that no algebraic differential equation satisfied by Γ could be irreducible, thus concluding the proof.

7.3 The Hypergeometric Equation

We turn now to the most important ordinary differential equation in our account: the hypergeometric equation. The major stimulus for work on the hypergeometric equation in the 1860s and 1870s was undoubtedly Riemann's paper on it (1857a), which we discussed above in Sect. 5.8.1, which led directly to the investigations by Fuchs and Schwarz that we now discuss. Further ramifications of the study of this equation eventually led Klein to create a theory that organised the special functions within a framework provided by complex analysis.

7.3.1 *Fuchs's Theory of Linear Differential Equations*

Lazarus Fuchs's career may be said to have begun with three papers he published in the years 1865, 1866, and 1868 each entitled “Zur Theorie der linearen Differentialgleichungen mit veränderlichen Coefficienten” (“On the theory of linear differential equations with variable coefficients”).¹⁷ In these papers he characterised those linear differential equations in the complex domain none of whose solutions have essential singularities. This class contains many interesting equations, notably the hypergeometric equation. In the course of this work Fuchs created much of the elementary theory of linear differential equations in the complex domain: the analysis of singular points; the nature of a basis of n linearly independent solutions to an equation of degree n when there are repeated roots of the indicial equation; explicit forms for the solution according to the method of undetermined coefficients.

Fuchs was born in Moschin near Posen in 1833 and went to Berlin University in 1854 where he started his graduate studies under Kummer, writing his dissertation in differential geometry (1858), and then papers in algebraic number theory. He was the first to present his thesis to the new Berlin University under Kummer's supervision, and his friend Koenigsberger was the first to present one under Weierstrass's (formally, it was also under Kummer's supervision). Fuchs seems to have gravitated to what became his lifelong interest in differential equations only after Weierstrass lectured on Abelian functions in 1863. When Fuchs presented

¹⁷More information on Fuchs's work will be found in [Ince \(1926\)](#), and [Gray \(1984a\)](#) and [Gray \(2000a\)](#).

his Habilitationsschrift in 1865 Kummer was the principal referee and Weierstrass the second, but by 1870 Fuchs spoke to himself as “a pupil of Weierstrass”.¹⁸ In 1868 Fuchs went to Greifswald, and he returned to Berlin in 1884 as professor Ordinarius, occupying the chair vacated by Kummer. His friend Koenigsberger described him as irresolute and anxious, a temporizer and one easily persuaded by others, but humorous and unselfish. He was to play a weak role in the battle between Weierstrass and Kronecker that polarized Berlin in the 1880s, which disappointed Weierstrass, and was no match for Frobenius in the 1890s, but several of his students, notably Schlesinger, spoke warmly of him.

Fuchs’s (1866) is the most important of his papers. It was published in Crelle’s *Journal* and it was widely read. He began it by stating that the task facing mathematicians was not so much to solve a given differential equation by means of quadratures, but rather to develop solutions defined at all points of the plane. This led him to look for branch points and singular points of the solutions, an approach that had been carried out, he observed, by Briot and Bouquet in their paper (1856b) and by Riemann in his 1857 lectures on Abelian functions. Fuchs proposed to investigate the general homogeneous linear ordinary differential equation, which can be written as

$$\frac{d^n y}{dx^n} + p_1 \frac{d^{n-1} y}{dx^{n-1}} + \dots + p_{n-1} \frac{dy}{dx} + p_n y = 0, \quad (7.11)$$

where the coefficients $p_i(x)$ are single-valued meromorphic functions of x in the entire complex x plane, or on some simply connected region T . Fuchs called the points in T where one or more of the $p_i(x)$ were discontinuous singular points, a term he attributed to Weierstrass, and he assumed each p_i had only finitely many singular points.

Fuchs first showed that a solution of the differential equation in a neighbourhood of a non-singular point x_0 is a single-valued, finite, continuous function which is prescribed once values for $y, \frac{dy}{dx}, \dots, \frac{d^{n-1}y}{dx^{n-1}}$ are given at the point x_0 . He showed that this means that the singular points of the solution must lie among the singular points of the coefficients $p_i(x)$. They are therefore fixed and determined by the equation. This contrasts markedly with the case of non-linear differential equations, where the singular points may depend on arbitrary constants involved in the solutions.

Then he considered solutions in the neighbourhood of a singular point, and asked: what conditions must be placed on the coefficients so that all the solutions are regular, i.e. have only finite poles at the singular points of the coefficients $p_i(x)$, and are at worst logarithmic upon multiplication by a suitable power of $(x - x_{ij})$ or $\frac{1}{x}$, where x_{ij} is a singular point of the coefficient function $p_i(x)$? His investigation of the behaviour of the solutions in the neighbourhood of a singular point was modelled on Riemann’s: he considered the effect of analytically continuing the

¹⁸Letter to Casorati, quoted in (Neuenschwander 1978b, 46).

solutions around the point—the monodromy relations. Indeed, like Riemann, he did not explicitly regard the transformations so obtained as forming a group. His solution to this question (1866, Sect. 4) produced a class of differential equations readily characterised by the purely algebraic restrictions upon the coefficients; this class has become known as the Fuchsian class, and such equations will henceforth be referred to as “equations of the Fuchsian class”. Fuchs himself always preferred more modest circumlocutions.

In particular, Fuchs showed that the most general equation of the Fuchsian class and the second order has the form

$$\frac{d^2y}{dx^2} + \frac{F_{\rho-1}(x)}{\psi} \frac{dy}{dx} + \frac{F_{2(\rho-1)}(x)}{\psi^2} y = 0,$$

where $\psi = (x - a_1)(x - a_2) \dots (x - a_\rho)$ and F_s is a polynomial in x of degree at most s . This is Fuchs’s main theorem, but as it applies to second order equations—Fuchs dealt with the n th order case throughout.

As Fuchs observed (1866, Sect. 6) the hypergeometric equation is of the Fuchsian class. It appears in the above form with $\psi = (x - a_1)(x - a_2)$, $F_1 = f_0 + f_1x$, $F_2 = g_0 + g_1x + g_2x^2$.

Fuchs also gave a significant characterisation of the hypergeometric equation. First order equations aside, the only equation of the Fuchsian type for which the exponents at the singular points determine the coefficients of the equation is the hypergeometric equation. This explains why Riemann’s methods worked so well for the hypergeometric equation, for it is precisely in that case that his initial data (the exponents) characterise not only the solution functions but also the equation itself. In all other cases, the number of exponents is too few to characterise the equation, and the excess numbers, called the auxiliary parameters, have since proved quite intractable.

It may help if we briefly illustrate Fuchs work with the case of a second order differential equation that obeys Fuchs’s conditions, and which we write in the form

$$\frac{d^2y}{dx^2} + p_1 \frac{dy}{dx} + p_2 y = 0.$$

We look for a basis of solutions in the form

$$x^\alpha \sum_j c_j (x - x_0)^j, \text{ and } x^\beta \sum_j d_j (x - x_0)^j,$$

where x_0 is a non-singular point of the coefficients of the equation. Our approach is to substitute either of these into the differential equation and equate to zero the lowest power of $x - x_0$, so we write

$$p_1(x - x_0) = a_{-k}(x - x_0)^{-k} + \text{higher terms, and } p_2(x) = b_{-m}(x - x_0)^{-m} + \text{higher terms.}$$

We obtain these contributions to the equation for the lowest power of x in the solution:

- $\alpha(\alpha - 1)$ of degree $\alpha - 2$ from the first term of the differential equation.
- $a_{-k}\alpha$ of degree $\alpha - 1 - k$ from the second term.
- b_{-m} of degree $\alpha - m$ from the third term.

For this approach to work these contributions must collectively depend on α , so we see that the coefficient p_1 can have at most a pole of order 1, because $1 + k \leq 2$, and similarly q_1 can have at most a pole of order 2, because $m \leq 2$. These conditions are exactly what Fuchs found in the general case.

Now, the exponents α and β can be found from the so-called indicial equation

$$\alpha(\alpha - 1) + a_{-1}\alpha + b_{-2},$$

which is a quadratic equation in α . In the simplest case α and β are distinct and do not differ by an integer. In this case, analytic continuation of a solution around a singular point returns $x^\alpha \sum_j c_j (x - x_0)^j$ in the form $e^{2\pi i \alpha} x^\alpha \sum_j c_j (x - x_0)^j$, and it returns $x^\beta \sum_j d_j (x - x_0)^j$ in the form $e^{2\pi i \beta} x^\beta \sum_j d_j (x - x_0)^j$. This means that the corresponding monodromy matrix that relates the basis of solutions before and after analytic continuation is $\begin{pmatrix} e^{2\pi i \alpha} & 0 \\ 0 & e^{2\pi i \beta} \end{pmatrix}$. It is in diagonal form, and the diagonal entries—the eigenvalues—are obtained from the roots of the indicial equation by exponentiation. The situation is more complicated if the indicial equation has roots that differ by an integer or which are the same, and these correspond to the other cases of possible Jordan canonical form: $\begin{pmatrix} \lambda & 1 \\ 0 & \lambda \end{pmatrix}$ and $\begin{pmatrix} \lambda & 0 \\ 0 & \lambda \end{pmatrix}$, respectively.

The details for the n th order case were worked out completely by Fuchs in his (1866, 1868), using the indicial equation. In 1871 Jordan showed how to express the corresponding matrix algebra in terms of his eponymous theory of the reduction of matrices to canonical form, and in 1873 Hamburger did the same using in Weierstrass's theory of the reduction of forms.

7.3.2 Schwarz's Algebraic Solutions

On 22nd August 1871, at a meeting of the mathematical section of the Swiss Naturforschenden Gesellschaft, Schwarz announced the solution to the problem: when does the Gaussian hypergeometric series represent an algebraic function? (See (Schwarz 1872b).) Schwarz considered the function as a solution of the hypergeometric equation, which he wrote in the form

$$\frac{d^2 y}{dx^2} + p \frac{dy}{dx} + q = 0, \quad (7.12)$$

where p and q are suitable polynomials in x and considered the behaviour of the function under analytic continuation.

Two cases arise. It can be that the equation has only one algebraic solution, or that it has two linearly independent algebraic solutions. Schwarz treated both cases, but the first one is less interesting, and we pass immediately to the second. In this case, every solution is algebraic, and so therefore is the quotient of any two solutions, say y_1 and y_2 , and the analytic continuation of any quotient. Combining a result of Abel's¹⁹ with Kummer's analysis of the 24 solutions of the hypergeometric equation (see Sect. 5.8.1), Schwarz reduced the problem to a consideration of these quotients, which are of the form $\frac{c_1 y_1 + c_2 y_2}{c_3 y_1 + c_4 y_2}$, where $c_1, \dots, c_4$ are constants. These quotients all satisfy a differential equation obtained by eliminating the three ratios $c_1 : c_2 : c_3 : c_4$ by successive differentiation:

$$\Psi(s, x) = \frac{2 \frac{ds}{dx} \cdot \frac{d^3 s}{dx^3} - 3 \left(\frac{d^2 s}{dx^2} \right)^2}{2 \left(\frac{ds}{dx} \right)^2} = 2p - \frac{1}{2}p^2 - \frac{dp}{dx}. \quad (7.13)$$

Cayley (1881) called this the Schwarzian of s with respect to x or the Schwarzian derivative, and the name has stuck. Schwarz regarded it as a special case of an equation in (Kummer 1834), and he wrote it out explicitly in terms of the parameters entering the hypergeometric equation. The advantage of (7.13) over (7.12) is that, as Heine pointed out to Schwarz,²⁰ if $\frac{y_1}{y_2}$ and $e^{-\int p dx}$ are both algebraic then y_1 and y_2 are algebraic, because $\frac{d}{dx} \left(\frac{y_1}{y_2} \right) = C y_2^{-2} e^{\int p dx}$. So Schwarz needed to consider the algebraic nature only of the quotient, $\frac{y_1}{y_2}$, and not the two functions in a basis of solutions for the hypergeometric equation.

Schwarz now proceeded by combining the results of Kummer and Riemann on the hypergeometric equation, which gave explicit forms for the hypergeometric series under various changes of the variable,²¹ and explained them in terms of the analytic continuation of the corresponding function around the singular points of the hypergeometric equation, which are $x = 0, 1, \infty$.

To solve (7.13), Schwarz first considered the solutions near a non-singular point, x_0 . Standard power-series methods, together with Kummer's solutions to (7.13) enabled him to establish this theorem: The map s from the complex x -plane to the complex s -plane maps each simply connected region X not containing $0, 1$ or ∞ onto a simply connected region S containing ∞ once or several times in its interior but having no branch point in its interior (see Schwarz 1872b, 225). It follows that away from the singular points the function s is real when x is real, and S is marked out by circular arcs.

¹⁹Abel derived differential equations, notably Legendre's, for functions defined by definite integrals in his (1827).

²⁰See Schwarz (1872b, 218).

²¹Schwarz now assumed that the coefficients of the corresponding hypergeometric function $F(\alpha, \beta, \gamma)$ are real.

Next, he considered the solutions to (7.13) in the neighbourhood of the singular points $x = 0, 1, \infty$ and showed that the upper-half x -plane is mapped conformally by s , a particular integral of (7.13), onto a simply connected domain S , having no winding point in its interior, which is, in general, a circular arc triangle. The angles at the vertices corresponding to $0, 1, \infty$ are $\lambda\pi, \mu\pi$, and $\nu\pi$, respectively, where

$$\lambda^2 = (1 - \gamma)^2, \mu^2 = (\alpha - \beta)^2, \nu^2 = (\gamma - \alpha - \beta)^2.$$

Schwarz then investigated how these circular-arc triangles are connected. He found in the general situation that corresponding points in the upper and lower half planes are mapped onto reciprocal points in the images (S and S' , say), that is, to points which are images under the Möbius transformation of inversion in the common side.²² Since the solutions of the hypergeometric equation are assumed to be algebraic, the question reduces to finding all circular-arc triangles which fit together to give a finite covering of the sphere. Schwarz observed that this problem had already been discussed to some extent by Riemann himself, in a paper published just after his death (Riemann 1867) where, in Sect. 12, Riemann considered the case where the solutions are algebraic, and in Sect. 18 where he alluded to the conformal representations of regular solids onto the sphere.

Schwarz now deduced that the solution of his problem is precisely that the triangles must either fit together to form a regular double pyramid or a regular solid, and he spelled out the various choices of parameters for which this can happen. In each case he showed that it was possible to write down the associated Gaussian hypergeometric series and exhibit it directly as an algebraic function, and he discussed explicitly one of the cases where the regular solid is a tetrahedron, divided by its symmetry planes into 24 triangles. He also discussed the cases of the octahedron, icosahedron, and dodecahedron in slightly less detail. His analysis was extended in (Brioschi 1877a,b,c) and completed in (Klein 1877).

Schwarz did not confine himself to the problem of finding those hypergeometric equations all of solutions are algebraic. He found elegant connections with elliptic function theory in the tetrahedral case, and, in an exploration whose full significance he did not appreciate, he opened up the transcendental case too. In Sect. 5 of his paper he showed that a circular-arc triangle with angles $\frac{\pi}{5}, \frac{\pi}{4}, \frac{\pi}{2}$, can be formed with sides perpendicular to a fixed boundary circle, and that successive inversions in these boundaries fill out the interior of this fixed circle with copies of the original triangle. In this case, he showed that the quotient function s is necessarily transcendental and single valued whenever the reciprocals of λ, μ, ν are integers, in which case the fixed circle is a natural boundary of s , beyond which s cannot be analytically continued. But there the matter was to rest until 1880 when Poincaré rediscovered such tessellations of the disc and saw the connection to non-Euclidean geometry, with the implications for the study of complex function theory that are described in Sect. 8.2.4 (Fig. 7.1).

²²Schwarz also dealt with the special cases where λ, μ, ν may be integers.

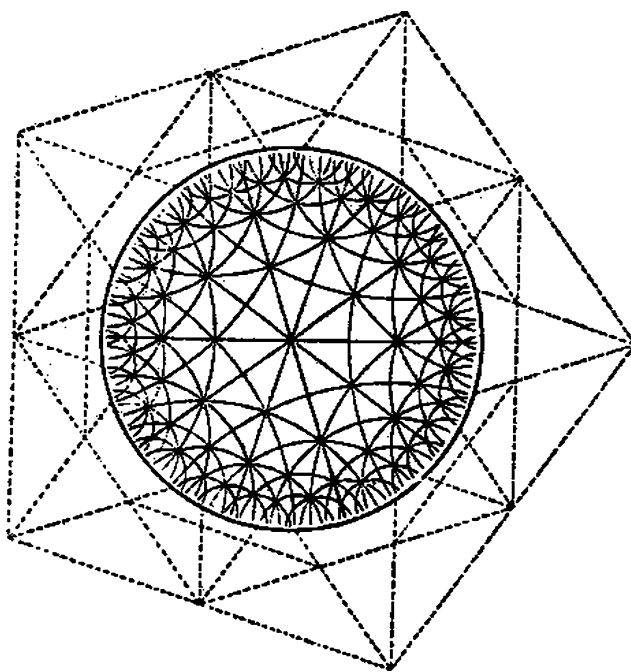


Fig. 7.1 Schwarz's tessellation of the disc (Schwarz, *Gesammelte Mathematische Abhandlungen* vol. 2, p. 240)

The topic of natural boundaries was of much interest to function theorists in the 1860s and 1870s, Hermite among them. In his (Hermite 1862) he expressed the functions $\Theta(x)$, $H(x)$, $\Theta_1(x)$, and $H_1(x)$ as power series in $q = e^{i\pi\omega}$, and recalled that their modulus k and its complement k' can be expressed as $k = H^2(K)/\Theta^2(K)$ and $k' = \Theta^2(0)/\Theta^2(K)$. Now he observed that k and k' , considered as functions of ω , represented “a completely new kind of analytic function of the highest importance among the functions of one variable” (Hermite 1862, 163), adding that “in the current status of our knowledge in analysis it does not seem to be possible to obtain all of their properties by starting uniquely from their definition as a ratio of the series given above”. To illustrate this difficulty Hermite remarked that k and k' “exist” as functions of $\omega = \alpha + i\beta$ as far as $\beta \neq 0$ and $\beta > 0$. Thus, Hermite (1862, 164) concluded “they are actually parts of functions that, consequently, escape many of the more commonly used methods”.

Some years later, on 1 July 1876, he wrote to his friend Fuchs about the disparate behaviour of elliptic integrals of the first and second kinds:²³

²³An excerpt of the letter Hermite sent to Fuchs was added by Schlesinger to the reprint of (Fuchs 1877) in vol. 2 of his *Ges. Math. Werke*. See Fuchs (1877, 113).

You should without doubt be able to show, by means of the principles at your command, that on setting $\frac{K}{K'} = \omega$ and $k = f(\omega)$, k is a single-valued function of $\omega = x + yi$ for all positive x , but what I cannot work out, and it interests me very much, is how to see clearly that on setting $\frac{J}{J'} = x + yi$ one ceases to have a single-valued function. Your methods, I don't doubt, should immediately give the reason for the difference in nature of the functions defined by the two equations.

Fuchs replied in November 1876.²⁴ His approach was to study K and K' as solutions of the differential equation (Legendre's equation) that they satisfy and to use the corresponding monodromy matrices to investigate their behaviour their quotient under analytic continuation around the singular points. So he studied the functions $H = \frac{c_1 K + c_2 K'}{c_3 K + c_4 K'}$, and $q = e^{-\pi H}$. He then compared his results with a similar analysis of J and J' . This gave him some insight into how the images of the singular points accumulate on the boundary of the unit disc. In the first case (K and K') he found that a natural boundary arose: k^2 as a function of q is holomorphic inside the unit circle but cannot be continued beyond it. However, in the second case there was no natural boundary, and analytic continuation in and out of the unit disc gave rise to the multi-valuedness that had intrigued Hermite. Fuchs's conclusions were valid, even though his analysis of the point sets themselves was flawed as Dedekind later showed (Dedekind 1877), but the whole subject of point sets was in its infancy and mistakes of this sort were to prove a powerful stimulus for the development of point set topology.

At all events, Hermite replied to Fuchs's letter to say that he was delighted with it.²⁵ Not only had it explained the difference between $\frac{K}{K'}$ and $\frac{J}{J'}$ but it had done so in a way, said Hermite in a revealing remark, "which I judge to be of the greatest importance for the history of elliptic functions. The truly fundamental point that the real part of H is essentially positive I had sought in vain to establish by elementary methods, in order not to be obliged to turn to the new method discovered by Riemann".

Hermite had also written in this vein to Fuchs's friend Koenigsberger after the latter had published his book on elliptic functions to say that "I only regret that the difficulty of language can be a serious obstacle when the subject is not at all perfectly understood by me, and this is what has hitherto prevented me from taking account in a sufficiently profound manner of the part of your book that is dedicated to the general study of functions on Riemannian principles".²⁶ After raising several specific questions, Hermite concluded "German students will give you more satisfaction in such matters". Although Hermite's claim was far from being accurate—one could recall Schwarz's and Weierstrass's sharp criticism of the book (see Sect. 6.6)—plainly, Riemann's ideas were going to have to wait at the gates of Paris for a few more years.

²⁴See the lengthy extract published as (Fuchs 1877).

²⁵Hermite to Fuchs, November 27th 1876, added by Schlesinger to the reprint of (Fuchs 1877) in vol. 2 of his *Ges. Math. Werke*. See Fuchs (1877, 113–114).

²⁶This refers to (Koenigsberger 1874). The letter is quoted in (Koenigsberger 1919, 147).

7.4 Confluent Differential Equations

From the standpoint of complex function theory, the singularities in Bessel's equation are essential, whereas the singularities in the hypergeometric equation and those of the Fuchsian class are merely finite poles. In the 1890s it was realised that equations defining the special functions can be obtained from those of the Fuchsian class by a process called confluence, in which simple singularities are amalgamated. From this point of view, the first good example derived from insights into Kummer's (1836) study of the hypergeometric series, although throughout that long paper the independent variable z was almost always real.

7.4.1 Kummer's Equation

Kummer, inspired by the well-known limiting expression for the exponential function

$$\lim_{m \rightarrow \infty} \left(1 + \frac{y}{m}\right)^m = e^y,$$

took the hypergeometric function $F(\alpha, \beta, \gamma, x)$, let $\beta = m, x = y/m$, and let m tend to infinity, obtaining this series (where we have replaced Kummer's y by x for our later convenience):

$$1 + \frac{\alpha \cdot x}{\gamma \cdot 1} + \frac{\alpha(\alpha+1)x^2}{\gamma(\gamma+1)1 \cdot 2} + \frac{\alpha(\alpha+1)(\alpha+2)x^3}{\gamma(\gamma+1)(\gamma+2)1 \cdot 2 \cdot 3} + \dots \quad (7.14)$$

In his paper (1836) he denoted the function defined by this series $\phi(\alpha, \gamma, x)$, and proceeded to study a special case of it, the function $\psi(\gamma, x)$ defined by the series:

$$1 + \frac{x}{\gamma \cdot 1} + \frac{x^2}{\gamma(\gamma+1)1 \cdot 2} + \frac{x^3}{\gamma(\gamma+1)(\gamma+2)1 \cdot 2 \cdot 3} + \dots \quad (7.15)$$

The connection between ϕ and ψ is given by the formula

$$\psi(\gamma, x) = e^{\pm 2\sqrt{x}} \phi(\gamma - 1/2, 2\gamma - 1, \pm 4x)$$

or, equally,

$$\phi(\gamma, 2\gamma, x) = e^{x/2} \psi\left(\gamma + 1/2, \frac{x^2}{16}\right).$$

The integral $y = -\int_0^\infty u^{\gamma-1} e^{-u} e^{-x/u} du$ satisfies this differential equation, as Kummer showed by differentiating it twice:

$$x \frac{d^2 y}{dx^2} + (1 - \gamma) \frac{dy}{dx} - y = 0,$$

of which the general solution, as Kummer wrote down immediately, is

$$y = A\psi(1 - \gamma, x) + Bx^\gamma\psi(1 + \gamma, x),$$

where A and B are arbitrary constants. Later in the paper he wrote down a differential equation differing only trivially from the one for which the function $\phi(\alpha, \gamma, x)$ is a solution:

$$x \frac{d^2 y}{dx^2} + (\gamma - x) \frac{dy}{dx} - \alpha y = 0. \quad (7.16)$$

This equation has come to be known as Kummer's equation.²⁷

That Bessel's equation can be reduced to Kummer's was first shown by Hansen in 1855, as [Hankel \(1869, 468\)](#) stated referring to ([Hansen 1855, 252](#)). The solution $J_n(x)$ of Bessel's equation

$$x^2 \frac{d^2 y}{dx^2} + x \frac{dy}{dx} + (x^2 - n^2) y = 0$$

is related to the solution $\psi(n+1, -x^2/4)$ of Kummer's equation by the equation

$$J_n(x) = \psi(n+1, -x^2/4) \frac{(x/2)^n}{\Gamma(n+1)}.$$

7.4.2 Confluence

In the second part of vol. 1 of his *Handbuch* Heine dealt extensively with Lamé functions. They were introduced in the 1830s by Lamé's as solutions of the differential equations that bear his name. Lamé was led to them when integrating the heat equation in the steady-state case (i.e. the temperature is independent of time), that is integrating Laplace's equation $\Delta V = 0$ for particular classes of bodies (and of their surfaces). Looking for isothermal surfaces, Lamé's idea was to introduce new systems of coordinates, and the corresponding coordinate surfaces, thus generalising an idea that Euler and Laplace had applied in the case of spherical coordinates. In (1837) he considered three families of isothermal, confocal surfaces of the second order, that is ellipsoids, hyperboloids of one sheet and hyperboloids of two sheets, depending on parameters μ, ν, ρ , respectively. [Lamé \(1837, 156–160\)](#) showed that they are mutually orthogonal, that is any surface of one family cuts the surfaces of the other two families orthogonally along the lines of curvature. Thus, any point in space is given by the intersection of an ellipsoid with two hyperboloids (of one and two sheets, resp.), and the parameters μ, ν, ρ can be considered as a new coordinate system. In the second part of the paper he limited himself to show that under a

²⁷ Another linearly independent solution is $x^{1-\gamma}\phi(1+\alpha-\gamma, 2-\gamma)$.

suitable change of variables Laplace equation $\Delta V = 0$ reduced to three ordinary differential equations (Lamé equations) of the form

$$\frac{d^2 E}{d\varepsilon^2} + [Qb^2(\mu^2 - c^2) + Rc^2(\mu^2 - b^2)] E = 0,$$

(where b and c are parameters of the isothermal, confocal surfaces, Q and R are constants) and the like for $Y(\eta)$ and $X(\xi)$, where ε, η, ξ are transcendent functions of μ, ν, ρ (Lamé 1837, 180). In a subsequent, epoch-making paper (Lamé 1839) he succeeded in integrating them for a three-axis ellipsoid by introducing suitable ellipsoidal coordinates that allowed him to use separation of variables to reduce Laplace partial differential equation to three ordinary ones.

The study of curvilinear coordinates and related families of mutually orthogonal surfaces quickly grew up as an important topic in itself. On the other hand, the importance of Lamé's work in the theory of differential equations was immediately recognised by Liouville in a footnote added to (Lamé 1837, 143). This was just the beginning of an enormous amount of work on Lamé's equations and functions that Heine summarised in no less than 150 pages of his *Handbuch*. In particular, there (vol. 1, 397–399) he mentioned recent researches by Hermite.

Since the theta functions satisfy the heat equation it is not surprising that heat conduction is a problem leading to elliptic integrals and thence to elliptic functions. This was decisively exploited by Lamé and Jacobi among others, although we note Lamé's caustic comments (Lamé 1857, 44) to the effect that one day teaching programmes in the major applied sciences will faithfully follow the progress of that science, but that this evident truth is so little recognised that these programmes could have been written by Laplace and his pupils and no trace can be found of the important discoveries of Fourier, Abel, Jacobi, and Sturm and other modern mathematicians.

The discoveries of Lamé and Jacobi were pulled together by Hermite in 1877, where he showed that many problems came down to the study of Lamé's differential equation (in the Jacobi form):

$$\frac{d^2 y}{dx^2} = (n(n+1)k^2 sn^2 x + h)y, \quad (7.17)$$

where n was an integer, k the modulus and h a constant.²⁸ The case $n = 1$ gave the rotating top as studied by Jacobi, and $n = 2$ the spherical pendulum. As with his predecessors, Hermite found that this question was fruitful both for applied mathematics and for the theory of elliptic functions. He also found that the interaction was fruitful for the study of the solutions to Lamé's equation, which he called elliptic functions of the second kind. Because these functions were much less

²⁸Beginning in 1877, Hermite published a series of notes in the *Comptes rendus* of the French Académie (Hermite 1877–1882) that he re-published separately in 1885 as a book (Hermite 1885).

well understood, Hermite made more use of the ideas of complex function theory in this part of his research. More precisely, his principal aim was to prove that for any h the general integral of the equation (7.17) was given by

$$y = CF(x) + C'F(-x),$$

where C and C' are constants. The function $F(x)$ satisfies the conditions

$$F(x + 2K) = \mu F(x)$$

$$F(x + 2iK') = \mu' F(x)$$

with μ and μ' constants and was determined by following the same route we described in (Sect. 6.6.2).

Eventually, by the early 1890s Lamé's equation attracted Klein's attention, and this led him to the idea of confluence. It was noticed by Klein in his lectures (Klein 1894a) and by Bôcher (1894) that the known useful linear differential equations can all be derived from one standard equation, a Lamé equation (see (7.17)) with five singular points where the exponent differences are all $\frac{1}{2}$, by a process called the confluence of singularities.²⁹ When two such singular points are made to coincide the result is a regular singular point with arbitrary exponent differences that can be determined by astute manipulation of the coefficients of the differential equation. When three or more singular points in the original equation are brought together, an irregular singular point results. Without going into the details (for which see Ince 1926, Chap. XX) the original equation yields Legendre's differential equation when three singular points coincide at a finite point and two at infinity, and the quadratic change of independent variable from z to x is made, where $z^2 = x$. If instead three singular points are made to coincide at infinity and the remaining two are left distinct, the result is Mathieu's equation. A quadratic change of variable applied to this equation yields Bessel's equation. If four singular points come together at infinity and a quadratic transformation is applied, the result is Weber's equation.

Commenting on this, Klein (1894a, 42) was happy to state that the idea of confluence, according to which "functions of the mathematical physics of seemingly so different form appear under a common, higher point of view" showed that "it is not only a mathematical abstraction but instead it lay in the nature of things". This became a topic much liked by Whittaker and other British applied mathematicians. Whittaker wrote the confluent hypergeometric equation as

$$\frac{d^2u}{dz^2} + \frac{du}{dz} + \left(\frac{k}{z} + \frac{1/4 - m^2}{z^2} \right) u = 0.$$

²⁹See (Klein 1894a, 40–42) and (Bôcher 1894, 193–194).

He set $u = e^{-z/2}W$ and treated it as an equation for W whose solutions are the Whittaker functions $W_{(k,m)}$, defined for all k and m and all z except the negative reals. These functions can express the error function, the incomplete Gamma function: $\gamma(n,x) = \int_0^x t^{n-1} e^{-t} dt$, the logarithmic integral function: $li(z) = \int_0^z \frac{dt}{\log t}$, and Weber's and Bessel's cylinder functions.³⁰

7.5 Integrals

The theories of the hypergeometric equation and the Bessel and Kummer equations were rich in expressions for integrals. As these topics gradually became complex, these integrals first acquired a somewhat formal status. The use of contour integrals became of particular importance in the approximate estimation of functions for large values of a parameter (see Schlissel 1976–1977). It began with a paper on the propagation of heat in a cylinder by Lipschitz (1859), who gave an asymptotic expansion for $J_0(x)$ via a contour integral along a rectangle in the complex plane, and with Riemann, who in 1855 published a short paper on a problem in physics that also led him to the case of Bessel's equation where $n = 0$. Riemann's paper proved unsuccessful as a piece of physics, but it led him to exhibit two semi-convergent (or asymptotic) series that approximated the solutions for large values of the variable. His cryptic treatment was later explained in (Weber 1872 and 1890).

7.5.1 Steepest Descent

The paper in which Riemann initiated the method of steepest descent is the posthumously published fragment (Riemann 1863) edited by Schwarz. In that paper, Riemann was occupied with the problem of expanding his P -functions as continued fractions. He used an integral representation for the functions in which the n th convergents depended exponentially on a parameter n . So his fundamental question became to estimate these integrals for large values of n . The basic idea of his method is due to Laplace (when all the variables and functions are real, see (Laplace 1785a and 1810), who argued that the greatest contribution to integrals where the integrand is of the form $g(t)e^{h(t)}$ is made when the exponential factor is as large as possible, and that this occurs where the function $h(t)$ has its local maxima. At these points, $h'(t) = 0$ and $h''(t) < 0$, and Laplace made a change of variable adapted to highlighting these points.

In the complex case, the driving idea is still to estimate the value of the integral by using those paths along which the exponential factor makes its greatest contribution.

³⁰See (Whittaker and Watson 1927, 337–354).

Now, as is described in (Erdélyi 1956, 40) if the absolute value $|e^{zh(t)}|$ is considered as a surface over the complex t -plane, the level curves on this surface are given in the t -plane by $\operatorname{Re}(zh(t)) = \text{constant}$, and the curves of steepest descent, the gradient lines, are given by $\operatorname{Im}(zh(t)) = \text{constant}$. Along the level curves the phase of $e^{zh(t)}$ is changing as fast as possible; along the gradient lines the phase is constant and the modulus of $e^{zh(t)}$ is changing as fast as possible. The lines of steepest descent meet at points, called cols, at which $h'(t) = 0$. To estimate the integral, the best paths to use are the paths of steepest descent that join one end point to the other via a sequence of cols. Along any of these paths of steepest descent the integral is monotonic, and Laplace's method can be used.

Riemann started from the observation that the convergents involved the expression

$$P_n = x^{a+n}(1-x)^c \int_0^1 s^{a+n}(1-s)^{b+n}(1-xs)^{c-n} ds,$$

with a similar expression for Q_n that we do not give here. In the part Schwarz reconstructed from formulae and two diagrams he took to have been drawn by Riemann, Riemann looked for an asymptotic expression for $\int_0^1 s^{a+n}(1-s)^{b+n}(1-xs)^{c-n} ds$ for large values of n . He set

$$s^{a+n}(1-s)^{b+n}(1-xs)^{c-n} = \phi(s)$$

and $\frac{s(1-s)}{1-xs} = e^{f(s)}$ and sought to calculate $\int_0^1 e^{nf(s)} \phi(s) ds$ as n tends to infinity. Schwarz suggested that Riemann had argued that one could consider paths of integration along which $e^{f(s)}$ had a constant modulus. For small values of the modulus these paths enclose the points $s = 0$ and $s = 1$, whereas for large values of the modulus the curves enclose $s = 1/x$ and $= \infty$. In between is a curve roughly in the shape of a figure 8 with a double point at $s = \sigma$ where $f'(s) = 0$. (We suppress an argument in which Riemann calculated the appropriate value of s for which $f'(s) = 0$.) The best path to use in evaluating the integral would then go, he suggested, from $s = 0$ to $s = \sigma$ along which the modulus steadily increases, and then from $s = \sigma$ to $s = 1$ along which it continually decreases. When n is "infinitely large", as he put it, only those parts of the integral near $s = \sigma$ make a significant contribution. He could now estimate his integrals and obtain the continued fraction expansion he sought.

7.5.2 Stokes' Sectors

More progress was made by Hankel, who dealt with $J_n(x)$ in *Mathematische Annalen* (Hankel 1869), and, independently, the Irish applied mathematician Sir George Stokes, who both discovered an important and truly complex phenomenon, that of Stokes' sectors. In an earlier paper (dated 1850 but published in 1856) Stokes had observed that Airy had given the integral

$$W = \int_0^{\infty} \cos \frac{\pi}{2}(w^3 - mw)dw$$

for the evaluation of the intensity of illumination near a caustic, but, commented Stokes, whereas experimenters had noted as many as 30 dark bands this integral only gave the location of the first two and only then after extensive use of 10-figure logarithms. The problem was that the expansion of the integral as a series in powers of the parameter m gave a series that converged too slowly to be of practical use. Stokes therefore developed a method that worked for higher values of the parameter, but which in turn raised problems of its own that he tackled in his (1864).

In this paper Stokes took Airy's differential equation

$$\frac{d^2u}{dx^2} - 9xu = 0$$

and wrote its solution in two ways: as an arbitrary sum of two series convergent in ascending powers of x that converge for all values of x , and as an arbitrary sum of two series in descending powers of $x^{-3/2}$ multiplied by terms of the form $x^{-1/4}e^{\pm 2x^{3/2}}$. These second series fail to be periodic in the amplitude of x , he said, unless the arbitrary coefficients are discontinuous as functions of the amplitude, and he proceeded to investigate this new phenomenon. He found that these series were asymptotic (semi-convergent, he called it) and showed how they could be used to calculate the value of the solution function if truncated at their smallest terms.

While he could therefore solve his equation, he found that he could not obtain solutions for large negative values of x from those for large positive values, because the series were, as he put it, illusory when x was small. Neither could he let x become imaginary, because then the integrals diverged. In a late night flash of inspiration he solved the matter by showing that the complex plane must be divided into sectors.³¹ In each sector a solution can be written as a sum of two approximate solutions with suitably chosen arbitrary constants, but the constants must be changed as one changes sectors. The position of these sectors is determined by the places where the formal solutions switch roles and the formerly smaller one comes to dominate the other, and this comes about because the asymptotic series are each multiplied by a dominant factor which goes rapidly from being very large to very small as one crosses the lines that separate the sectors.

Hankel's approach to Bessel's equation for large values of the parameter m had also led him to divergent series which could be truncated at their smallest terms to yield good approximate solutions.³² He too encountered Stokes' phenomenon, and

³¹See the letter quoted in (Schlissel 1976–1977, 317n).

³²Stokes, in a footnote he added to the reprint of his (1864, 80) in his collected papers, observed that he had found the same formulae for the Bessel functions that Hankel gave, eleven years before Hankel, who then gave “the same explanation of the discontinuities of their constants”. He went on: “At that time the method of complex contour integration, developed by Riemann in his fundamental memoir on the hypergeometric series, a few months before the present paper, afforded the natural

explained it by cutting the complex plane along the negative imaginary axis, where, as he said, the formal power series lost their asymptotic character. He knew that his treatment of these semi-convergent series (as he called them, following traditional terminology) was unrigorous, and hoped that it might yet serve as the first building brick in a general theory, which could be clarified for the first time by complex function theory, just as the theory of convergent series had been.

Unfortunately, Hankel did not live long enough to accomplish this task himself, and the methods of Hankel and Stokes, initially imprecise but suggestive, were only to be made rigorous in the 1880s with Poincaré's creation of a theory of asymptotic analysis. However, it should be noted, as Berry has eloquently pointed out (Berry 1989), that Poincaré's preference for studying the error by approximating an asymptotic series at a fixed order deals less well than Stokes's method when it comes to investigating the size and nature of the error.

7.5.3 Asymptotic Expansions

Poincaré was led to organise the ideas that were current about non-convergent series in his (1886) because of the way they arose in problems concerning linear ordinary differential equations that were not of the Fuchsian type. He began by reminding his readers of the curious property of Stirling's series for $\log \Gamma(x+1)$, which is

$$\frac{1}{2} \log(2\pi) + \left(x + \frac{1}{2}\right) \log x - x + \frac{B_1}{1.2} \frac{1}{x} - \frac{B_2}{3.4} \frac{1}{x^2} + \frac{B_3}{5.6} \frac{1}{x^3} \dots,$$

where the B_k are the Bernoulli numbers. This series diverges for all values of x but yet for each n , if S_n denotes the sum of the series up to and including the term in $\frac{1}{x^n}$, the expression $x^{n+1}(\log \Gamma(x+1) - S_n)$ tends to zero as x increases. Accordingly this divergent series nonetheless provided excellent approximations to the value of the Gamma function for large values of x . Cauchy had already noticed this peculiar behaviour in 1843 in a paper on "the legitimate use of divergent series",³³ but no-one took topic up until Poincaré did in 1886. At the same time, Stieltjes made the study of asymptotic expansions for real, positive values of x ("semi-convergent series", as he called them) the subject of his *Thèse*.³⁴

Poincaré said that a divergent series $A_0 + \sum_j \frac{A_j}{z^j}$ represented a function $J(z)$ asymptotically, or was an asymptotic series, if S_n , the sum of the first $n+1$ terms, was such that $z^n(J - S_n)$ tends to zero as z increases. It follows that $J - S_{n-1} = \frac{A_n + \varepsilon}{z^n}$, where A_n is finite and ε very small. He developed the algebra of such series,

means of procedure". He observed that his method worked even "when no expression of the form of a complex integral is available".

³³See his *O. C.* (1) 8, 18–25.

³⁴See (Stieltjes 1886). For a more modern introduction to the subject, see (Erdélyi 1956).

showing that they can be added and multiplied together according to the usual rules for convergent series, and that they can validly be integrated term by term. He then applied these ideas to linear differential equations where the method of formal power series solutions generated asymptotic series expansions for solutions in a neighbourhood of $z = \infty$. In particular he gave conditions for these asymptotic series to represent the solutions asymptotically, noting carefully that an asymptotic representation cannot be valid for all values of the argument of z unless it is in fact convergent, and that for any given fixed value of the argument of z the asymptotic series representing a given function is unique if it exists at all. Incidentally, it is worth mentioning that Poincaré's paper was criticised by Thomé, who claimed in his (1887) that Poincaré's results were scarcely of interest, and pointed out by means of an example that the exponents in the formal divergent expansions could be completely different from the ones in the actual expansions in series. In his short reply Poincaré (1887b) objected that "when facing two equally unsolvable problems it not useless to show that they reduce to each other" (Poincaré 1887b, 334).

In spite of this, Poincaré's intervention seems to have created a theory of asymptotic expansions where previously there had only been ad hoc techniques, and to have opened the door for the return into rigorous mathematics of divergent series. Borel's *Leçons sur les séries divergentes* (1901) is a major work in this regard. It is also true, as Lord Rayleigh and others pointed out, that the study of optics, and especially diffraction, continued to generate delicate problems involving the estimation of integrals involving a large parameter.

It is often said that the first to bring the topic back to the complex domain was Debye, in his paper (1909), and indeed his is the paper from which the modern theory derives. However, mention should be made of (Nekrasov 1885–1886) in which the Russian mathematician extended the approach in (Cauchy 1829a) without knowing anything of Riemann's work. Nekrasov gave a more general account of the saddle-point method than Debye was to do, but gave neither an asymptotic estimate nor an asymptotic expansion of his integrals (see Petrova and Solov'ev 1997).

Debye sought to show how naturally complex integration enters the problem and provides answers that can be used to study diffraction problems. He focussed his attention on a Hankel function, already highlighted by Sommerfeld in his (1896) paper on diffraction. This is the function

$$H_2^\alpha(x) = -\frac{1}{\pi} \int e^{-xf(t)} dt,$$

where the integral goes from $\frac{3\pi}{2} + i\infty$ to $\frac{\pi}{2} + i\infty$ and $f(t) = i\left(\sin t - \frac{\alpha t}{x}\right)$. He wrote $f = R + iJ$, so the absolute value of the integrand is e^{-xR} and argued that as the parameter x grows R has to become as small as possible. This is achieved by making the normal derivative of R zero, and by the Cauchy–Riemann equations this happens for a direction s for which $\frac{\partial J}{\partial s} = 0$, so J must be a constant. These are the curves of steepest descent. A further analysis shows that the paths to be used are those that pass through the cols and have a head and a tail joining them to the designated end points of the integral. It remained to estimate asymptotically the value of the

integral, and here Debye, relying on the techniques in (Borel 1901b) found that the series expansions that entered the estimate were only semi-convergent.

7.5.4 Complex Oscillation Theory

In his paper on the zeros of Bessel functions Hurwitz (1889) noted that the zeros of the function J_n are all real when the parameter $n \geq 1$. He then extended the theory to cover all real values of n by a method applicable to complex functions in general, which is of independent interest.³⁵

Hurwitz argued that a power series $f(z) = \sum_j a_j z^j$ is approximated by its successive partial sums $g_m(z) = \sum_j^m a_j z^j$ and the zeros of f are arbitrarily well approximated by the zeros of $g_m(z)$. More precisely, he said, the set of roots of the equations

$$g_1(z) = 0, g_2(z) = 0, g_3(z) = 0, \dots$$

has some accumulation points, and every accumulation point of this set that lies in the circle of convergence for f is a zero of f and vice versa. To prove it, Hurwitz first established the lemma that if $f(z)$ and $g(z)$ are any two continuous single-valued functions inside and on the boundary B of a simply connected region R , and if neither f nor g vanish at any point of B , and if on B $|f(z) - g(z)| < |f(z)|$, then the function f vanishes as often inside the region R as g does. This is the result we call Rouché's theorem, but Hurwitz seems not to have known of the earlier publication. Hurwitz proved it by setting $f(z) - g(z) = f(z) \cdot u$ and considering the logarithmic integrals around the boundary B . This yields

$$\int d \log g(z) dz = \int d \log f(z) dz + \int d \log(1 - u) du,$$

but because the above inequality implies that $|u| < 1$ the boundary B does not include the zero of the third integral, which means that $\log(1 - u)$ is constant and so this integral vanishes. Therefore

$$\int d \log g(z) dz = \int d \log f(z) dz,$$

from which the result follows. It extends, he noted, to a comparison of the difference in the number of zeros and poles of f and g when these functions have poles. The lemma yields the main result, on the assumption that the g_m tend uniformly to f in some bounded domain, by comparing f and successive g_m . Hurwitz's argument

³⁵Hille (1976, 284) incorrectly ascribed the discovery of this result to Hurwitz, but it was known earlier, to Poisson. He also neglected Klein's contributions entirely and called the whole approach function-theoretic, thereby slighting the geometrical aspects.

permits a more general conclusion, by which his result is usually known today: if a sequence of meromorphic functions tends uniformly in a given bounded domain to a function that is not identically zero, then every point where the limit function is zero is a limit point of the zeros of functions in the sequence. Moreover, this remains true when multiplicities are taken into account: an r -fold zero of the limit function is a limit of r zeros of the functions in the sequence.

To apply his theorem in any given case it was necessary for Hurwitz to choose the g_m suitably. To study the Bessel functions

$$J_n(z) = \left(\frac{z}{2}\right)^n \left(\sum_{j=0}^{\infty} \frac{(-1)^j \left(\frac{z}{2}\right)^{2j}}{\Gamma(n+j+1)\Gamma(j+1)} \right),$$

where, ultimately, n will be a complex number with positive imaginary part, he introduced the functions

$$f_n(z) = \sum_{j=0}^{\infty} \frac{z^j}{\Gamma(n+j+1)\Gamma(j+1)}.$$

that enters Bessel's series and which he took over from the work of Heine, Christoffel and Lommel. The function $f_n(z)$ is related to the n th Bessel function J_n by the equation

$$J_n(z) = \left(\frac{z}{2}\right)^n f_n\left(-\frac{z^2}{4}\right).$$

This means that $w \neq 0$ is a zero of f_n if and only if $\sqrt{-4w}$ is a zero of J_n . The partial sums of the series for $f_n(z)$, the corresponding $g_m(z)$, are the numerator and denominator of the continued fraction expansion of $\frac{f_n(z)}{f_{n+1}(z)}$, which made them particularly attractive as approximations.

Hurwitz then pursued the analysis set out in his opening paragraphs. The zeros of the functions g_{2j} can be understood: when $-1 < n < 0$ one zero of g_{2j} is positive and the remaining $j-1$ are negative, and when $n > 0$ all the zeros are negative. Hurwitz concluded that the zeros of $f_n(z)$ are all real when $n > -2$, they are all negative when $n > -1$, and in between, when $-1 > n > -2$ one zero is positive and the remainder are negative. The case $n < -2$ needs further examination, because now some of the zeros of g_{2j} can be imaginary, but Hurwitz was able to obtain some results even in this case, which we suppress here.

In 1890, Klein wrote a paper ([Klein 1890b](#)) investigating the number of zeros of the hypergeometric series in the interval $[0, 1]$. He began by recalling the results of Sturm–Liouville theory for a real 2nd order ordinary differential equation

$$\frac{d^2y}{dx^2} + p(x)\frac{dy}{dx} + q(x)y = 0,$$

and the nets of triangles Schwarz had introduced in his solution to the problem of when the hypergeometric equation has algebraic solutions. In the complex case, Klein produced a net of n -sided polygons, where n is the total number of poles of p and q (p and q are still assumed to have real coefficients). Each polygon is the image of a half-plane under the map $\eta(x)$, where η is a quotient of a basis of solutions to the differential equation. Accordingly, when the hypergeometric equation is considered, the net is composed of triangles. Unlike the algebraic case, however, the angles of the triangles are not restricted to a small range of values, and the net will have branch points. The zeros of the hypergeometric series are counted in a way that reflects how the net overlaps itself: the number of times a side returns to or overlaps itself differs by at most 1 from the number of zeros of any real solution of the differential equation on the corresponding interval. (Separate considerations dispatched the residual ambiguity.)

Klein had now turned his problem into a geometrical one, which suited his temperament, and was able to describe how many zeros of $F(\alpha, \beta, \gamma, x)$ lie in the interval $[0, 1]$. His results were promptly confirmed by Hurwitz in a short paper the same year (Hurwitz 1890) using the methods of Sturm–Liouville theory directly. The conclusion is

	α	β	γ	N
I.	+	+	−	$\frac{1-(-1)^v}{2}$
II.	+	−	+	μ
III.	+	−	−	$\mu - v$, for $\mu \geq v$ otherwise $\frac{1-(-1)^{\mu+v}}{2}$
IV.	−	−	−	$\frac{1-(-1)^{\lambda+\mu+v}}{2}$

where $\lambda = |1 - \gamma|, \mu = |\alpha - \beta|, v = |\gamma - \alpha - \beta|$.

In 1902 Klein’s arguments were revised, simplified, and extended by the American mathematician Edward van Vleck (van Vleck 1902).³⁶ Van Vleck had studied mathematics at Johns Hopkins for two years before going to Göttingen to take a PhD under Klein (on the continued fraction expansion of analytic functions) (see Parshall and Rowe 1994, 213). He praised Klein’s paper as being “notable both for its results and for its method” (p. 110) and showed how to dispense with the above-mentioned ambiguity. More importantly, he showed how Klein’s method helped locate the zeros in the imaginary domain, and indicated how the same method could be applied to other linear ordinary differential equations.

Hurwitz also returned to the subject, in 1907, at which time he was still unaware of van Vleck’s paper. He learned of it only when his own paper (Hurwitz 1907) was submitted to *Mathematische Annalen*, when Klein sent him a copy. Hurwitz commented in a footnote at the end of the paper that his results agreed with those

³⁶Klein had made the subject part of his fifth Evanston Colloquium lecture, and mentioned some of van Vleck’s earlier work there, see Klein (1894b, 39).

of van Vleck, but that he had also obtained a formula for the number of zeros of an arbitrary branch of a Riemannian P -function (Hurwitz 1907, 705). In fact, he also corrected some of van Vleck's formula in the special cases when α, β and $\gamma - \alpha$ or $\gamma - \beta$ are negative integers.³⁷

7.6 Expansions in Series and Functional Operations

In the early 1880s results on the expansion of analytic functions in series of special functions such as the Legendre polynomials or Bessel functions, as given by the works of Neumann, Heine, Frobenius and others, suggested to Pincherle that he should tackle the general problem of the expansion of analytic functions in series of functions belonging to a given system. This was included in the subjects he chose to deal with in his 1882/83 course of higher analysis in Bologna. He asked Poincaré for advice on June 10, 1882 and sketched his program in a long letter that is worth quoting in some detail for the interesting survey it offers of open problems in complex function theory.³⁸

Pincherle collected them in four classes: "A. 'Given an element of an analytic function, establish the properties of the function that it defines'." Following Weierstrass's approach, Pincherle observed, it is known that from the element one obtains the value(s) of the function in its "whole domain of validity" by means of analytic continuation, but this method is "not well suited to establish (1) the limits of the domain of validity; (2) whether the function is single- or multi-valued; (3) whether it satisfies either an algebraic equation or an algebraic-differential equation or whether it belongs to any known class of functions. "Thus", Pincherle concluded, "it seems that one of the main problems of function theory should be the following: "to recognise the three above characteristics of a function from the laws of the coefficients of the element". I do not know whether this problem has been solved apart from the cases of the recurrent, and the hypergeometric series".

The second class of problem Pincherle identified with the general statement: "B. 'What functions can be expressed by means of determined, arithmetical operations?' ". Of course, if the arithmetical form is given by a finite number of rational operations the function is a rational one, and there nothing more to say. But, Pincherle asked, in the case of infinite series, infinite products or continued fractions, what about the domain of convergence of the arithmetical form? If the domain two-dimensional, when does the form represent an analytic function? These questions, he said, have been partly answered in the case of series, but he admitted that he did not know if they had been answered in the other cases. As for series, Pincherle went on, uniform convergence had provided a sufficient condition, not a necessary one. The construction of functions with prescribed zeros and infinities

³⁷ All these results were also obtained in (Schafheitlin 1908).

³⁸ For the correspondence between Pincherle and Poincaré see Dugac (1989, 210–217).

belongs to this class of problems, as does the fact that the very same arithmetical form can represent different functions in disjoint domains. Also included in this class of problems is the study of series of the form $\sum a_n P_n(x)$ where the functions $P_n(x)$ belong to a given system (Fourier series, spherical functions, etc.).

Pincherle's third class of problems was given by "C. 'Research into functions satisfying a given property in their domain of validity'." This class of problems included the solution of differential as well as functional equations, and above all the solution of Cauchy's problem by means of analytic functions. Finally, the last class of problems, D, involved the study of the behaviour of a function on the boundary of its "domain of validity", including Weierstrass's and Riemann's results on essential, and resp., branch points, and Weierstrass's (and Poincaré's) results on gap series.

In his prompt reply³⁹ on June 15 Poincaré agreed with Pincherle's approach for, he wrote, "this way is best suited to expound the general theory of functions if one wants the true sense of the problems that one is going to deal with to be understood". Poincaré agreed with Pincherle that the problem of recognising the essential properties of a function represented by a power series from its coefficients "is far from being solved" and "there is still a lot to do". As for recurrence and hypergeometric series, "I think—Poincaré added—that under the latter name you include not only Gauss's series but all the series that represent the integrals of linear, differential equations with rational coefficients; in fact, there is a linear recurrence relation among p consecutive coefficients of such a series (completely analogous to Gauss's series) involving the rank n of the first of these p coefficients. So there is a condition that allows one to recognize from the law of the coefficients whether the series satisfies a linear equation; and, consequently, *whether it represents an algebraic function*." [Poincaré's emphasis]. There are other cases in which the law of coefficients "immediately shows what is the domain of validity of the function", Poincaré added. As examples he gave the series

$$\sum \frac{1}{2^n} x^{3^n} \text{ or } \sum \varphi_p(n) x^n,$$

where $\varphi_p(n)$ represents the sum the p -powers of the divisors of n , whose "domain of validity" is the unit disk.

As for Pincherle's second class of problems, Poincaré admitted that the case of continuous fractions had not yet been studied as deeply as it deserved.

Finally, Poincaré added a new class of problems dealing with the conformal mapping and Dirichlet principle that "unfortunately have been treated for a long time without a sufficient rigour, but one finds a rigorous solution of them" in (Schwarz 1870c). Some years later (see Sect. 7.7.1 below) Poincaré himself was to take up this latter problem. In addition, he devoted his (1885a) to the study of

³⁹This letter is in the first of 55 manuscript volumes by Pincherle kept in the library of the Mathematical Department of the Bologna University. See Bottazzini and Francesconi (1989), and Dugac (1989, 215–217).

the domain of convergence of series of the form $\sum c_n p_n(x)$ where $p_n(x)$ are analytic function belonging to a given system.

On his part, Pincherle tackled some of problems he had presented in (Pincherle 1882) where he studied series of the form $\sum \phi_v(x) f_v(x)$ under various hypotheses on the systems of functions $\phi_v(x)$ and $f_v(x)$. There he began by establishing the following general theorem: If to every point x_0 of a connected domain C , including its boundary, corresponds one and only one value of a function (in the more general sense of the term) $X(x)$, and if a neighbourhood U of x_0 can be assigned such that $\limsup |X(x)| = L(x_0)$ with $x \in U$ and $L(x_0)$ a finite number, then there exists a finite number N such that $|X(x)| < N$ in C . In the particular case of plane domains this theorem corresponds to the theorem usually named after Heine and Borel, as Pincherle (1925, 46) himself later recognised when claiming priority in the matter. In the same paper he studied the case where $\phi_v(x) = c_v x^v$ and $f_v(x)$ are regular functions at $x = 0$, which all the more important particular cases can be reduced to. Then, in a couple of memoirs Pincherle (1883–1884) referred to Neumann's and Heine's work on the expansion of an analytic function within an ellipse with foci ± 1 in series of Legendre polynomials (see Sect. 7.2.3 above) to remark that this result depends on a general property which connects the boundary curves of the domain of convergence of the series expanded in functions $f_n(x)$ of a given system with the nature of the generating function of the $f_n(x)$. He considered a function $T(x, y) = \sum_m \sum_n a_{mn} (x - x_0)^m y^n$ whose singularities x, y satisfy an entire (rational or transcendental) equation $f(x, y) = 0$, and succeeded in showing that the determination of the boundary curves for the convergence of the series $\sum c_n p_n(x)$, where $p_n(x) = \sum_m a_{mn} (x - x_0)^m$ depends on the moduli of the roots of the latter equation. Accordingly, Neumann's theorem turns out to be the particular case where $f(x, y) = 1 - 2xy + y^2$. In addition, Pincherle studied more extended classes of functions $p_n(x)$ to which a system of function $P_n(y)$ can be associated so that in bounded domains one has $\sum p_n(x) P_n(y) = \frac{1}{y-x}$. Later on, in a collection of papers published between 1889 and 1890, Pincherle dealt with the expansion of functions in series in the cases in which the coefficients are functions involving recurrent systems, finite difference equations, and continuous fractions.

More importantly, all these problems led Pincherle to face the question of the inversion of an integral, more precisely the question of solving an integral equations of the form

$$\int_{(c)} A(x, y) \phi(y) dy = f(x),$$

where $A(x, y)$ and $f(x)$ are given functions, and (c) is a suitable path. As Pincherle observed, this defines a linear functional $\mathbf{a}$ such that $\mathbf{a}\phi = f$. In the first part of his (1886) he established the properties of the functional in abstract terms then applied them to some particular cases including the Abel–Laplace transform where $A(x, y) = e^{xy}$. In the second part he considered the special case

$$\psi(x) = \frac{1}{2\pi i} \int_{(c)} A(x, y) \phi(y) dy,$$

where $A(x, y)$ is a rational function and (c) is a circle, thus re-obtaining results in Hermite's (1881) in particular.

The Laplace transform, which had been successfully applied by Poincaré (1885a, 1886) to the integration of differential equations, was studied by Pincherle from his new functional point of view in (1887a), where he showed that the Laplace transform allowed him to build new classes of entire, transcendental functions and, as a special application, to re-obtain Neumann's and Heine's entire theory of cylindrical functions.

In the same year Pincherle presented his functional approach in a paper published in *Acta* where he stated that the problem of the inversion of the integrals studied by him in general terms "occurs frequently in mathematical physics, and especially in potential theory" (Pincherle 1887b, 146).

7.7 Rigorous Methods in Potential Theory

The work of Green, Gauss, and Riemann showed very clearly that there was a rich domain in the study of real functions of two and three variables that would be essential in the study of physics (gravitation and electro-magnetism), which had its own charms, and which (in two dimensions) was a powerful tool in the emerging subject of complex function theory. But it contained an apparent weak spot: Dirichlet's principle. If this principle was used to solve Dirichlet's problem, then it needed to be proved, and that seemed very unlikely indeed. If the principle was not used, there needed to be an alternative solution to the Dirichlet problem, else the whole theory of harmonic functions collapsed. And yet no physicist doubted the existence of potential functions under any conditions. The gap between mathematics and physics was becoming painfully wide in the 1860s and 1870s.

Two types of issue emerged. While some mathematicians sought rigorous solutions to Dirichlet's problem in two or three dimensions for regions with simple boundaries, others analysed the nature of the boundary of a region, in the hope that perhaps all boundaries were simple enough to be accessible to the known methods. This hope was ultimately to be realised in two dimensions but shown to be false in three or more, and partly for that reason the following account is largely confined to two dimensions. That said, we cannot pursue here the full history of potential theory in the period and reach even that terminus of the story in two dimensions. It will be enough to see some of the major issues that were raised, and how some of them were solved.

7.7.1 Schwarz's Solution of the Dirichlet Problem

Prym may well have been the first mathematician to find fault with the use of the Dirichlet principle to solve the Dirichlet problem, but the person whose work made the point most successfully was Weierstrass's former student Schwarz, and unlike

Prym Schwarz then went a considerable way towards solving the problem. So too did his contemporary and rival Carl Neumann, who in 1870 solved the Dirichlet problem for convex domains in the plane (and in space as well) by using a rather different method, the method of arithmetic means. He expounded it at length in his (1877) and used it in 1884 in the second edition of his book (Neumann 1865a) to establish Riemann's existence theorems, and to solve the boundary problem named after him in the case of a disk. Further related researches were later published in (Neumann 1887). Neumann's important work is not discussed here in detail. We limit ourselves to remarking that Neumann's proof of the convergence of the series representing the sought-for harmonic function was flawed by a *beginner's mistake*, as Lebesgue (1937, 152) called it, which rather surprisingly went unnoticed for quite long time. Lebesgue pointed out that the essential step in Neumann's reasoning was to majorise the ratio of two consecutive terms of a series, and in doing this he failed to distinguish between $\frac{u_{n+1}}{u_n} < 1$ and $\frac{u_{n+1}}{u_n} < \lambda < 1$. Thus, Lebesgue concluded, in the end Neumann's reasoning was founded on the same confusion between upper bound and maximum (resp. lower bound and minimum) that Weierstrass had criticised. Lebesgue also noted that further criticism had already been raised by Volterra in (Volterra 1897).

Recall that the Dirichlet problem asks for a function defined on a simply connected domain T that is harmonic on the interior of T and takes prescribed values on ∂T , the boundary of T (say given by a continuous function defined on ∂T). Moreover, it is to be shown that this function is unique.

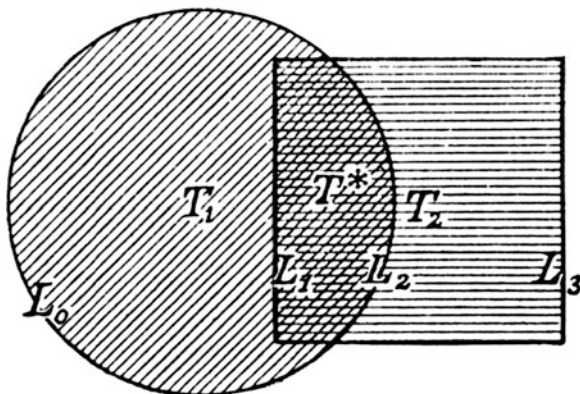
Schwarz developed his solution to this problem in a series of papers around 1870. He first showed that the problem can be solved when the domain T is a disc. It is therefore solved for any domain that is conformally equivalent to a disc, but around 1870 there was no consensus as to what such domains were. Riemann had claimed that every simply connected domain was conformally equivalent to a disc, but by methods that did not command universal assent (and are indeed difficult to make precise). Schwarz had earlier thought it worthwhile to show that a square was conformally equivalent to a disc, which makes it clear that the status of more complicated domains was as yet undecided. Schwarz therefore proposed to show how domains for which the Dirichlet problem was solved could be patched together and the problem solved for a very large class of simply connected domains indeed. This is his famous alternating method.

7.7.1.1 Schwarz's Solution for the Disc

In his paper (1870c) Schwarz argued as follows. He let $f(\phi)$ be an everywhere finite, continuous, real-valued function that is periodic with period 2π —so we can think of the function f as defined on the unit circle. He then considered the function $u(r, \phi)$ defined by two equations, the first of which is a Poisson integral, but Schwarz gave it no name:

$$u(r, \phi) = \frac{1}{2\pi} \int_0^{2\pi} f(\psi) \frac{1 - r^2}{1 - 2r \cos(\psi - \phi) + r^2} d\psi, \quad 0 \leq r < 1$$

Fig. 7.2 Schwarz's alternating method (Schwarz, *Gesammelte Mathematische Abhandlungen* vol. 2, p. 136)



and

$$u(1, \phi) = f(\phi).$$

This function is a solution of the Dirichlet problem, because it is finite and continuous on the whole closed unit disc $0 \leq r \leq 1$ and satisfies the differential equation $\Delta u = 0$ in the interior of the disc. The first claim requires a careful, but not difficult, analysis of the situation as $r \rightarrow 1$; the second follows by straightforward differentiation.

7.7.1.2 Schwarz's Alternating Method

Schwarz now supposed he had two domains, T_1 and T_2 which overlapped nicely, by which he meant that $T^* = T_1 \cap T_2$, the region common to both of them, was two-dimensional and the boundaries of the regions crossed with distinct tangents. That part of the boundary of T_1 that lay outside T_2 he called L_0 , and the part of the boundary of T_1 that lay inside T_2 he called L_2 . Similarly, that part of the boundary of T_2 that lay outside T_1 he called L_3 , and the part of the boundary of T_2 that lay inside T_1 he called L_1 (Fig. 7.2).

He further supposed that he had a solution to the Dirichlet problem for each domain T_1 and T_2 separately, for any prescribed functions g_1 on the boundary of T_1 and g_2 on the boundary of T_2 . He now sought a solution to the Dirichlet problem for the domain $T_1 \cup T_2$ with a prescribed function u on the boundary $L_0 \cup L_3$.

The region $T_1 \cup T_2 \setminus T^*$ he called T . His idea was that an air pump (his term) could be imagined that pumped air from the region T^* alternately into the two halves of T , the regions $T_1 \setminus T^*$ and $T_2 \setminus T^*$, through the membranes L_1 and L_2 . In mathematical terms, this meant that first the Dirichlet problem would be solved on T_1 with the boundary values u_1 on L_0 and a constant, k on L_2 . The solution is a harmonic function inside T_1 , u_1 , say. Schwarz then solved the Dirichlet problem for the region T_2 for a function which took the same values as the function u on L_3 and the values of u_1 on L_1 . He then treated the region T_1 as he had just treated T_2 to

obtain a harmonic function u_3 , then turned to T_2 again to obtain a harmonic function u_4 , and so on.

Consider now the maximum and minimum values of the function which is u on $L_0 \cup L_3$; let them be g and k , respectively, and define $G = g - k$. On L_2 the maximum value of the function $u_2 - u_1$ is less than $g - k$.

Next, since $u_3 - u_1$ is never negative inside T_1 , its maximum value is less than G , and its maximum value on L_1 is less than Gq_1 , where $q_1 < 1$.

Similarly, the maximum absolute value of $u_4 - u_2$ on L_1 is less than Gq_1 and on L_2 is less than Gq_1q_2 , where $q_2 < 1$.

Continuing in this way, Schwarz obtained two sequences of functions, $\{u_{2i-1}\}$ defined on T_1 and $\{u_{2i}\}$ defined on T_2 , with the properties that along L_1 $u_{2i-1} = u_{2i}$ and along L_2 $u_{2i+1} = u_{2i}$. He then defined two new functions

$$\begin{aligned} u' &= u_1 + (u_3 - u_1) + (u_5 - u_3) + \dots + (u_{2i+1} - u_{2i-1}) + \dots, \\ u'' &= u_2 + (u_4 - u_2) + (u_6 - u_4) + \dots + (u_{2i+2} - u_{2i}) + \dots. \end{aligned}$$

These series converge unconditionally because successive terms diminish faster than a geometric progression with ratio q_1q_2 . The function u' is harmonic inside T_1 , and the function u'' is harmonic inside T_2 , and they agree on both L_1 and L_2 , that is to say on the entire boundary of T^* . But that means that the functions u' and u'' agree on T^* , and so they define a harmonic function on the whole of $T_1 \cup T_2$, that has the prescribed boundary values of the function u on the boundary $L_0 \cup L_3$. So the Dirichlet problem is solved for the union of the two regions, which is what Schwarz set out to do.

7.7.2 Neumann's and Harnack's Contributions

In 1884 C.A. Neumann published the second edition of his 1865 book of lectures on Riemann's theory of Abelian integrals.⁴⁰ In it he directed attention back to one of the Riemann's own principal uses of his mapping theorem: to establish the existence of Abelian integrals on a Riemann surface. Neumann revised Riemann's account of how a simply connected surface is obtained from a given Riemann surface of genus g by cutting it open along $2g$ curves. He then showed how his own earlier approach to the Riemann mapping theorem in terms of an iterative process based on potential theory could be used to establish the existence of a complex function on which jumped by a constant across each cut and for which the real part of each jump was prescribed. Because the curves may be chosen at will, this extension of Riemann's theorem did not have to confront difficult questions about the nature of the boundary, and later readers, even if accepting its validity rather too readily, were right to locate

⁴⁰This section follows (Gray 1994).

it along side Schwarz's work. For Schwarz's alternating method left the nature of the boundary vague. It was to be made up piecewise by analytic arcs crossing transversally, but Schwarz did not investigate the kind of curves that could arise in this way. On the other hand, Carathéodory was to praise Schwarz in Schwarz's *Festschrift* volume (Carathéodory 1914, 20) for separating out the interior part and the boundary part of the Riemann mapping theorem. Poincaré's method of sweeping out (Poincaré 1890c) similarly made certain simplifying assumptions about the boundary but left the extent of the method unresolved. In the next few years attention was to be directed first to finding more rigorous proofs of the Riemann mapping theorem valid for any boundary, and second to the nature of the boundary itself. So for example, in a note (1891) Painlevé showed that it was enough to insist that the boundary have an everywhere continuously varying tangent.

Potential theory was much studied in the 1880s. The mathematician who first published a satisfactory method for solving a suitable version of Dirichlet's problem was Harnack. In this book (1887) Harnack first reviewed the attempts by Schwarz and Neumann to solve the Dirichlet problem for a variety of domains and noted that these authors had not fully studied the nature of the boundary. He admitted that he, too, had been unable to extend their methods, and so he had turned to a different approach using Green's functions. While this method could be applied to two- and three-dimensional problems alike, nonetheless Harnack found that some planar questions could be resolved by the theory of conformal mappings, and so in his book he confined his attention to potential theory in two dimensions. He gave a thorough account of the existence theory for functions with prescribed singularities, derived the general theorems in Riemann's paper on Abelian functions, and showed how his ideas led to a proof of the Riemann mapping theorem.

Harnack's book was generally cited favourably by subsequent mathematicians, usually for what has become known as Harnack's theorem (Harnack 1887, 67). This involves a sequence of harmonic functions u_n defined on a surface F . Each function u_n restricts to a continuous function U_n on the boundary of F . Harnack assumed that a uniform continuity held: for every arbitrarily small δ and for every point s of the boundary there was a finite domain which contained interior points of F and was partly bounded by a piece of the boundary of F containing s such that the values each function u_n took on this domain (including its boundary) varied by less than δ . For this, he remarked, it is necessary that the functions U_n be continuous. A preliminary theorem then asserted that: if the sum $U = \sum U_n$ converges uniformly, then the sum $u = \sum u_n$ converges at every interior point of the surface F to a harmonic function. From this Harnack's theorem followed: If a sequence of harmonic functions u_n all have the same sign (say, positive) and the sum $u = \sum u_n$ converges at an interior point of the surface F , then it converges at every interior point of the surface F to a harmonic function. Alternatively, if a sequence of harmonic functions u_n tend from below to the values of a function u , then u is a harmonic function.

Harnack explained more carefully than any previous author what it is for a domain to be connected: any two points can be joined by a finite polygonal arc which can be covered by overlapping discs all lying in the interior of the domain. Later authors were to tease apart the concepts of domain (every point

has a disc-like neighbourhood lying entirely in the domain) and connectedness (here, path-connectedness). Boundary points were then defined as those points every neighbourhood of which contained some points belonging to the domain and some that did not. Harnack then claimed that the boundary of a simply connected domain has a continuous boundary, although the boundary may be nowhere differentiable and may have corners and cusps. Implicitly it need not even be rectifiable. One could even add to a boundary an arbitrary number of incisions, lines drawn inwards from boundary points, which would be traversed twice by any circuit of the boundary.

On the basis of his theorem and this analysis of the boundary Harnack then established the existence of a Green's function for every bounded region with an arbitrary boundary. He accepted that Neumann's approach established the existence of a unique harmonic function agreeing with a given continuous function on the boundary for polygonal regions with no re-entrant angles. If the given function on the boundary is always finite but has isolated jump discontinuities, then there is still a harmonic function agreeing with the given one at points on the boundary where the given function is continuous. Harnack then used an approximative argument which he attributed ultimately to Schwarz to deal with the general simply connected domain, in which the domain is steadily approximated by polygonal regions. He used his theorem to establish that the sequence of harmonic functions converged to a harmonic function on the given domain. He then patched together arbitrary bounded domains out of simply connected pieces. To prove the Riemann mapping theorem (and its extension to non-simply connected domains) Harnack then used his Green's function approach to establish the existence of a suitable harmonic function and thence a complex function mapping the given bounded domain onto a circle (if simply connected) or a domain bounded by several circles (if not).

7.7.3 Poincaré's Approach

Poincaré developed an interest in potential theory in the late 1880s when he took up the broad theme of mathematical physics. His first major paper in this area was (Poincaré 1890c). He later explained what motivated him in an analysis of his own scientific work in a memoir written in 1901 (published posthumously in *Acta mathematica* in 1921, see (Poincaré 1921a): in theoretical physics it is important to get the physics right, while in applied mathematics it is important to get the mathematics right. More soberly, in applied mathematics it is often the author's intention not to find new results but to find a valid way of getting known results. In particular, he suggested that three particular partial differential equations involving the Laplacian were of great importance for mathematical physics:

$$\Delta u = ku, \quad \Delta u = k \frac{\partial u}{\partial t}, \quad \Delta u = k \frac{\partial^2 u}{\partial t^2}.$$

It was usual, he said, to rely on more-or-less intuitive proofs of the existence of solutions to these differential equations, based on an appeal to Dirichlet's principle, and he admitted that he did this when lecturing on the subject. These observations, he said, although without value for the mathematician, are of the right sort to satisfy a physicist because they leave the mechanism of the phenomena apparent. However, more rigorous arguments for the existence of solutions depended on convergence arguments: those of Schwarz, Neumann, and his own "method of sweeping out", which we shall look at shortly. However, this convergence was usually too slow, and the approximations involved too complicated for such approaches to yield effective numerical procedures.

In his (1890c) he had spelled out these views at greater length. One could not be content with the lack of a rigorous proof, he said; analysis itself should be able to solve such problems. Any rigorous solution is, of course, a solution, and even if crude nonetheless teaches us something. He asked if it was not needlessly pedantic to seek the rigorous solution of equations that had only been established by approximate methods and which rested on imprecise experimental foundations? His answer was "no": how could one be sure that something less than a rigorous proof was not actually flawed; had one the right to say that something inadequate for mathematics was yet good enough for physics?—the line was impossible to draw. One could not, as a mathematician, settle for less, and in any case many of these equations had applications not only in physics but also in pure mathematics (for example, he observed, Riemann himself had based his magnificent theory of Abelian functions on his use of Dirichlet's principle).

7.7.3.1 Sweeping Out

Poincaré gave an account of his method of sweeping out in his (1890c). The American mathematician Oliver D. Kellogg gave this accurate account of it in his book *Foundations of potential theory* (Kellogg 1929, 283–284) which is easier to follow. Here R denotes a three-dimensional region, but the argument works just as well in two dimensions. It is beyond the scope of this book to discuss the difficulties that present themselves in three or more dimensions that do not arise in dimension 2.

Instead of building up the solution from functions which are harmonic in R and do not take the right boundary values, Poincaré builds a succession of functions which are not harmonic in R , but do take on the right boundary values, the functions becoming more and more nearly harmonic. Briefly, the process is as follows. He shows first that if the problem can be solved when the boundary values are those of any polynomial in x, y, z , it can be solved for the boundary values of any function continuous in R .

The problem is then to solve the Dirichlet problem for the boundary values of a polynomial f . This polynomial is, in R , the potential of a distribution of density

$$\kappa = -\frac{1}{4\pi} \nabla^2 f$$

(in two dimensions replace the 4π by 2π) in R , plus certain surface potentials. An infinite succession of spheres is then formed, so that every interior point of R is interior to some

sphere of the set. In the first sphere, f is replaced by the harmonic function with the same boundary values on the sphere, a thing which is possible because the solution of the Dirichlet problem for spheres is known. Call the function, thus defined in the first sphere, but equal to f elsewhere in R , W_1 . W_1 is then replaced in the second sphere by the harmonic function with the same boundary values as W_1 on the sphere, and the new function, elsewhere equal to W_1 , is called W_2 . The process is called sweeping out, because in each sphere after such a process, the Laplacian becomes 0, so that there are no masses in the sphere. But the sweeping out may sweep masses into an intersecting sphere already clean. Accordingly, after the second sphere is swept out, the first is swept again, and so on, in the order 1, 2, 1, 2, 3, 1, 2, 3, 4, 1, 2, 3, 4, 5, $\dots$, so that each sphere is swept infinitely often. It is shown that the process gradually sweeps the masses toward the boundary, and that the sequence

$$W_1, W_2, W_3, \dots$$

always keeping the right boundary values, converges to a function which is harmonic within R . This is the idea of the method.

Poincaré's paper of 1890 was the first of three in which he used the Dirichlet principle as a heuristic for exploring problems and then sketched novel approaches that should lead to his newly discovered results but without relying on the contentious principle. It is not possible here to analyse them in any detail (see [Dieudonné 1981](#)) but one can see if his work measured up to his own demands.

It must be said that in Poincaré's hands analysis got close to delivering rigour. Indeed, by proposing that the idea of a solution be weakened Poincaré was onto a better idea than he or his contemporaries realised. Poincaré had noted that it was difficult to prove the convergence of the Fourier series that represented the solution of a problem. He wrote the difference between the solution function u and the Fourier series $\sum_1^n a_i v_i$, where the functions v_i are a basis of solutions,⁴¹ as

$$R = u - \sum_1^n a_i v_i.$$

He took from Chebyshev the idea of considering the integral $S = \int R^2 d\tau$ as a measure of the error, and commented (p. 92):

We have therefore shown that the average error tends to zero, but not that R tends to zero (with increasing n). This can perhaps suffice for the moment. In fact, how can S tend to zero but R not? It must be the case that R oscillates more and more rapidly as n increases, in such a way that for n very large R takes very different values at points that are very close together. No physicist believes that if such a state exists at an initial moment it can persist. This is what leads me to be content for the moment with the preceding considerations.⁴²

The idea that the mean error may tend to zero without the series converging to a solution is also rich in significance for theoretical physics.

⁴¹We pass over here without discussion Poincaré's introduction for the first time of the idea that the heat equation can be studied as an eigenvalue and eigenvector problem: the v_i are eigenvectors and Poincaré was the first to show that there are infinitely many eigenvalues and to study the spectrum.

⁴²In fact, ([Zaremba 1899](#)) shows that Poincaré's "solution" to the problem was indeed a solution.

7.7.4 *Functional-Analytic Attempts*

In his first, pioneering paper on “functions depending on functions” (or “functions of lines”, as he called them later) [Volterra \(1887a\)](#) limited himself to state that motivations for his work lay in his aim at extending Riemann’s theory of complex functions.⁴³ This aim became more precise in several notes that he submitted to the Accademia dei Lincei between 1887 and 1888 ([Volterra 1887c](#)). In the concluding lines [Volterra \(1887c, 350\)](#) recognised that the starting point of his research was given by a theorem recently established by Poincaré in ([Poincaré 1887a](#)). There Poincaré began by observing that although “only a step was seemingly needed” to extend Cauchy’s theory of complex integration to double integrals, most of the efforts so far had failed. Even the important results that Jacobi was credited with having obtained “had been lost to science” for lack of publication.⁴⁴ Then Poincaré considered double-integrals of functions $F(\xi, \eta)$ of two complex variables and established integrability conditions in order that the double-integral does not depend on the surface of integration but only on the limiting curve of the surface. His fundamental theorem (pp. 454–458) stated that if S and S' are two oriented surfaces (in $\mathbb{C}^2$) having the same contour, and if by a “continuous deformation” the surface S transforms into S' without the function $F(\xi, \eta)$ becoming discontinuous or infinite, then its integral along the surface S equals the integral along the surface S' . In addition, “the integral along a closed surface S depends only on the singular curves that lay in the interior of S ”. Thus, [Volterra \(1889a, 365\)](#) commented, “the integration of the functions of two variables leads to functions of lines”.

As [Vesentini \(1992, 243\)](#) has observed, Poincaré’s result “constitutes a model for the development of all [Volterra’s] theory”. Accordingly, in his notes Volterra introduced pairs of “functions of lines”, established an existence theorem for them with considerations analogous to the principle of Dirichlet (1887c, 339), proved an integral theorem analogous to Cauchy’s and extended to his “functions of lines” the theorem of Morera.⁴⁵

Inspired by Volterra’s papers, in 1889 Arzelà observed that Volterra had defined continuity of his “functions of lines” but avoided dealing with the problem of the existence of maxima and minima for such objects. Before facing this question Arzelà observed that the matter seemed of interest since in his view a suitable

⁴³Volterra’s first contribution to Riemann’s approach to function theory was his (1882), a paper that he wrote when he was still a student. Schwarz gave this paper a very detailed review (JFM 15.0358.02) in which he pointed to some incorrect statements and proofs.

⁴⁴([Jacobi 1830](#)) is worth mentioning in this connection. After an early reference to Cauchy’s theory of residues, there Jacobi introduced multidimensional residues (without naming them this way) as the coefficients of the powers of degree -1 in the expansions of the roots of two (resp. three) equations in two (resp. three variables) power series, and re-obtained Lagrange series as a particular case.

⁴⁵For an account in modern terms of Volterra’s work, including his (1890, 1913, 1917) and some related problems, see [Vesentini \(1992\)](#).

extension of it was “closely related to the well-known problem of the validity of the principle of Riemann–Dirichlet” (Arzelà 1889, 296). Arzelà’s paper (1889) is interesting in itself for he introduced the notion of equicontinuous family of curves there (p. 297) and showed that equicontinuity was a necessary and sufficient condition for an infinite family of curves to have a limit-curve, thus meeting a theorem just established by Ascoli (1884) to which paper he referred. Then, he considered an infinite family of equicontinuous curves in a (closed) domain C , defined a function of them (that is, in modern terms a functional), and he established the existence of a minimum and a maximum for it under the condition of equicontinuity. This, he observed, generalised Weierstrass’s theorem for ordinary functions.

Some years later, in his (1895–1896) Arzelà offered an extended and improved version of his results. He considered an infinite sequence of continuous functions $u_n(x)$ defined on an interval $[a, b]$, and defined the functions $u_n(x)$ to be equicontinuous “if for any given arbitrarily small σ there exist a positive number δ such that in every interval smaller than δ the oscillation of the functions $u_n(x)$ is less than σ ” (Arzelà 1895–1896, 330). Then he showed that the existence of a continuous function that is limit-function for the sequence $u_n(x)$ implies the equicontinuity of the infinitely many functions of the sequence, and vice versa.⁴⁶

As he had promised, these results enabled him to tackle the proof of Dirichlet principle. In his (1896–1897) he succeeded in proving the existence of a function, belonging to a suitable family, which minimises Dirichlet’s integral, and satisfies Laplace’s equation under suitable conditions on the derivatives. However he was unable to prove that the family can be so determined that these conditions are satisfied. Although inconclusive, Arzelà’s attempt is important for he offered an insight that the new functional methods could provide a proof of this lengthily debated principle. Independently of him, this was actually done by Hilbert some years later.

7.7.5 Hilbert’s Approach

Poincaré’s only rival for the title of the greatest mathematician of his time was the younger figure of David Hilbert. Hilbert had been born and educated in Königsberg, and after a good, but not remarkable, career as a student had suddenly shot to prominence in the mid-1880s as an algebraist who utterly transformed a difficult technical subject known as invariant theory. He then turned to algebraic number theory—which was something of a German speciality—and in 1897 published what became the definitive introduction to the subject at the research level for a generation, a massive and innovative work of synthesis called the *Zahlbericht* (Hilbert 1897a).

⁴⁶We will see in (Sect. 8.6) how Arzelà’s work connected to Montel’s concept of normal family of functions.

He then astonished his colleagues by taking up elementary geometry, using it as the opportunity to revive and greatly increase the value of axiomatic thinking, which he demonstrated to great effect in his *Grundlagen der Geometrie* (Hilbert 1899).

It was only natural, therefore, that he was invited to be one of the keynote speakers at the second International Congress of Mathematicians, to be held in Paris in August 1900. His close friend Minkowski urged him to seize the day, writing to him:

Most alluring would be the attempt to look into the future, in other words, a characterisation of the problems to which the mathematicians should turn in the future. With this, you might conceivably have people talking about your speech even decades from now. Of course, prophecy is indeed a difficult thing⁴⁷

and this, after some shilly-shallying, is what Hilbert did. He did not describe his own work. Rather he presented two fruitfully contrasting views on mathematics—mathematics for its own sake and mathematics in the service of science—suggested that high standards of rigour and understanding brought these strands harmoniously together, and then offered some 23 problems, several in several parts, as good problems to work on. The resulting address and its publication slowly became one of the decisive events of the twentieth century in mathematics, as mathematicians felt especially honoured when they solved one of the problems (see Gray 2000a and Yandell 2002). Few remain unsolved today, one of those that does is the Riemann hypothesis.

Among the problems Hilbert singled out was the Dirichlet problem and the Dirichlet principle. As he put it (note that by a regular problem he means one such as the Dirichlet problem):

20. THE GENERAL PROBLEM OF BOUNDARY VALUES.

An important problem closely connected with the foregoing is the question concerning the existence of solutions of partial differential equations when the values on the boundary of the region are prescribed. This problem is solved in the main by the keen methods of H. A. Schwarz, C. Neumann, and Poincaré for the differential equation of the potential. These methods, however, seem to be generally not capable of direct extension to the case where along the boundary there are prescribed either the differential coefficients or any relations between these and the values of the function. Nor can they be extended immediately to the case where the inquiry is not for potential surfaces but, say, for surfaces of least area, or surfaces of constant positive Gaussian curvature, which are to pass through a prescribed twisted curve or to stretch over a given ring surface. It is my conviction that it will be possible to prove these existence theorems by means of a general principle whose nature is indicated by Dirichlet's principle. This general principle will then perhaps enable us to approach the question: *Has not every regular variation problem a solution, provided certain assumptions regarding the given boundary conditions are satisfied* (say that the functions concerned in these boundary conditions are continuous and have in sections one or more derivatives), *and provided also if need be that the notion of a solution shall be suitably extended?* (Hilbert 1901, 321–322)

⁴⁷See Rüdénberg and Zassenhaus (1973, 119) for the letter from Minkowski to Hilbert, 5 January 1900.

Hilbert's question was repeated as an actual statement in his own lecture on Dirichlet's principle (Hilbert 1900, 11), later re-published as (Hilbert 1905). His idea was to prove the existence of a solution of such variation problems in a direct way. He illustrated it with two examples: the first one was to determine the geodesic joining two points of a given surface. The second one was "to find a potential function $z = f(x, y)$ that takes given boundary values on a given boundary curve of the x, y -plane" (Hilbert 1900, 12).

"For the sake of simplicity" Hilbert assumed that the boundary curve had continuous tangents and curvature, and that the boundary values were given by a function with continuous derivative. His reasoning can be summarised as follows: consider the admissible functions $F_n(x, y)$ and their Dirichlet integrals $J(F_n)$, and let j be the greatest lower bound of these integrals. Then, among the admissible functions $F_n(x, y)$ search for a minimising sequence $f_n(x, y)$ such that

$$\lim_{n \rightarrow \infty} J(f_n(x, y)) = j.$$

Given this, Hilbert proved that there exists a subsequence $\varphi_n(x, y)$ of the sequence $f_n(x, y)$ that is uniformly convergent to a function $\varphi(x, y)$ for all interior and boundary values of (x, y) . This means that $\varphi(x, y)$ is continuous, and in fact it is the sought-for potential function. "The proof of this offers no difficulty", Hilbert (1900, 14) concluded. His proof, he added, only utilised a minimum property—a property of compactness, we would say in modern terms—and therefore this "method of proof can be applied to more general problems of the theory of surfaces and of mathematical physics".

Commenting on Dirichlet principle many years later Volterra wrote: "In 1900 Hilbert gave a complete and rigorous proof, thus obtaining a definitive result that Arzelà had not attained. But it was by following the route the former had opened that Hilbert could succeed" (in Volterra and Pèrès 1936, 110). Volterra is likely to have gone too far in his claim. For, even though Hilbert's approach reminds one of Arzelà's, it seems very unlikely that he knew of the latter's work.

Hilbert summarised the matter in a lecture he gave in 1901 on the occasion of the 150th anniversary of the Göttingen *Gesellschaft der Wissenschaften*.⁴⁸ Because of its "methodological interest" he offered a detailed proof of the existence of a harmonic function of a Riemann surface satisfying prescribed singularities.

Hilbert's bold idea, not entirely established by him even in the later paper, was to bridge the gap between mathematicians and physicists by delineating a class of boundary conditions which were general enough to cover the physical situations and yet narrow enough to finesse the mathematical difficulties. If pressed, Hilbert admitted, one might retreat a little on the idea of what was meant by a solution (here, recall some of Poincaré's remarks in Sect. 7.7.3). Hilbert pointed to a large class of equations, containing several that arise in various physical problems, and asked for

⁴⁸An extended version of it was later published as (Hilbert 1904).

theorems guaranteeing the existence of solutions, perhaps in some attenuated sense to be made precise by future research. It is fair to say that much research in partial differential equations to this day follows that agenda.

7.8 Minimal Surfaces

In his ICM 1900 lecture in Paris quoted above Hilbert expressed the conviction that the “general principle” he stated there could fruitfully applied to prove existence theorems for surfaces of least area.

The study of minimal surfaces may be said to have begun when Euler showed that among all plane curves of a given length between two points the catenary is the one that generates the surface of revolution of minimal area (Euler 1744). Not long afterwards, Lagrange showed that among surfaces that are graphs of functions $z = z(x, y)$ and have a given perimeter the one which has the least area is a solution of the Euler–Lagrange equation for the area functional. The equation, which he did not write down explicitly, is

$$\frac{\partial}{\partial x} \left(\frac{p}{W} \right) + \frac{\partial}{\partial y} \left(\frac{q}{W} \right) = 0, \quad (7.18)$$

where

$$p = \frac{\partial z}{\partial x}, \quad q = \frac{\partial z}{\partial y}, \quad \text{and } W = \sqrt{1 + p^2 + q^2}.$$

It is an elliptic partial differential equation, and it resembles Laplace’s equation when the coefficients p and q are small, so a connection with harmonic functions is only to be expected, although it was to be some decades before it was discovered.

The next significant contribution was that of Meusnier, who followed earlier work of Euler, which he may not have known. Euler had considered the curves obtained by passing planes through the normal to a surface at a point. He found the resulting formulae rather daunting, but he was able to show that generally there are two directions at which the plane sections have their greatest and least curvatures (as measured by their signed radii of curvature, the radii of the best approximating circles). Moreover, these extreme curves, which he called the principal curves, are at right angles. The average of the corresponding curvatures is called the mean curvature of the surface. In his (1785) written when he was only 18, Meusnier was able to show quite easily that a minimal surface—defined as one that satisfies the partial differential equation laid down by Lagrange—has zero mean curvature everywhere. Meusnier also showed that the catenoid and the helicoid are minimal surfaces.

There, however, matters rested for many years, despite the attention of Legendre, Monge, Scherk, and others, who discovered a handful of other minimal surfaces. The significant breakthrough came indirectly in the work of Gauss, who took up and eventually transformed the whole of differential geometry in the 1820s.

7.8.1 Gauss's Essays on Differential Geometry

Gauss had been drawn into the survey of Denmark that Frederick VI, King of Denmark, had commissioned in 1816. The man appointed to head the survey was Heinrich Christian Schumacher, the director of the observatory in Altona, who had studied astronomy under Gauss in 1808–1809 and become a life-long friend of his.⁴⁹ Gauss initially refused to work on the Danish survey, but in 1819 Schumacher persuaded Joseph Banks, the President of the Royal Society of London, to persuade the Hanoverians that now was the time to survey of their estates, which border on Denmark, and Gauss could not decline the resulting request. The survey of Danish and Hanoverian lands began in 1820. Lower Saxony was covered in the years 1821–1823, and the survey was then extended intermittently until 1827, when Gauss withdrew, after which it staggered on for a few more years.

In 1820, under Schumacher's influence, the Copenhagen Academy of Sciences sponsored a prize on conformal mappings, but no entries for the prize were submitted by the original closing date, so it was re-advertised in 1822. In June Gauss wrote to Schumacher regretting that the prize had had to be re-advertised but “the practical measurement work has taken so long that naturally I cannot think at all of working out subtle theoretical details”. The survey stopped, however, in the autumn with the onset of poor weather, and Gauss was able to send Schumacher his essay for the prize in December 1822. In July 1823 Gauss was informed officially that he had won the prize, and in 1825 his prize-winning essay was published in the *Astronomische Abhandlungen*, the journal edited by Schumacher alongside his *Astronomische Nachrichten* as (Gauss 1825). Gauss, who was short of money at the time, asked that the medal be converted to a cash award.

The central result of Gauss's essay is that any (smooth) surface can be mapped conformally onto the plane. All previous work on conformal maps had been in geodesy and had dealt with maps of the surface Earth onto a plane (see Sect. 2.2.3). Gauss transferred the study of conformal maps to the domain of differential geometry. First of all, he focussed attention on patches of a surface, defined by triples of functions of the form $(x(u, v), y(u, v), z(u, v))$, where u and v are coordinates on a piece of the usual plane. Such patches need not be graphs of a function, but more importantly Gauss viewed a map from one surface to another as essentially a map from a patch on one surface to a patch on the image surface. Each surface inherits a metric from the ambient space—Gauss went some way to thinking of surfaces as two-dimensional objects, but always regarded them as surfaces in $\mathbb{R}^3$. Gauss could therefore ask if the given map was conformal and answer it by looking at the effect it had on the metrics.

A metric on a surface in space is something the form

$$ds^2 = E(u, v)du^2 + 2F(u, v)dudv + G(u, v)dv^2,$$

⁴⁹The edition of their correspondence (Gauss 1860–1865) runs to six volumes.

where u and v are local coordinates, where, with the usual notation for partial derivatives,

$$E(u, v) = x_u^2 + y_u^2 + z_u^2, F(u, v) = x_u x_v + y_u y_v + z_u z_v, \text{ and } G(u, v) = x_v^2 + y_v^2 + z_v^2.$$

If the coordinates on the image surface are u' and v' , there are analogous expressions for the metric there, which can be denoted as ds'^2 . If a map between the surfaces is given, then the metric on the image surface can be compared with the image of the metric on the domain surface by finding ds^2 in terms of u' and v' . This will be an expression of the form $ds^2 = e(u', v') du'^2 + 2f(u', v') du' dv' + g(u', v') dv'^2$, and it expresses how the original (u, v) grid has been distorted when it is mapped onto the image surface. The map is conformal at a point if it is a scaling and a rotation there, which reduces to the claim that

$$e : f : g = E : F : G.$$

The great advance Gauss made in the theory of conformal mappings was to show that the question of conformal maps between surfaces can be reduced to the question of conformal maps between the coordinate domains, the (u, v) and (u', v') domains in $\mathbb{R}^2$. As Stäckel pointed out in his essay “Gauss als Geometer”, Gauss showed⁵⁰ that a metric on a surface can always be written in the form

$$ds^2 = \psi(\lambda, \mu) (d\lambda^2 + d\mu^2).$$

It follows easily with this simplification of the metric that any curved surface can be mapped conformally onto the plane.

Three years later, in 1828, Gauss published his remarkable theory of the differential geometry of surfaces, the *Disquisitiones generales circa superficies curvas*. In it, Gauss defined a concept of curvature of a surface and showed that it was intrinsic to the surface. To define curvature at a point on a surface, Gauss considered the surface embedded in $\mathbb{R}^3$. He then defined a map, later called the Gauss map, from the surface to the unit sphere by associating with each point P the direction of the normal at P by supposing that normal is moved parallel to itself until it is based at the centre of the unit sphere. The curvature at the point P he then defined as the limit

$$\lim_{S \rightarrow P} \frac{\text{area of } S'}{\text{area of } S},$$

where S is a small region about the point P , and S' is its image under the Gauss map. The plane has Gaussian curvature zero because all its normals point in the same direction and so the Gauss map sends the entire plane to a point. The curvature of a circular cylinder is also zero, because the Gauss map sends the entire cylinder to a

⁵⁰By the standards of his day, but see Gray and Micallef (to appear).

line. The curvature of a sphere of radius R is $\frac{1}{R^2}$, because distances on the domain sphere are scaled by a factor of $\frac{1}{R}$ by the Gauss map. Saddle-shaped regions have negative curvature.

The curvature is said to be intrinsic because it can be defined entirely without reference to any ambient space in which the surface lived (such as, say, a normal to the surface implies). As Gauss put it: if one surface can be mapped isometrically onto another, then the values of the curvature agree at corresponding points. This was an entirely novel and unexpected idea (Gauss called the corresponding theorem the exceptional theorem, *Theorema egregium*), and it gradually transformed the subject of differential geometry.

Gauss also showed that the value of the curvature at a point was always the product of the extremal radii of curvature at that point. In particular, for a minimal surface, writing K for the Gaussian curvature and $\pm k$ for the radii of curvature at a point, one has $K = -k^2$.

7.8.2 Minding's Families of Curves

Gauss seems not to have been interested in the topic of minimal surfaces, but those who came after him used his ideas in ways that were eventually to infuse the subject with the new ideas of complex function theory. Minding (1852) defined two families of curves on a surface given as a graph with an equation of the form $z = z(x, y)$, where, as was customary, he set $p = \frac{\partial z}{\partial x}$ and $q = \frac{\partial z}{\partial y}$. The first family is defined by the equation $p = kq$ for some constant k and the second by the equation $p^2 + q^2 = C$, where C is a constant. Minding understood these curves by looking at the surface normals along them (which amounts to considering their images under the Gauss map, although Minding did not use that expression, which did not come into use until much later). The unit normal at the point (x, y, z) on the surface is $\frac{(-p, -q, 1)}{\sqrt{1+p^2+q^2}}$.

So the surface normals along the first family of curves are all parallel to the plane $x = ky$, which makes their images under the Gauss map meridians or longitudes on the sphere. Similarly, the second family of curves turn out to be those of constant height on the sphere: the curves of latitude. Minding called these two families curves on the surface meridians and curves of constant pole height, because they are the pre-images under the Gauss map of the longitudes and latitudes on the sphere.

Minding now investigated when these pre-images are orthogonal, because, of course, longitudes and latitudes are orthogonal on the sphere. He excluded the developable surfaces from consideration, because the Gauss map of such a surface maps it onto a curve.⁵¹ He differentiated the first condition to obtain the condition $pdq = qdp$, and differentiated the second condition to obtain $pdp + qdq = 0$ and so

⁵¹ A surface is said to be developable if there are curves on it along which the tangent spaces have a line in common.

found conditions that the tangent vectors to these curves must satisfy. After a short calculation he found that the curves on the surface are orthogonal if and only if

$$(r+t+(pt-qs)p+(qr-ps)q)((p^2-q^2)s+pq(t-r))=0,$$

where $r = \frac{\partial^2 z}{\partial x^2}$, $s = \frac{\partial^2 z}{\partial x \partial y}$, and $t = \frac{\partial^2 z}{\partial y^2}$. The condition

$$(p^2-q^2)s+pq(t-r)=0$$

is satisfied by surfaces of revolution, and on such surfaces it is clear that the meridians and curves of constant pole height meet at right angles, and it is easy to see that the Gauss map is not conformal on a surface of revolution that is not also a minimal surface.⁵² The remaining condition,

$$r+t+(pt-qs)p+(qr-ps)q=0,$$

rearranges to become

$$(1+p^2)t+(1+q^2)r-2pqs=0$$

which, as [Minding \(1852, 70\)](#) said, “has long been known to be the equation of a minimal surface with a given boundary”. He concluded that on a minimal surface the two families of curves he had defined are orthogonal, but he did not go further and show that on a minimal surface the Gauss map is conformal.

7.8.3 Bonnet’s “Surfaces of Minimum Area”

Gauss’s approach to differential geometry was set before a French audience by Liouville in a series of appendices to his re-edition of Monge’s *Applications de l’analyse* in 1850 (see [Monge 1850](#), 547–638). This book had an immediate effect on Ossian Bonnet, whose conclusions closely resembles Minding’s. In his (1851 and 1860) Bonnet considered a grid on a surface, defined by the lines of curvature on the surface, these are the two parameter families of curves through each point whose radii of curvature are the extremal values. He showed that this grid on a minimal surface is mapped by the Gauss map onto a grid on the sphere that maps under stereographic projection to a grid in the plane composed of two families of orthogonal straight lines. It followed, by Gauss’s paper of 1825 on conformal mappings, that the Gauss map of a minimal surface was conformal.⁵³

⁵²The only minimal surface that is a surface of revolution is the catenoid.

⁵³[Weingarten \(1863\)](#) obtained similar results.

In his lengthy paper (1860) Bonnet defined spatial coordinates x , y , and z in such a way that the metric on the sphere is $\frac{1}{\cosh^2 y}(dx^2 + dy^2)$, which is the right form for Gauss's theorem on conformal maps. He then showed that the formula for the line element of a surface can be written in the form

$$d\sigma^2 = (u^2 + v^2) dx^2 + 2v(u + w) dx dy + (v^2 + w^2) dy^2,$$

where u and v are functions of x , y , and z ,⁵⁴ in such a way that the equation $v = 0$ defines surfaces for which one family of lines of curvature lie in parallel planes (for example, surfaces of revolution) and the condition $u + w = 0$ defines surfaces of minimum area (to use Bonnet's term). For these, therefore, the line element takes the form:

$$d\sigma^2 = (v^2 + w^2)(dx^2 + dy^2).$$

For these two classes of surface, and only these, the meridians and parallels meet at right angles. Bonnet's argument was that the radii of curvature at a point on the surface satisfy the equation

$$R^2 - (u + w) \cosh(y)R + (uw - v^2) \cosh^2(y) = 0,$$

a fact known earlier to Monge. Therefore the condition $u + w = 0$ is the condition that the mean curvature of the surface vanishes or, in other words, that the surface is a minimal surface.

In Bonnet's papers complex conjugates played a ubiquitous role in the handling of the equations, but no connections were made to the subject of complex function theory. The same was true of Enneper's. Enneper's (1864) displays the array of formulae that describe the main features of the differential geometry of surfaces in terms of coordinates u and v on the surface and ambient coordinates x , y , and z in $\mathbb{R}^3$. Enneper then studied what can be said under various special hypotheses, one of which was the assumption that the sum of the principal curvatures at each point is a constant, which he then further specialised to the case where this sum is zero. He did not say at this point that this makes the surface a minimal surface; he did, however, show how to follow Liouville and obtain the general solution of the minimal surface equation as

$$\frac{1}{t} = 4 \frac{\phi'(u + iv)\psi'(u - iv)}{(1 + \phi(u + iv)\psi(u - iv))^2},$$

where the radii of curvature at each point are t and $-t$, and ϕ and ψ are arbitrary functions. From this equation Enneper deduced that

$$\frac{\partial^2 x}{\partial u^2} + \frac{\partial^2 x}{\partial v^2} = 0,$$

⁵⁴We omit his complicated definitions, see Gray and Micallef (to appear).

but he did not say in words that this established that the coordinate functions x , y , and z are harmonic. After four more pages of work he deduced that in terms of coordinates $p = u + iv$ and $q = u - iv$ and arbitrary functions $\phi(p) = p_1(u) = \Phi(p) + i\Psi(p)$ and $\psi(q) = q_1(q) = \Phi(q) - i\Psi(q)$, the coordinates for the surface in question can now be written as (Enneper 1864, 107, equations 19)

$$\begin{aligned}x &= \int \frac{p_1^2 - 1}{\frac{\partial p_1}{\partial p}} dp + \int \frac{q_1^2 - 1}{\frac{\partial q_1}{\partial q}} dq \\y &= i \int \frac{p_1^2 + 1}{\frac{\partial p_1}{\partial p}} dp - i \int \frac{q_1^2 + 1}{\frac{\partial q_1}{\partial q}} dq \\z &= \int \frac{p_1}{\frac{\partial p_1}{\partial p}} dp + \int \frac{q_1}{\frac{\partial q_1}{\partial q}} dq.\end{aligned}$$

These equations are today known as Enneper's equations but we should note that considerations of complex function theory are entirely absent from his paper. Enneper did, however, proceed to exhibit attractive new solutions to this minimal surface problem by choosing simple expressions for p_1 and q_1 , including an algebraic surface that today bears his name⁵⁵ (Fig. 7.3) and a new derivation of Scherk's surface.

Bonnet's paper established that harmonic functions and conformal maps played a significant role in the study of minimal surfaces, but neither he nor Minding before him took the step of thinking in terms of complex analytic maps, which suggests that even in 1860 such a connection might not have been generally appreciated. Instead, the first to put the picture together that way were the two leading complex analysts of the day, Riemann and Weierstrass. They worked independently. Riemann's account was entrusted by him to Hattendorff for editing in April 1866, but apparently dates from 1860 to 1861. The original manuscript consists purely of formulae, and Hattendorff supplied a text; the result was published in 1867 as (Riemann 1867). Weierstrass's account was first given in a lecture at the Berlin Akademie in 1866.⁵⁶ Weierstrass was also the first to give a general account of algebraic minimal surfaces (Weierstrass 1867b).

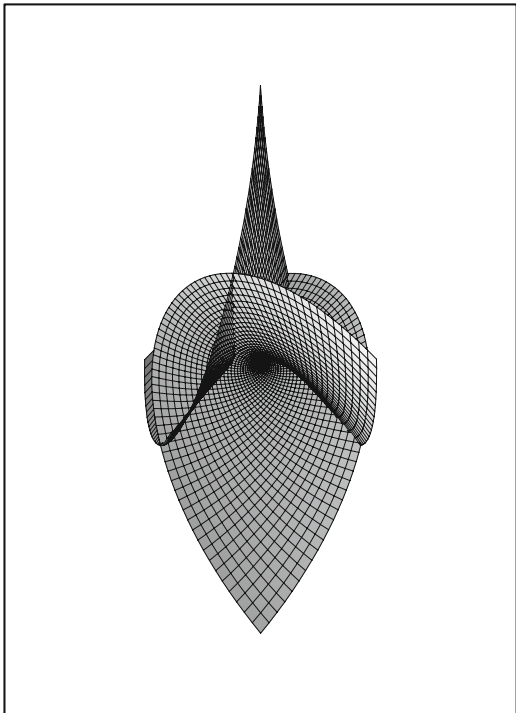
7.8.4 Riemann's Approach to the Plateau Problem

Riemann's approach was to see what happened to the Gauss map when one imposed geometrically meaningful conditions on the domain surface. He defined a piece of

⁵⁵See Enneper (1864, 108). For a picture of it and an instructive commentary, see Fischer (1986).

⁵⁶Weierstrass (1866a) and two papers (Weierstrass 1866b) and 1867a) only published for the first time in his *Werke*, 3.

Fig. 7.3 Enneper's surface
(Jeremy Gray, Maple output)



surface by a map from a patch of $\mathbb{R}^2$ in (p, q) -coordinates into $\mathbb{R}^3$ in the usual (x, y, z) -coordinates, then mapped the surface onto the unit sphere by the Gauss map. The areas of infinitesimal pieces of the surface in $\mathbb{R}^3$ and its image on the sphere are related by the Jacobian of the Gauss map. The area of the entire surface is then given by a double integral, and the condition that the surface be a minimal surface is that the first variation of that integral vanishes. Riemann showed that this condition reduced to the statement that a certain differential form was exact:

$$-\sin r \sin \phi dy + \sin r \cos \phi dz,$$

where r and ϕ are polar coordinates on the sphere, and the pole is the point $(1, 0, 0)$. Note that on the sphere

$$\cos r dx + \sin r \cos \phi dy + \sin r \sin \phi dz = 0.$$

The polar coordinates (r, ϕ) are polar coordinates on the plane, to which the sphere has been mapped by stereographic projection from the opposite pole. Riemann now introduced complex coordinates: η in the codomain plane and $s = y + iz$ in the domain plane. In terms of these equations he wrote the first of the above equations as

$$(1 + \eta \bar{\eta}) d\xi i - \bar{\eta} ds + \eta d\bar{s} = 0$$

and the second one as

$$(1 - \eta \bar{\eta}) dx + \bar{\eta} ds + \eta d\bar{s} = 0.$$

These simplified on introducing the complex variable $X = x + i\xi/2$ to

$$ds = \eta dX - \frac{1}{\bar{\eta}} d\bar{X}$$

and

$$d\bar{s} = -\frac{1}{\eta} dX + \bar{\eta} d\bar{X}$$

Since ds and $d\bar{s}$ are to be exact differentials, it must be that

$$\frac{\partial \eta}{\partial \bar{X}} = 0 \quad \text{and} \quad \frac{\partial \bar{\eta}}{\partial X} = 0.$$

In short, Riemann remarked (Sect. 5), η must be a complex function of X . It followed (Sect. 7) that the minimal surface was represented conformally on the sphere and the plane, and Riemann went on to show (Sect. 10) that mean curvature at each point was zero.

It is in this connections that Riemann gave what may be the first statement of the reflection principle later named for Schwarz (but it must be recalled that the words in this posthumously published paper of Riemann's were written by Hattendorff). In Sect. 12 Riemann was interested in calculating a certain expression he denoted $\frac{du}{dt}$. He commented:

In order to form the expression for $\frac{du}{dt}$ we must observe that dt is always real along the boundary, and du is either real or purely imaginary. Hence $\left(\frac{du}{dt}\right)^2$ is real when t is real. This function can be continuously extended across the line of real values of t by the condition that, for conjugate values of t and $\bar{t}$ of the variable, the function will have conjugate values. Then $\left(\frac{du}{dt}\right)^2$ is determined for the whole t -plane and turns out to be single-valued.

Until the work of Enneper it was hard to exhibit any minimal surfaces at all. The much harder problem is to find a minimal surface that spans a given contour; this problem is called the Plateau problem after the blind Belgian physicist Joseph Plateau who, in the 1840s and 1850s had drawn attention to the fact that a soap film spanning a wire frame will take up the shape of a minimal surface. The contours can have quite arbitrary shapes. The disparity between mathematicians ability to solve the Plateau problem and the behaviour of soap films was a potent challenge, but even Riemann found this task of finding the right surface for a given boundary curve daunting. He was able to give explicit solutions only for simple boundaries: two skew lines in space (when the surface is a piece of a helicoid); two intersecting lines and a third lying in a plane parallel to the first two, three skew lines (which led

to a generalisation of the Riemann P -function); the space quadrilateral (later studied by Schwarz), and two circles in two parallel planes.

7.8.5 Weierstrass's Approach to Minimal Surfaces

Weierstrass's approach to the minimal surface problem in (Weierstrass 1866a and 1866b) was different. When he first tackled the subject in a communication to the Berlin Akademie he said: "I dealt with the theory of the so-called minimal surfaces especially because, as I will show, it is intimately connected with the theory of analytic functions so that any such function determines a surface of the above kind, and vice versa" (Weierstrass 1866a, 39). Accordingly, he started with the expression for the mean curvature, which he said he had known since 1861, and assumed that the given piece of surface is defined by a conformal map from a patch U of $\mathbb{R}^2$ with coordinates p and q into $\mathbb{R}^3$ with coordinates x, y, z . As was his custom, he gave no references and remarked only that it was well-known that one could do this. He then showed that x, y , and z must be harmonic functions of p and q , and so the real parts of three complex functions f, g , and h of $u = p + iq$, meromorphic on the whole of U . They are related by the differential equation which the minimal surface satisfies, which, in terms of f, g , and h , is

$$f'(u)^2 + g'(u)^2 + h'(u)^2 = 0.$$

This implies that there are functions $G(u)$ and $H(u)$ such that

$$f'(u) = G^2 - H^2, \quad g'(u) = i(G^2 + H^2), \quad h'(u) = 2GH.$$

The condition on f, g , and h forces the same condition on G and H .⁵⁷

Weierstrass then defined the complex variable s by $s = \frac{H(u)}{G(u)}$, and a technical argument, which we omit, finally led Weierstrass to the complex function $G^2(u) \frac{du}{ds}$, which he called $S = F(s)$, upon which he based his analysis. He gave explicit power series expressions for the coordinate functions of a minimal surface in terms of S and also for its Gaussian curvature. He pointed out that the principle of analytic continuation then allowed the coordinate functions to be defined for the entire minimal surface. So he could finally proclaim that to every single-valued analytic function there corresponds a surface with mean curvature everywhere zero. Moreover, the minimal surface is algebraic if and only if $S = F(s)$ is an algebraic function.

In 1867, prompted by some of Schwarz's investigations, Weierstrass gave some specific examples of solutions to Plateau's problem, but like Riemann, only for

⁵⁷Weierstrass omitted the trivial case $f' = ig', h' = 0$, which yields the plane as a minimal surface.

surfaces with very simple boundaries. However, because Weierstrass's accounts were published and taken up by his students before Riemann's posthumous account appeared, most subsequent authors credited Weierstrass with discovering the intimate connection between minimal surfaces and complex function theory.

7.8.6 Schwarz's Investigations

As we saw in Chap. 6, Schwarz was the student of Weierstrass who remained most closely associated with the master. This is particularly true of his earliest work, as one would naturally suppose. In 1865 Weierstrass gave a seminar at the University of Berlin on minimal surfaces (Schwarz 1890, 1, 174). Perhaps as part of the seminar, Schwarz took up and solved the problem of finding the minimal surface bounded by a space quadrilateral, unaware that this problem had already been solved by Riemann. Schwarz's mathematical solution was presented to the Berlin Akademie by Kummer, accompanied by a Gypsum model that Schwarz, it seems, had made himself, based on experiments with glycerine.⁵⁸

On the strictly mathematical side Schwarz's analysis made considerable use of the theories of elliptic and hyperelliptic functions, because it chose a particular hyperelliptic function for substitution into the Enneper–Weierstrass equations. This gave his surface a natural periodicity: pieces of it could be fitted together to form an annular region with two triangular boundaries, and these pieces could in turn be joined up to form an infinitely extended surface with infinite topological genus. Altogether a remarkable discovery with which to embark on a career in mathematics. See the illustrations, which are taken from Schwarz's *Abhandlungen*.

A deep insight into analytic functions underpinned this work, namely that a straight line in a minimal surface is a line of symmetry. So, if two pieces of minimal surface meet along a common line and have the same normals there then each is the analytic continuation of the other. This is the origin of the Schwarz reflection principle, which Schwarz went on to state in his (1869a, 66), and which, he wrote, he had picked up in a conversation with Weierstrass (Schwarz 1890, 1, 181).

It is amusing to note, in view of Schwarz's later reputation as a rigorous analyst, that his proof of the reflection principle is weak at the crucial point. The principle claims that if a complex function f defined on some domain D in the upper half plane together with the real axis restricts to a continuous real-valued function on (an interval in) the real axis, then extending the definition of f to the region that is mirror image of D in the real axis by defining $f(\bar{z}) = \overline{f(z)}$ produces an function analytic on the union of the two domains. It is easy to see, as Schwarz did, that this expression captures the analytic continuation from the region above the interval to the region

⁵⁸Schwarz made such models all his life, as numerous remarks in his papers attest. He brought them with him and proudly presented them to his French colleagues when he visited Paris in 1883. See also Hamel's obituary of him (Hamel 1923).

below. This gives a function analytic at all points above or below the real axis. To establish analyticity at points on the real axis, Schwarz gave a representation of the function via a Cauchy integral, but he did not explain how this establishes analyticity at points on the real axis. Modern text books usually use Morera's theorem at this stage, which, of course, was not available to Schwarz, but something has to be said, and Schwarz was silent. Nor is there any evidence in the correspondence between Schwarz and Weierstrass that his mentor pulled him up on this point. Perhaps the missing few words were obvious to them, but perhaps they nodded.

The pleasing, and useful, converse to Cauchy's integral theorem was not discovered for quite some time. It fell to the Italian mathematician Giacinto Morera⁵⁹ to discover and publish it in his (1886a). There he showed that if a continuous, finite, and single-valued complex-valued function u of a complex variable z is such that the integral taken round the boundary of any region T is zero, then, as he put it, the function is a complex function in Riemann's sense—that is, it is a holomorphic function. The proof consists of observing first that under the stated conditions the integral of the function on any path lying within T depends only on its endpoints, and then on checking that the integral is an analytic function of its upper endpoint. From this Morera deduced that the sum of a uniformly convergent series of continuous, finite, and single-valued functions is itself a continuous, finite, and single-valued function.⁶⁰ In his (1902) he returned to this theme, and showed that this property can be used to give a new definition of an analytic function. Meanwhile, and presumably independently of Morera (whom he does not cite) Osgood (1896) had proved a very similar result to the Italian's: a continuous function defined on a simply connected domain T with the property that its integral taken around any closed curve in the interior of T vanishes, is holomorphic. His proof was also very much the same.

Although Schwarz is best known as an analyst, he maintained a lifelong interest in minimal surfaces. He was the first to investigate the implications of the fact that any approach to minimal surfaces based on the calculus of variations will yield functions that are only local extrema. He showed (Schwarz 1885) that just as the equation $\frac{dy}{dx} = 0$ picks out extremal points on a curve $y = y(x)$, but one must look

⁵⁹Morera was born in Novara in 1856 and studied mathematics at the University of Turin, where he was a pupil of Siacci's. From there he went to Pisa, then Pavia, then Berlin, where he met Kronecker, and Leipzig, where he had discussions with Klein and Mayer. Most of his work was on applied topics, including the theory of harmonic functions and dynamics. He became a professor of rational mechanics at Genoa in 1885, and in 1900 he moved to the Faculty of Sciences at the University of Turin, where he held various positions culminating in the Presidency of the Faculty. Shortly before his death in 1910 he was elected a Corresponding Member of the Mathematical Society of Kharkov (see Maggi 1910 and Somigliana 1910.)

⁶⁰In response to Morera's paper, and by resorting to his theory of functions of lines Volterra (1887c, 348–350) proved a three-dimensional extension of Morera's theorem, and claimed that it could be easily extended to n -dimensional spaces. Indeed, if the integral $\int f(z_1, \dots, z_n) dz_1 \dots dz_n$ of a continuous function on the boundary of any polycylinder lying in a domain B vanishes, then the function is analytic. (See Behnke and Thullen (1934, 41)).

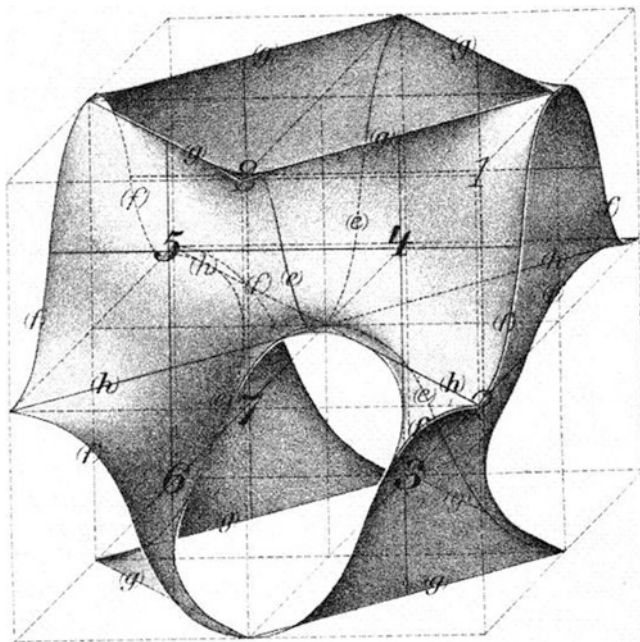


Fig. 7.4 One of Schwarz's minimal surfaces (Schwarz, *Gesammelte Mathematische Abhandlungen* vol. 1, p. after 132, Tafel 4)

at $\frac{d^2y}{dx^2}$ and sometimes even higher derivatives to discriminate between maxima, minima and inflection points, so too one must discriminate between different kinds of solution to the minimal surface equation by looking at least at their second variation.

In paper (Schwarz 1894) written after Schwarz had published his *Gesammelte Mathematische Abhandlungen*, he did return to the subject of his first memoir and establish what had seemed obvious but had escaped proof. The solution to the minimal surface equation that has a given space quadrilateral for its boundary is indeed unique. Moreover, Schwarz's method proved capable of careful generalisation to certain other boundaries made up of straight lines; Schwarz chains, as they are called today. But as late as 1914, in the 2nd edition of his (1887–1896) Darboux remarked that “Thus far, mathematical analysis has not been able to envisage any general method which would permit us to begin the study of this beautiful question” (Fig. 7.4).⁶¹

It is impossible to follow the history of minimal surfaces further here. We cannot resist noting that a Riemann surface given by an equation $F(z, w) = 0$ was shown to be a minimal surface in $\mathbb{R}^4$ in (Kommerell 1905). Much more significantly, a

⁶¹See Darboux (1887–1896, 1, 490).

new period was begun with the work of Garnier, Radò, and above all Jesse Douglas in the 1920s and 1930s.⁶² In this period the deep analytic questions were resolved about whether a minimal surface with a given boundary is indeed the surface of least area with that boundary—it need not be—and whether it is unique—again it need not be. Moreover the question of the existence of a minimal surface with a given simple closed Jordan curve as boundary was finally resolved by Radó and Douglas. As Douglas observed this form of Plateau’s problem embraces not only the Riemann mapping theorem but also the investigations of the boundary problem in the Riemann mapping theorem. Thus in a sense the theory of minimal surfaces came to lie at the heart of complex function theory.⁶³

7.9 Conformality

Riemann was the first to emphasise the conformal properties of a holomorphic function, although as we have noted (see Sect. 7.8.1) Gauss was also aware of this property. His gaze rested on the general importance of the concept, and it was left to others to find explicit formulae that map one given region conformally onto another. A number of examples were already known: Möbius transformations, elliptic functions and the j -function, and Schwarz’s triangle functions, but much remained to be done to solve the general problem.

7.9.1 Conformal Maps: Schwarz and Christoffel

Elwin Bruno Christoffel went to Berlin University in 1849, when he was 20, and studied there for four years. Under the influence of Dirichlet, which was to be life-long, he studied not only mathematics but also physics (under Dove) and even chemistry, and his doctoral thesis of 1856 was on potential theory and electrostatics. Three years followed in which he immersed himself in the works of Dirichlet, Riemann, and Cauchy and investigated problems in elasticity, the dispersion of light, hydrodynamics and acoustics, treated by means of ordinary and partial differential equations. In 1859 he habilitated at Berlin, sponsored by Kummer and Martin Ohm. He then taught there as a Privatdozent until 1862, when he went to the Eidgenössische Polytechnische Schule in Zurich, leaving there in 1872, with Schwarz as his successor, to go to the Gewerbeakademie in Berlin and finally to Strasburg in 1872, where he remained as an influential teacher until he

⁶²For his contributions, Douglas was awarded one of the first Fields medals in 1936; the other went to Ahlfors. The work of Radò, Douglas, and Courant is briefly described in (Gray and Micallef 2008) and in more detail in the forthcoming study (Gray and Micallef, to appear).

⁶³For a more detailed account of the history of minimal surfaces, see Gray and Micallef (to appear).

retired in 1895.⁶⁴ The main subject of his cycle of lectures was function theory: definite integrals and infinite series, complex function theory, elliptic function theory, Abelian function theory with applications. Lectures, rather than books, were his preferred means for publishing his own work in this area. He also lectured on the theory of forms and invariants and on partial differential equations.⁶⁵

But if his time at Berlin and the topics of his lecture courses suggests that he was a product of the Weierstrass school, a closer look reveals otherwise. He is best remembered for the formula in conformal mappings that we shall discuss shortly and for the Christoffel symbols in differential geometry—and that is the clue. More and more his interest in physics drove him to appreciate the Riemannian approach to complex analysis. So his lectures followed Riemann “as their guiding star” (Timerding, quoted in (Butzer 1978, 10)). Perhaps for this reason his relationship with Weierstrass gradually soured. Weierstrass had supported Christoffel’s bid to go to Zurich, but when in 1869 a position came up in Berlin with the departure of Fuchs to Greifswald, Dove supported Christoffel but Weierstrass and Kummer chose Thomé instead. In 1885 Weierstrass wrote to Kovalevskaya that Christoffel was a marvellous crank, whose belief that “in 20 or 30 years present day function theory will have gone to its grave and all analysis will have become the theory of forms can be dismissed with a shrug of the shoulders” (See Bölling 1993, 330 and Mittag-Leffler 1923b, 194). This hostility doubtless fuelled Christoffel’s wish to publish in Italian journals, and may well be connected with the unfortunate priority dispute over the Schwarz–Christoffel formula. But it should be said that by 1885 Weierstrass’s anxieties were directed not against Riemann but against Kronecker, as the full letter to Kovalevskaya on 24 March 1885 reveals.

The problem that the Schwarz–Christoffel formula solves is of the conformal representation of a polygonal region on a disc or half-plane. Christoffel proposed it in (Christoffel 1867) in these terms. Given a simply connected plane region F with boundary K , and a function $\psi(x,y)$ defined on the boundary, one wants a single-valued harmonic function with a continuous derivative defined in F that agrees with the given function $\psi(x,y)$ on the boundary. He noted that this was the problem of finding the stationary heat distribution on the surface with given boundary conditions. Also, if the boundary is to be mapped by the solution function to the unit circle then this problem appears as a special case of the Riemann mapping theorem. To obtain explicit solutions, Christoffel let the region F be a polygon with vertices $z_1, \dots, z_n$ and angles there of $\lambda_1, \dots, \lambda_n$.⁶⁶ This done, he sought to represent the polygon on the upper half- Z plane so that the images of the vertices were $a_1, \dots, a_n$ and that as z ran round the boundary of the polygon Z traversed the image in the obvious piecewise linear way. He found that the solution was given by an integral formula:

⁶⁴Among his pupils was Fujisawa, one of the first Japanese mathematicians to study in the West after the Meiji revolution and later the teacher of Takagi.

⁶⁵For an account of Christoffel’s life and work, see Butzer (1978) and Butzer (1981).

⁶⁶Christoffel required that the sum of the exterior angles was 2π .

$$z - z' = \int_{-\infty}^w c e^{\alpha i} (a_1 - Z)^{(\lambda_1/\pi)-1} \dots (a_n - Z)^{(\lambda_n/\pi)-1} dZ. \quad (7.19)$$

(Here α is the angle $z_n z_1$ makes with the increasing x -axis.)

The solution function is obtained by inverting this integral, which is branched over the vertices of the polygon. To see that it is correct, one differentiates the integral and takes logs.

In 1870 Christoffel returned to this theme, seeing it as a contribution to the theory of conformal representation concerning which “almost nothing happened since Riemann wrote his inaugural dissertation” (Christoffel 1870, 11). By now Schwarz had published his own account, (Schwarz 1869a). There he claimed that his interest in the problem had been spurred by Mertens pointing out to him (when they were attending Weierstrass’s lectures on analytic functions) that despite Riemann’s general existence theorem there were no explicit examples known that would map even a triangle onto a disc. He had, he said, presented his solution in a seminar at Berlin in 1864 and again as part of his promotion at Berlin.

In the paper where he put forward the reflection principle, Schwarz set himself the task of mapping a square onto a disc in a way that was conformal everywhere except at the corners. Since the interior of the disc is conformally equivalent to a half-plane, Schwarz sought a conformal map of the square to a half-plane. Under this map the sides of the square are to be mapped onto the real axis, so the reflection principle suggested mapping two copies of the square (arranged as a domino) onto the plane. A further use of the reflection principle to create a larger square suggests mapping four copies of the square onto the plane in a two-to-one fashion, such as an elliptic function does, and indeed the evident periodicity that comes from tiling the domain with copies of the large square makes it clear that the solution will be a lemniscatic function, which Schwarz then found explicitly.⁶⁷ (This is confirmed by the Schwarz–Christoffel formula on letting each $\lambda = \pi/2$, a special case considered by Christoffel in 1867.)

Having squared the circle, Schwarz indicated how other explicit mapping problems can be solved. For example, Mertens’ problem is solved by the inverse of the integral

$$u - u_0 = \int_{z_0}^z (t - a)^{\alpha-1} (t - b)^{\beta-1} (t - c)^{\psi-1} dt.$$

As Schwarz observed, quoting Christoffel and the first edition of Briot and Bouquet (1859), this function is infinitely many-valued unless the triangle is one of the four that tile the plane. For a figure bounded by circular arcs he showed that the

⁶⁷The map has a branch point of order two at each corner of the original square, thus converting the angle of $\pi/2$ there to an angle of π , as it should.

Riemann mapping theorem reduced to a non-linear third order ordinary differential equation, whose solutions were the quotients of two solutions of the hypergeometric equation. This marks the first appearance of the Schwarzian derivative in Schwarz's work, although Schwarz himself said he had learned of it from Weierstrass and it is prominent in Kummer's work on the hypergeometric equation (see [Gray 2000a](#)). Other particular cases followed, including the conformal representation of an ellipse on a circle.

The priority dispute that arose seems impossible to resolve ([Butzer 1978](#), 13). Christoffel, who was a prickly person, did not get over it quickly. But the significance of the discovery of Schwarz and Christoffel was undoubted, even if it left the general status of the Riemann mapping theorem unclear to any discerning analyst. Indeed, it is striking that explicit solutions to the Riemann mapping problem even for the contrived cases of polygonal regions, seemed to stretch the contemporary resources of complex function theory to the limit. As with the theory of minimal surfaces, simple intuition was to prove an uncomfortable measure of ability and therefore a stimulus to research for many years to come.

7.9.2 Conformal Maps and Fluid Flow

A great source of interest in conformal mappings came from the realisation that one could transform a flow around one boundary to a flow around another, much simpler boundary. Since the stream lines are associated with harmonic functions, such a map sends flow lines to flow lines. One can therefore hope to transform a given problem to one with a simple boundary, solve it there, and then transform back to solve the original problem. For example, let us quote Maxwell on the use of solid harmonics to determine the potential due to a conducting sphere and a single electrified point: "We require to expand the potential due to the electrified point in a series of solid harmonics, and to determine a second series of solid harmonics which express the potential, due to the electrification of the sphere, in the space outside" ([Maxwell 1879](#), 1, Chap. XI, § 155.) He credited W. Thomson, later Lord Kelvin, with the discovery that this problem reduces to the determination of the potential due to two points: the given one and its inverse in the sphere. This abolishes the sphere. The method is capable of application to the case of two spheres, as Maxwell described, but other configurations were too difficult to handle.

However, Maxwell found the problem in two dimensions much more tractable. He set himself this task in Chap. XII, § 182 of his *Treatise* (we paraphrase the passage slightly): "A continuous space ... bounded by closed curves C_1, C_2 , etc. being given, to find the form of an everywhere finite, single-valued, continuous harmonic function, V , such that at these boundaries its value may be V_1, V_2 , etc. respectively, being constant for each boundary". If one takes a solution to this problem as the real part of an analytic function, one obtains a solution to the Riemann mapping problem for a non-simply connected domain, a problem hardly broached by mathematicians in 1873. Reasonably enough, Maxwell wrote

“I am not aware that any perfectly general solution of even this problem has been given”.

Maxwell outlined an approach in terms of what he called “conjugate” functions, α and β , and which are simply the real and imaginary parts of a complex analytic function. By Maxwell’s definition they satisfy the Cauchy–Riemann equations and are therefore separately harmonic. In so doing, he was, he acknowledged, following earlier work of C.A. Neumann and Schwarz, and it follows so smoothly from Riemann’s work that one can almost put the opening of Riemann’s (1851) at the head of the chapter and pass it off as the work of one man. That said, there is no doubt that this chapter must be counted among those that made his *Treatise* unintelligible to his Cambridge contemporaries. Even Forsyth (1935) remained unaware that complex function theory had reached the Cambridge lecture theatres, albeit wrapped up in physics. Maxwell did not confine his solutions to problems simplified by inversions, but presented for example the conformal transformation that maps a family of confocal ellipses and hyperbolas to the square lattice (see his *Treatise*, 1, § 192.)

The apotheosis of this approach was reached in 1882 when Gustav Holzmüller published his book on conformal mappings with applications to mathematical physics, in which he described the theory behind the transformations and then presented his results concerning numerous types of transformation in theoretical terms and also in 26 tables containing 66 figures, many of great complexity and considerable charm (see Holzmüller 1882) (Fig. 7.5).

The most famous example of all such uses of conformal mappings is surely the Zhukovsky aerofoil. Indeed it occurs in several books on complex function theory written in the twentieth century as an illustration of the applicability of complex analysis, often as the only one. Happily, for once its fame is deserved. Nikolai Yegorovich Zhukovsky was a Russian mathematician of the powerful Moscow school. He graduated from Moscow University in 1868, and after teaching for some years defended his PhD there in 1882 with a topic on the stability of motion. He became head of the Department of Mechanics at Moscow University in 1886, and his loyalty was such that he declined the opportunity of becoming a member of the St. Petersburg Academy of Sciences in 1900 because he was unwilling to move there.

He worked extensively on hydrodynamics, and in the 1880s became interested in the possibility of powered flight. There was a considerable disparity between various theories that estimated the lift caused by airflow over a wing and between these theories and experiment. In the 1890s he pursued the observation that maps of the form

$$w = az + \frac{b}{z}$$

can be made to map the circle $z = ae^{i\theta}$ onto a variety of elliptical shapes having the rough form of an aerofoil. By suitably choosing the parameters a and b the circle may be mapped onto an ellipse. If $b = a$ is real and positive, the circle is mapped 2–1 onto a real line segment of length $4\sqrt{b}$. Since the solution for flow round a circle is easy to obtain, one can analyse other aerofoils in this way, and Zhukovsky

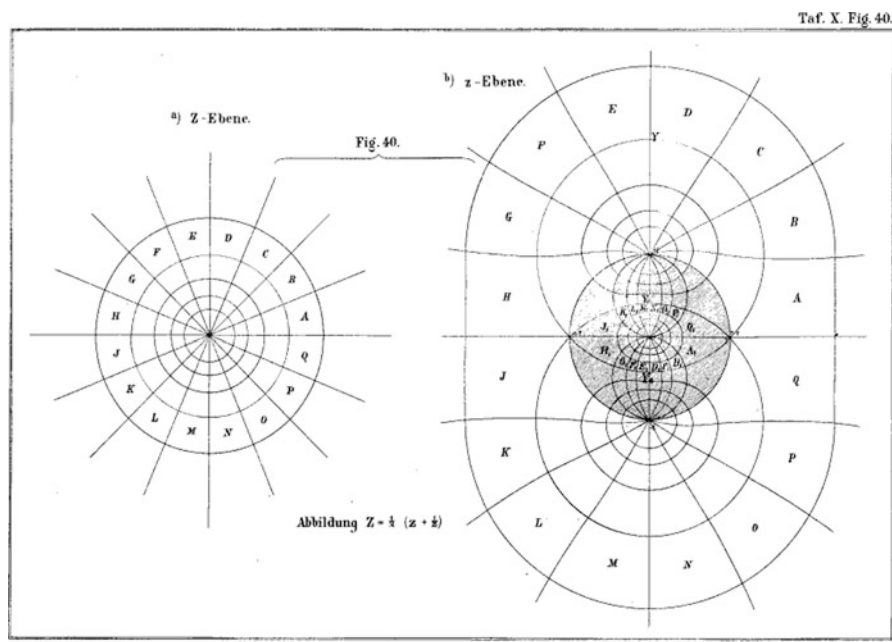


Fig. 7.5 A picture of $\frac{1}{2}(z + 1/z)$ (Holzmüller *Einführung in die Theorie der isogonalen Verwandtschaften und der conformen Abbildungen*, Fig. 40)

was able to estimate the amount of lift that should be generated. The flow is singular at the ends of the line segment, and this raises questions about what is really going on, so Zhukovsky resorted to early wind-tunnel experiments, which showed to what extent theory departed from actual experience. Indeed it departed considerably. The problem is the creation of vortices.⁶⁸ His involvement in all aspects of flight was deep and lasting, and with his student Chaplygin he is remembered as one of the founders of the Russian school of aeronautics; indeed, some refer to Zhukovsky as the father of Russian aviation. It seems, however, his work, similar work by Kutta in Germany, the more practical investigations of those such as Lanchester in England, and the successful breakthrough to powered flight by the Wright brothers in 1903 must all be considered as independent of one another.⁶⁹

To Chaplygin is due another curious discovery. In 1902 he showed that the minimal surface equation describes flow round an aerofoil. Although this gives an exact description only for an implausible gas, it gives a good description of adiabatic frictionless flow and has been used in this way (see Bers 1958). This

⁶⁸See, for example, Chap. 4, Classical Aerofoil Theory, in (Acheson 1990).

⁶⁹Unfortunately, most of the literature on Zhukovsky is in Russian, but we can mention (Grigorian 1981a,b, 1983) in addition to his (1929). However, Zhukovsky's *Collected Papers* (1936–1937), carry articles in English, French, and German as well as Russian (the commentaries are in Russian).

discovery recalls an earlier discovery of Weingarten's (1890). Helmholtz had looked at the stationary (time-independent) flow of incompressible fluids that are given by a harmonic function. He had looked for surfaces of equal pressure in such a flow that are made up of flow lines and had found that this happened in what might be called degenerate situations where the harmonic function depends only on two parameters, not three, and in exploring this situation had shown that the components of the speed of a particle in such a flow satisfy a linear equation. This inspired Weingarten to study harmonic functions whose first derivatives everywhere satisfy the same equation, a condition that forces functional dependence and is therefore captured by the vanishing of the Hessian derivative of the harmonic function. He found that if $V(x, y, z)$ is a harmonic function of this kind, and $dV = \xi dx + \eta dy + \zeta dz$, then the equation ξ , η , and ζ satisfy is that of a minimal surface. By further geometric arguments Weingarten was able to turn this result round and show that there are minimal surfaces for which the corresponding harmonic function gives flows for which there are flow surfaces of equal pressure.

7.9.3 Conformal Structure

The importance of using coordinates provided by a harmonic map, and the corresponding connection to complex function theory, was slowly reworked in the 1860s. In 1867 the French mathematician J.N. Haton de la Goupillière, whose career at the École des Mines lasted almost 50 years, outlined a theory of coordinate transformations for use in problems to do with heat flow (see Goupillière 1867). As part of his analysis, he considered transformations between coordinate systems of the same kind (rectangular Cartesian to rectangular Cartesian, or polar to polar, for example) and showed that in these cases the form of the heat equation did not change. Isothermal coordinates were therefore mapped to isothermal coordinates.

In the same year, 1867, Beltrami⁷⁰ published a much more substantial piece of work investigating when coordinates u and v on a patch of surface can be taken to be defined by a complex function of $w = u + iv$. His conclusion was that the map from a region of the (u, v) plane must be conformal and so, if the line element on the surface

⁷⁰One of the outstanding Italian mathematicians of the second half of the nineteenth century, Beltrami completed his studies at the secondary school (*Liceo*) in his native town Cremona. Then he entered the University of Pavia to get a mathematical degree, but he was soon expelled, accused of having taking part in riots against the Director. Without any doctoral degree, he continued his studies *privatim* under the direction of Brioschi and Cremona. Beltrami began his academic career in 1862, when he was named professor of Algebra and analytic Geometry at the University of Bologna. In 1864 he moved to Pisa, Pavia and eventually to Rome. Like Betti, Brioschi, Cremona, and many other mathematicians of his generation, Beltrami was named Senator of the Italian Kingdom, and after Brioschi's death he succeeded him to the Presidency of the Accademia dei Lincei in 1898. In addition to his celebrated essay of interpretation of non-Euclidean geometry, Beltrami made substantial contributions to differential geometry and mathematical physics. For an account of his life and work, see Loria (1901).

is given as usual by $ds^2 = Edu^2 + 2Fdudv + Gdv^2$ then is it necessary and sufficient that $E = G$ and $F = 0$. This, as he noted, was the condition for the coordinates to be isothermal. He then broadened his investigation to coordinate systems where the u curves cross the v curves at a fixed angle λ that is not necessarily a right angle, thus generalising the work of Gauss. He gave expressions for such quantities as the Gaussian curvature in terms of complex coordinates and concluded by obtaining the analogue of Green's theorem in this setting, from which he deduced the Gauss–Bonnet formula.

Eight years later the physicist Kirchhoff took up the problem of finding the stationary electric flow on a curved surface and solved it by finding a conformal map of the surface onto a plane (Kirchhoff 1876). As Beltrami had done, but apparently independently, he gave the partial differential equation that the potential function must satisfy in terms of the local coordinates on the surface. He then illustrated the general theory with the example of stationary flows on a sphere and a cylinder (which he attributed to Boltzmann). He then considered the case of a torus, choosing coordinates u and v so that the metric on the torus is given by

$$ds^2 = \frac{a^2 - b^2}{(a - b \cos(v/b))} (du^2 + dv^2),$$

where the torus is generated by a circle of radius b whose centre is drawn round a circle of radius a . Kirchhoff looked for a function $\phi + i\psi$ of $u + iv$ with the property that ϕ is a potential function that is logarithmically infinite at two points but otherwise finite (the source and sink of the electric flow) and periodic in u with period $2\pi\sqrt{a^2 - b^2}$ and in v with period $2\pi b$. He exhibited the answer in terms of the Jacobian theta function, where the ratio of the moduli of the corresponding elliptic function is given by

$$\frac{K}{K'} = \frac{b}{\sqrt{a^2 - b^2}}.$$

He noted that the full solution is particularly simple if the source and sink are at the top and the bottom of the torus in its “vertical position”. In this case, $\phi + i\psi$ is given by the function $\log \sin am$.

It seems to have been left to Klein to put the package together in a satisfactory way, drawing on the work of de la Goupillière and Beltrami. In his famous little book *Ueber Riemanns Theorie der algebraischen Funktionen und ihrer Integrale* (1882) he outlined a way in which the x and y curves of a complex function can be thought of as an orthogonal system of flow lines. He first discussed orthogonal systems on the plane, and then on a sphere, and then sought the condition that a system of coordinates on an arbitrary surface can be thought of in this way. He wrote down the Laplacian in arbitrary coordinates, which expresses the condition that a function u' be a harmonic function on the surface in terms of the coordinates and the line element. Then he showed that any such function u' admits a conjugate v' (determined up to a constant) and that u' has the same relationship to v' that v' has to $-u'$, namely, as he then showed, they are orthogonal. He then noted (1882a, 521) that this means that:

If one surface is conformally mapped upon another, every complex function of position on the first is changed in a function of the same kind on the second.

He noted that these conditions were satisfied automatically for a complex function defined on the plane and therefore for complex functions on any surface.

In view of the fact that Klein was comfortable with the Gaussian idea that the geometry of a surface is intrinsic, his derivation of the conformal structure on a surface from its metric is a step towards the articulation of Riemann's vision of complex function theory on a Riemann surface that is not a disguised ("many-valued") function theory on the complex plane. In this booklet he argued, correctly, that a Riemann surface "not only provides an intuitive illustration of the functions in question, but it actually *defines* them" (Klein 1882a, 555), and noted that most mathematicians who came after Riemann had been content to separate these two parts and define the functions in some other way. One of the things Klein did when he moved, around 1880, to take up the Riemannian mantle was to try to produce an abstract theory of Riemann surfaces. He did this in his lectures at Göttingen in 1891/92, when he observed that a Riemann surface can be given a conformal structure by working locally and covering it with a finite number of domains each conformally equivalent to a disc.

7.10 Problems in Mechanics

We now turn to two problems in mechanics: the motion of the top and the stability of the solar system. The first of these had been studied by means of elliptic integrals by Legendre in vol. 3 of his *Exercices* (1811–1817), so the introduction of complex analytic methods is perhaps to be expected but the second of these exhibits a surprising connection.

7.10.1 The Spinning Top

Jacobi's analysis of the motion of the top had made considerable use of the properties of elliptic functions. Much later Kronecker showed how the many formulae Jacobi had produced could be reduced to three and derived by an astute use of the Cauchy integral theorem (Kronecker 1881). This was a further pedagogic achievement of complex function theory. Meanwhile, Jacobi's discovery of the interplay between a rich mechanical problem and the dense cluster of formulae expressing the properties of elliptic functions had proved typical.⁷¹ Inspired by Jacobi, Tissot gave a similar analysis of the motion of a spherical pendulum in 1852

⁷¹Weierstrass took the subject of the rotating top as one of his applications of elliptic functions in lectures of 1873 and 1875; see his *Math. Werke* 6, 252–329.

(see [Tissot 1852](#)). This is the problem of a pendulum, here a light rod with a weight at one end and fixed but free to rotate at the other, or equivalently, of the motion under gravity of a particle constrained to move on a sphere. The energy integral once again expresses the time as a function of the position variables; by passing to the Jacobian elliptic functions Tissot was able to express the position as a function of the time. In this way Tissot re-proved a result of Puiseux's ([1842](#)) which we digress briefly to state. Tissot located the particle on the sphere by its depth below the equatorial plane, denoted z , and the angle a plane through the particle and the centre of the sphere makes with a fixed vertical line. This angle he denoted ψ and he showed that its derivative ψ' is a periodic function (with period $2T$), so ψ itself is the sum of a periodic function and a function proportional to the time. If we give a frame of reference the corresponding angular velocity, the motion would appear to oscillate periodically about a fixed line. Puiseux's theorem is that if the angular velocity is Ψ/T , then Ψ is always greater than $\pi/2$ (see [Tissot 1852](#), 99). In the second volume of his *Traité des fonctions elliptiques* (1886–1890), Halphen showed that this angle is always less than π , and in 1896 A. de Saint-Germain, following a paper of Hadamard's, showed how Halphen's inequality can be derived from the theory of residues.⁷²

7.10.2 Kovalevskaya's Prize Paper

The motion of the top was also studied by Kovalevskaya (Fig. [7.6](#)) from a viewpoint that more explicitly involved complex function theory (see [Kovalevskaya 1890](#)). She interpreted the Euler equations for the body as six differential equations for the angular velocity (which has three components) and the instantaneous axis of rotation (which has two independent components, being a unit vector). The problem has six degrees of freedom because the initial conditions involve six numbers: three for the initial position of the body axes and three for the initial angular velocity. But the differential equations are autonomous (i.e., of the form $\frac{dW}{dt} = f(W)$, where the right hand side is independent of t). So if $p(t)$ is a solution so is $p(t - t_0)$, which restores a sixth degree of freedom to the problem. The equations between the unknowns established by Euler and Lagrange expressed the fact that the instantaneous axis of rotation is a unit vector, that the sum of the kinetic and potential energies is constant, and that the vertical component of the angular momentum is constant. Kovalevskaya pointed out that in certain cases they had considered, the unknowns were all expressible as single-valued functions of time whose only solutions were meromorphic (the only singularities for finite complex values of the time variable t were poles). These cases possessed an unusual degree of symmetry: the centre of gravity lay on one of the axes of the corresponding inertial ellipsoid. She asked

⁷²Puiseux's inequality can also be derived that way; the result is stated incorrectly in ([Weinstein 1942](#)). For an account employing the Weierstrass $\wp$ -function, see [Whittaker \(1904, 104–106\)](#).

Fig. 7.6 Sonya Kovalevskaya (*Acta Mathematica Table générale des tomes 1–3, 1882–1912, p. 153*)



whether this was the case in general. If it was, the unknowns could be expressed as power series in t perhaps multiplied by some negative integer power of t . These series should converge for a disc of t -values and involve five arbitrary coefficients; if they involved less these solutions would be linearly dependent and the other solutions, linearly independent of these five, would not be meromorphic.

By means of a lengthy computational analysis she showed that there could be a meromorphic solution if two of the moments of inertia of the top were equal to twice the third and the centre of gravity is related in a specified way to the centre of the axes of inertia. Heavy use of the combinatorial formulae for the Weierstrassian elliptic functions then enabled her to display the new meromorphic solution to the Euler equations in this case and to express the answer in terms of theta functions of two variables (themselves linear functions of the time). For this feat she won the Prix Bordin of 1890.⁷³ It is worth mentioning here that at Kovalevskaya's request, on August 16, 1888 Weierstrass asked Schwarz to provide her with a physical model satisfying the hypotheses of her theorem, which he did. The model was sent to the Paris Académie, but, as Cooke (1984, 159) has remarked, it "apparently did not adequately convey the essence of the Kovalevskaya case". In 1897 Zhukovsky shed some geometrical light on Kovalevskaya's formulae,⁷⁴ and also in that year her work was complemented by Roger Liouville (1896), who showed that Kovalevskaya had found the only case that Euler and Lagrange had missed: there are no other

⁷³For an account of the circumstances surrounding the prize, which several mathematicians seemed keen for her to win, see (Cooke 1984, 110–115). We have also followed Cooke's account of Kovalevskaya's work (Cooke 1984, 151–159).

⁷⁴See also Zhukovsky (1891).

Fig. 7.7 Christian Felix Klein (in Stubhaug, *The mathematician Sophus Lie*, Springer ©2002, p. 336)



tops for which there are algebraic integrals other than the energy and momentum equations provided. Liouville's work—which is too lengthy to be described here—built on that of Bruns, who had shown in 1887 that the three-body problem had no algebraic integrals other than the obvious ones (see Sect. 7.10.4). Like Bruns's paper, it represents a success for the methodological programme of asking: “when can a differential equation have algebraic solutions?”.

7.10.3 Klein's American Lectures

In the 1890s Klein took up the problem of the top rotating about a fixed point as part of his growing interest in applied mathematics (Fig. 7.7). He chose this as the subject of a series of lectures he gave at Princeton University in October 1896 (see Klein 1897). There he represented the point (x, y, z) of the sphere of radius r by means of stereographic projection in the form $Z = \frac{x+iy}{r-z}$. A rotation of the sphere sending Z to ζ is given by a transformation of the form

$$\zeta = \frac{\alpha Z + \beta}{\gamma Z + \delta},$$

where α, β, γ , and δ are complex but $\alpha = \bar{\delta}$ and $\beta = -\bar{\gamma}$. Klein based his analysis on these complex quantities and the complex variable ζ . The parameters α, β, γ , and δ were introduced by Riemann, Klein commented, and they have shown to be useful in geometrical problems connected with function theory, especially minimal surfaces and the theory of regular solids. His aim was now to prove that they may be used “to quite as great advantage” in the study of a rigid body rotating about a fixed point.

He showed how his parameters were related to the usual ones and to Cayley's quaternionic ones (see [Gray 2005](#)). To explain how they behaved as differential equations of the time, t , he allowed t also to become complex, writing that

It is only by taking this step that it becomes possible to bring about the intimate association of kinetics and the theory of functions of a complex variable at which we are aiming ([Klein 1897](#), 624).

He then showed how the Lagrangian equations of motion, which had simple enough solutions for the direction cosines of the top as functions of time, had even simpler solutions for his new parameters as functions of time.

For a proper understanding of the problem of the motion of the top, [Klein \(1897, 629\)](#) commented, some knowledge of elliptic functions is obviously needed, and “I know no readier means of gaining this than *Riemann's method of conformal representation*” (italics Klein's). Accordingly, he constructed the relevant Riemann surface and the conformal representation of it in the t -plane.

On introducing those parameters, he wrote

A remarkable simplification at once ensues such as renders any further reduction unnecessary. Surely a pre-established harmony exists between the problem before us, and our parameters α, β, γ , and δ ([Klein 1897](#), 632, italics Klein's).

Simple considerations of the elliptic integral defining them showed that their logarithms were all normal integrals of the third kind, and so they were quotients of the form

$$ke^{\lambda t} \frac{\sigma(t + ia)}{\sigma(t)},$$

where σ denoted a Weierstrass sigma-function. So they “miss being doubly periodic by an exponential factor only”, but are otherwise of the simplest kind, so “we have resolved the problem of the top into its simplest elements” ([Klein 1897](#), 634).

Klein then showed how these solutions enabled one to deduce such results as the polhode and herpolhode of the spherical top are elliptic plane curves of the first degree; a result obtained earlier but in a more complicated way, he said, by Darboux in Despeyrou's *Cours de mécanique* (1884–1886). He concluded his series of four lectures by showing how the ordinary toy top, one with no fixed point, leads to hyperelliptic integrals that can be treated by introducing automorphic functions. In this connection he observed that the motion of the apex of the top was best described by non-Euclidean geometry, but sadly we shall have to pass that matter over in silence.

7.10.4 The Three-Body Problem

The three-body problem, which asks for an analysis of the motion of three bodies moving in space subject only to their mutual gravitational attractions, is one of the most celebrated problems in dynamics. It originated in Newton's attempt in his

Principia to study the motion of the moon as it orbits the Earth under the attractions of the Earth and the Sun. Newton's attempt was remarkable and yet unsuccessful; Clairaut's more successful attempts in the 1740s were influential for that reason in winning acceptance for Newton's hypothesis of the inverse square law of gravity among Continental mathematicians. The very intractability of the problem drove mathematicians to propose a number of simplifications. We shall be concerned with one: the so-called restricted three-body problem (the name is due to Poincaré).

7.10.4.1 Poincaré's Prize Paper

Poincaré had been led to this simplification as part of his work for a prize competition. On the advice of Mittag-Leffler, Weierstrass, and Hermite the prize was to be awarded by Oscar II of Sweden to mark his 60th birthday on January 21, 1889. Apparently Weierstrass was concerned with this question as early as 1874 or even before, as he wrote to Sonya Kovalevskaya on December 16 remembering the time when their friendship had become closer, and they "lost themselves in scientific dreams and phantasies". By that time they also used to talk about the conditions of the stability of the world system. Weierstrass added to have dealt "seriously for some weeks" with the problem of the integration of the relevant system of n differential equation. He guessed he had found a route for reaching the goal, but he confessed that "an even greater effort was needed in order to go along it successfully" (see Bölling 1993, 163–164 and Mittag-Leffler 1923b, 152–153).

The question that Poincaré submitted an answer to asked for a proof of the convergence of the series usually used in astronomy. He found even this too difficult and simplified the problem to the restricted three-body problem, which postulates two bodies, often called S and J for the Sun and Jupiter, orbiting each other in a fixed plane. They have mass and so, by Kepler's laws, they travel in ellipses about their common centre of gravity. The third body (the "planetoid") is supposed to travel in the fixed plane and to have negligible mass, which means that it is affected by the massive bodies S and J but does not itself affect them. The problem is to find the orbit of the planetoid. Even in this case, when Poincaré tackled this problem in the 1880s surprisingly little was known, special initial configurations aside. Poincaré was to discover an excellent reason for this: even when closed, periodic orbits exist, nearby orbits can exhibit chaotic behaviour. For an stimulating account of Poincaré's discovery and the writing of his prize-winning memoir of 1889, eventually published as (Poincaré 1890a), the reader is referred to (Barrow–Green 1994, 1997).⁷⁵ Here we concentrate on the involvement of, and implications for, complex function theory.

That such a seemingly real problem as the motion of celestial bodies arises in the study of complex function theory says much about the state of differential equation theory in the nineteenth century. In Poincaré's formulation of the restricted three-

⁷⁵See also Diacu and Holmes (1996) and Andersen (1994).

body problem, four variables, x_1, x_2, y_1 , and y_2 , describe the position and momentum of the planetoid at any one time. They can be chosen so that the equations of motion can be written in Hamilton–Jacobi form:

$$\begin{aligned}\frac{dx_1}{dt} &= \frac{\partial F}{\partial y_1}, & \frac{dy_1}{dt} &= -\frac{\partial F}{\partial x_1}, \\ \frac{dx_2}{dt} &= \frac{\partial F}{\partial y_2}, & \frac{dy_2}{dt} &= -\frac{\partial F}{\partial x_2},\end{aligned}\tag{7.20}$$

where F is a function of the four variables and conservation of energy ensures that indeed F is constant (so it is independent of the time variable, t). The first question for Poincaré was: under what conditions does such a system of partial differential equations have a solution? For systems of ordinary differential equations, he said, there was an excellent theory going back to Cauchy and improved by Weierstrass. The method was to obtain formal power series solutions to the equations and then to establish their uniform convergence by a majorising argument. Weierstrass had applied this method to the three-body problem, but had not been able to establish the required convergence.⁷⁶ Cauchy's approach had been extended to systems of partial differential equations by Kovalevskaya, and Poincaré had sought to extend her ideas in his own thesis of 1879.⁷⁷

In her paper, Kovalevskaya showed that a certain system of linear first order partial differential equations with algebraic coefficients the formal solutions obtained by the method of undetermined coefficients have a common positive radius of convergence, and so define analytic solutions on that neighbourhood, provided certain compatibility conditions are met.⁷⁸ Rather than discuss what her theorem says can happen, it is more interesting to look at what she showed need not happen—this is certainly what impressed Weierstrass. Kovalevskaya showed that the one-dimensional heat equation $\frac{\partial u}{\partial t} = \frac{\partial^2 u}{\partial x^2}$ has no solution with the initial distribution $u(x, 0) = (x - 1)^{-1}$.⁷⁹ However, if conditions are imposed on the second variable, say $u(x_0, t) = f(t)$, $\frac{\partial}{\partial t} u(x_0, t) = g(t)$ where f and g are analytic functions of t , then the differential equation has a unique solution that is analytic in a neighbourhood of (x_0, t_0) . As we noted above, Sect. 6.10.2, Weierstrass was surprised and impressed by this result, as he confessed in a letter to du Bois–Reymond (Weierstrass 1923a, 204).

Poincaré had earlier studied the equation

⁷⁶See the long letter he sent on August 7, 1885 to Mittag-Leffler in (Mittag-Leffler 1912, 37–46).

⁷⁷See Kovalevskaya (1875) and Poincaré (1879). Later reproved and extended in (Koenigsberger 1894).

⁷⁸For a more detailed account, see Cooke (1984, 30–34).

⁷⁹Cooke (1984, 33) supplies a physically plausible initial condition with the same effect: $u(x, 0) = (1 + x^2)^{-1}$.

$$X_1 \frac{\partial z}{\partial x_1} + X_2 \frac{\partial z}{\partial x_2} + \dots + X_n \frac{\partial z}{\partial x_n} = \lambda_1 z,$$

where each function X_i was developable in powers of $x_1, x_2, \dots, x_n$ and each X_i was of the form $x_i - Y_i$, where Y_i consisted of terms of degree at least 2. He showed indeed that under certain hypotheses on the λ 's there was a holomorphic solution of the equation developable in powers of the x_i 's. The details need not concern us; what does is that the only rigorous theory of differential equations available to Poincaré was one in which rigour was assured by the careful handling of power series expansions. In the context of dynamics, the supposition that the function F in the Hamilton–Jacobi equations was holomorphic was entirely justifiable; it was usually an algebraic function of the coordinates of the planetoid. The existing theory then tipped the analysis towards the search for holomorphic solutions, and since the motion was in a plane it was moreover a natural step to replace the position coordinates by a complex coordinate, z . Partial differential equations with analytic coefficients make good sense in applied mathematics; the theorems Kovalevskaya and Poincaré proved about them involve analytic solutions for both mathematical and historical reasons.

Poincaré's approach to the Hamilton–Jacobi equations involved a further extension of these ideas. He now supposed that the function F depended on a parameter, μ , and that when $\mu = 0$ there was a closed orbit. He then looked at nearby orbits by allowing the parameter μ to vary. By linearising the problem he could obtain some hold on the nearby solutions in terms of what he called the characteristic exponents. These depended on μ and when negative indicated that the periodic solution attracted, when positive that it repelled nearby orbits. The case when the characteristic exponents vanished with μ was much more delicate. What Poincaré showed was that when this occurred, the characteristic exponents could be written as power series not in μ but in $\sqrt{\mu}$ and that this subtle change converted some of the power series solutions to the Hamilton–Jacobi equations from being convergent for a range of values of μ to being merely asymptotic. To his surprise⁸⁰ this resulted in chaotic behaviour among the nearby solutions.

It is worth an excursion to see why. Poincaré argued as follows. A periodic orbit corresponds to a closed path in (x_1, x_2, y_1, y_2) -space. By moving the origin if need be, the surface $S = y_2 = 0$ meets the orbit in a point, P , at which $y_1 = 0$. He showed that there is a curve C^+ through P of points P^+ such that the orbit through P^+ moves away from P , in the sense that this orbit returns to S at a point on C^+ further away than P^+ . There is also a curve C^- through P consisting of points P^- such that orbits through P^- move towards P . As P moves around its orbit, the curves C^+ and C^- sweep out surfaces, which Poincaré called the asymptotic surfaces for the orbit of P . The question is: how do they intersect the surface S ? One possibility, which Poincaré himself favoured for a time, is that they form one closed curve. He convinced himself by a long approximation argument that if the power series

⁸⁰Not to say great initial embarrassment, see Barrow–Green (1997).

solutions converged then indeed the intersection would be a closed curve. But when he realised that the formal solutions do not converge and are only asymptotic, he realised that the intersection may not be a closed curve. In which case the C^+ and C^- parts must intersect transversely, while C^+ cannot never intersect itself, and nor can C^- . What of their intersection point? On following it round its orbit, it returns to S at another point common to C^+ and C^- , and so the curves must intersect at infinitely many points: chaos. Writing about this in 1899, Poincaré said “One is struck by the complexity of this figure, which I do not even attempt to draw” (Poincaré 1899a, 389).

This dramatic, and completely unexpected, geometrical implication of the theory of asymptotic expansions of analytic functions that Poincaré had been developing in the late 1880s was convincing proof of the importance of the idea. Other implications, for the theory of special functions, are described in the next section. But more was to follow for the study of the three-body problem. In an important later section of his prize-winning memoir, Poincaré showed that certain series introduced by Lindstedt⁸¹ likewise failed to converge, although they too could be rescued for practical purposes by the theory of asymptotic expansions.⁸² Lindstedt had considered a problem raised by Gylden of a suitably perturbed harmonic oscillator, and had sketched a method showing that it would oscillate with a new frequency, but the method was one of the successive approximations, and the convergence question had seemed hopeless. Poincaré re-interpreted the problem in the language of Hamiltonians and showed that the infinite series was a formal solution that converged asymptotically. He then went on to show that the Hamilton–Jacobi equations admitted no other single-valued analytic solution other than the one provided by the conservation of energy ($F = 0$). This generalised an earlier result of Bruns, who had shown that there was no algebraic integral.⁸³

7.10.4.2 Painlevé’s and Levi–Civita’s Contributions

The general three-body problem received a further impetus from complex function theory with the introduction of ideas about the nature of the singularities the solutions could possess. In 1895 Paul Painlevé gave a series of lectures on the subject at Stockholm that he collected in a monumental treatise on the singularities of differential equations whose general solutions were single-valued (or at most n -valued) analytic functions (Painlevé 1897), presented to Oscar II, King of Sweden. Almost at the end of this 589-page work (indeed, on p. 586) he showed that the three-

⁸¹ Anders Lindstedt was a Swedish astronomer graduated from Lund in 1877 and worked for a time in Dorpat (now Tartu, Estonia, but then a Russian province). In 1886 he moved to the Royal Technological Institute in Stockholm.

⁸² The matter proved understandably controversial, and a number of papers were exchanged before it was resolved.

⁸³ Bruns (1887), see Whittaker (1904, 4th ed. 1937, Chap. 14).

body problem admitted solutions that were uniform everywhere except possibly at times when two or even all three bodies collided. So, he said, it was important to determine the initial conditions that could lead to a collision, but this he was unable to do. Painlevé's result is not trivial: it rules out the possibility of one of the bodies escaping to infinity in finite time.⁸⁴

The study of motion near a collision in the restricted three-body problem was investigated by two Italian mathematicians, [Bisconcini \(1905\)](#) and [Levi-Civita \(1905 and 1906\)](#). Levi-Civita showed that it was possible to change variables in the differential equations of the problem so that the equation became regular. This meant that its solutions could not have essential singularities, and indeed could be continued analytically through any collision. How could this be? The collision of the planetoid with a massive body will reverse the motion of the planetoid, just as a tennis ball bounces off the Earth. Replacing the z -coordinate that describes the position of the planetoid by a coordinate w related to z by the equation $w^2 = z$ and opening out the w -disc converts the path in the z -domain to a straight line in the w -domain, demonstrating the possibility of analytic continuation.

7.10.4.3 Sundman's Resolution

It remained for Sundman to resolve the general three-body problem completely, in a paper Birkhoff called "one of the most remarkable contributions to the problem of three bodies that has ever been made".⁸⁵ He showed that there could only be a simultaneous collision of all three bodies if the total angular momentum of the system vanished, and that if this case was excluded all the singularities of the solutions were of the removable type just described. He showed that one could continue the solutions analytically in this fashion through a succession of shocks, and that this process could be continued for arbitrary intervals of time unless a triple collision occurred. He was then able to obtain convergent power series solutions valid for all values of the time variable.⁸⁶ Indeed, such was the weight of the context in which Sundman was working that his time variable was complex, and the convergence was obtained for all values of t in a strip about the real axis.

The main step in Sundman's regularisation of the problem was his introduction of a new independent variable, u , related to the time, t , by an equation of the form $u = \text{power series in } t^{1/3}$. Triple collisions being excluded, he showed that at each double collision the radius of convergence in u always exceeded a fixed amount (determined by the total angular momentum). This involved investigating the possibility of collisions occurring for complex values of the "time" variable, u .

⁸⁴It was shown in ([Xia 1992](#)) that this can happen in the five-body problem.

⁸⁵See [Sundman \(1912\)](#) and [Birkhoff \(1927, 260\)](#). We recommend the short account ([Saari 1990, 105–119](#)), and for information about Sundman, ([Barrow–Green 2010](#)).

⁸⁶It is usually claimed that their rate of convergence is too slow to prevent them being of any use, but lately this claim has been contested. See the references in ([Barrow–Green 2010](#)).

His result showed that complex singularities had to lie outside a fixed strip about the real axis. This enabled him to introduce his final choice of independent variable, a complex variable τ that took real values between -1 and 1 precisely when t took real values between $-\infty$ and ∞ . The conclusion of the paper was that the positions of the bodies were expressible as power series in τ that converged for $|\tau| < 1$, and represented the motion for all time, double collisions being allowed but triple collisions excluded. However, as Eddington noted in his commentary on Sundman's paper, the formula connecting τ and t expresses τ as the hyperbolic tangent of an integral involving t , and it increases monotonically with t "so for a remote instant must be represented by a decimal beginning with a large number of 9's. Assuming, as we probably may, that a million years hence the three bodies will be describing orbits very much as they do now, it follows that the series will be extraordinarily sensitive to a minute change of τ when τ approaches unity. This suggests that the series must be of a slowly convergent and not very 'useful' type" (Eddington 1915).

Sundman's resolution of the three-body problem seems to have almost ended research into it along classical lines, although later contributions were made. Part of the reason was the very completeness of his work, another the very slow convergence of the series just alluded to. It is also true that astronomers were turning from the arduous task of computing orbits of asteroids to exploring the new realms of stellar astronomy. From the standpoint of a history of complex function theory, it is noteworthy that from the time of Poincaré to Sundman there was a consensus that differential equations with analytic coefficients should be solved by complex analytic functions. This consensus only eroded with the arrival in the 1950s of a new viewpoint on the question: KAM theory, named after its founders, Kolmogorov, Arnold, and Moser. In this approach, the Hamiltonian function (F , above) is still analytic, and the attempt is no longer to perturb it by small variations of a parameter, μ , in the manner of Poincaré. The main theorem is that under the perturbation most closed orbits remain closed, and these closed orbits form a nowhere dense, closed set whose measure is large compared with μ . The amusing hypothesis on F is that it be 354-times differentiable.

It should also be noted that the three-body problem provided another impetus to complex function theory. As we shall discuss in Chap. 9, the memoir by Poincaré is also one of the few important early papers in the theory of complex functions of several variables, and was one of the main reasons why he was drawn to that subject.

7.11 Concluding Remarks

By the end of nineteenth century complex variables ideas and methods had spread in the fields of mathematical physics and applied mathematics more generally to such an extent that it would be difficult, and in any case beyond of the scope of this book, to provide a detailed account. But, by the end of 1900 complex function theory was still far from being a unified field of mathematics. The theories of the "founding fathers" Cauchy, Riemann, and Weierstrass, offered different approaches

and a consensus was far from being reached. However, it was becoming more and more apparent from the ways in which complex function theory influenced the theories of ordinary and partial differential equations and topics in such areas as potential theory and mechanics, that Riemann's methods were conceptually the most fruitful.

This derived in part from Riemann's very definition of a complex analytic function by means of the Cauchy–Riemann equations. This naturally exhibited their close connection with harmonic functions, and thereby opened up the possibilities of other analogies with functions in mathematical physics. Indeed, it could be said to place complex analysis within the field of partial differential equations to invite explorations of that broader field—one that Weierstrass was markedly reluctant to enter. But by also insisting on the conformal properties of analytic maps Riemann had opened a door to geometry as well. Here one naturally finds the study of minimal surfaces and more generally conformal maps. Once global questions are asked about such a map, even in the form of its possible analytic continuations, the geometrical, even topological, setting of Riemann surfaces was the natural way to develop the theory.

But Riemann had not dealt exclusively with grand ideas. He had also provided a range of technical insights: the monodromy of solutions to the hypergeometric equation, ingenious contour integrals, and the method of steepest descent. These helped convince mathematicians and physicists that complex analysis had a major role to play also in work on topics where explicit numerical answers are required. Far from being a topic only of interest to the purer mathematician drawn perhaps to the tidier algebra of the complex field, complex analysis was proving to be an appropriate place to gain physical insight.

Chapter 8

Advanced Topics in the Theory of Functions

The theory of functions, as one must call it, today attracts the youngest, the most active, and the most inventive among us with an irresistible force. A new spirit animates them: they have already given us some beautiful and profoundly original discoveries, and they promise even more for the future.

Darboux

8.1 Introduction

By 1880 the main features of the present-day complex function theory had been created, although not assembled into their modern order. Cauchy and Riemann were dead, but Weierstrass was in firm control of his own version of the theory and lecturing on it as part of his 2-year cycle of lectures in Berlin. As we saw in the previous chapter, complex function theory in its various forms had found in widening number of successful applications to other domains of both pure and applied mathematics. In this chapter we look at how the ideas of the founders led to important developments in the theory of complex functions itself and to the discovery of some of the central features of complex function theory as a rich research topic in its own right.

One development concerned Riemann's ideas, which, although obscurely expressed, were taken up quite early, perhaps because they suggested a rich source of other melodies that, as with Schubert's music, were so tantalisingly presented. Or perhaps, and more prosaically, Riemann's ideas suggested more of a programme that could be followed and would in any case have to be followed if one was to read this particular master at all. The route to understanding Riemann was to pass close to the still-unpublished ideas of Gauss about the geometric representation of complex quantities and complex functions. The result was a branch of the subject called geometric function theory today. It grew out of attempts to understand the behaviour of the modular function and its appearance in other parts of mathematics.

Of these, one deserves special mention: the solution of equations, and we shall start this chapter with it. The subsequent adoption of the viewpoint of geometric function theory includes Picard's remarkable theorems, which were to inspire a generation of mathematicians, and led to several problems, of which two were central: the Riemann mapping theorem, and a problem Riemann would undoubtedly have savoured, the uniformisation theorem. These are considered in Sect. 8.3 of this chapter, which leads us to a consummation of sorts in Sect. 8.4, where we look at Weyl's ideas about Riemann surfaces.

If the ideas of Cauchy, notably the integral theorem and the residue theorem, may be taken as uniting the twin themes of analytic functions and their power series expansions, it was not until the 1880s that significant new variations on these themes were found, notably the connection between the singularities of a function and the coefficients of its Taylor series. So on chronological grounds that part of the story is told in Sect. 8.5. Interestingly, this part of the story is largely French. It starts with Laguerre's introduction of the concept of genre of an entire function and its exploitation by Poincaré, Hadamard, and, in a refined form, by Borel. These mathematicians were often motivated by Picard's theorems, as were their German rivals, Schottky, Landau, and Carathéodory, and the French and German approaches provide an interesting contrast.

In Sect. 8.6 we look at the study of series of analytic functions, arguably begun in earnest by Runge, taken up by Osgood, and brought to a definitive state by Montel. In Sect. 8.7 we look at the work of Fatou and Julia on iteration, which represents a high point in function theory that could not be surpassed until the invention of modern computers. In both these sections advances in the subject drew not only on function theoretic ideas but also on ideas concerning the topology of sets of points in the plane.

8.2 Geometric Function Theory

8.2.1 Solving Polynomial Equations

As was explained in Chap. 2, the solution of polynomial equations by algebraic means seemed paradoxical in the sixteenth century. The formula or method due to Tartaglia and Cardano would provide the unique real solution in the case when there was only one, but cubics with three real roots could only be solved by juggling with complex numbers, at least until it was understood how the sum of two complex numbers could be real. The way out had been to abandon the algebra in favour of trigonometry.

It is easy to see that the cubic equation $x^3 + bx + c = 0$ and real coefficients has three real roots if and only if $4b^3 + 27c^2 \leq 0$. Indeed if, on replacing x by λx , the equation $\lambda^3 x^3 + b\lambda x + c = 0$ can be reduced to $4\sin^3 \theta - 3\sin \theta + \sin 3\theta = 0$ then $\frac{\lambda^3}{4} = -\frac{b\lambda}{3} = \frac{c}{\sin 3\theta}$. It follows that $\lambda^2 = \frac{-4b}{3}$, so b must be negative, and that

$\sin 3\theta = \frac{-3c}{b\lambda}$. The condition $-1 \leq \sin 3\theta \leq 1$, in the form $0 \leq \sin^2 3\theta \leq 1$, implies, after eliminating λ , that $\frac{27c^2}{-4b^3} \leq 1$. The denominator is positive, so the reduction can be effected if and only if $27c^2 \leq -4b^3$, which rearranges to the stated condition $4b^3 + 27c^2 \leq 0$.

So when there are three real roots the solutions of the equation $x^3 + bx + c = 0$ are found essentially by trigonometry as follows. The solutions of the equation $4\sin^3 \theta - 3\sin \theta + \sin 3\theta = 0$ are $\sin \theta$, $\sin(\theta + \frac{2\pi}{3})$, and $\sin(\theta + \frac{4\pi}{3})$. These are also the solutions of the equation $\lambda^3 x^3 + b\lambda x + c = 0$ if $\frac{\lambda^3}{4} = -\frac{b\lambda}{3} = \frac{c}{\sin 3\theta}$. These equations determine λ and $\sin 3\theta$ in terms of b and c so the solutions of the equation $x^3 + bx + c = 0$ are $\lambda^{-1} \sin \theta$, $\lambda^{-1} \sin(\theta + \frac{2\pi}{3})$, and $\lambda^{-1} \sin(\theta + \frac{4\pi}{3})$ for a particular value of θ . If sine is allowed to take complex values, then the same method solves any cubic equation.

The importance of the modular equations (discussed briefly in Sect. 1.3) in this connection cannot be overestimated. First, Galois noted that for the primes 5, 7, and 11 but no others the modular equations of degrees 6, 8, and 12 reduce in degree by 1. This is already an interesting phenomenon requiring explanation. Then in 1858 a number of mathematicians, Hermite among them, noted that the reduction of the equation of degree 6 leads to a solution of the general quintic equation along the lines of the trigonometric solution of the cubic. This discovery was all the more exciting because Galois' proof that the quintic is unsolvable by radicals was still in the process of being assimilated by the mathematical community.¹

The modular equation of degree 6 relates the value of the modular function at K/K' and $5K/K'$. Let us write $\theta = K/K'$ and f for the modular function, so the modular equation can be thought of as an equation for $f(\theta)$ in which $f(5\theta)$ is a known quantity. Suppose that the function f is well understood and a table of its values is known. Then an equation of degree 6 (for the moment) which has the same coefficients as the modular equation, for a suitable value of 5θ has as its solutions the values of $f(\theta)$, which may be found from the table.

What Hermite noticed was that, on the one hand, the reduction of the modular equation of degree 6 to an equation of degree 5 gave an equation that depended on a parameter, but, on the other hand, the family of quintic equations was also essentially a one parameter family. They could all be reduced to equations of the form $x^5 + x + a = 0$ by making at most quadratic or cubic changes of variable. So given a quintic equation, with a fixed value of a , could the parameter in the modular equation be chosen so that the reduction of this particular modular equation was equivalent to the given quintic? He found that it could, and that the equation for the parameter was only a quartic. So the quintic equation, although not solvable by radicals, was solvable by modular functions. The result was very exciting and was immediately discussed by a number of authors, notably Brioschi and Kronecker. The natural question was whether the next case, the modular equation of degree 8,

¹Edwards (1984); Lützen (1990); Nicholson (1993); and Neumann (2007).

likewise reduced to the general equation of degree 7, but Kronecker showed that it did not.²

So cubics, and by Ferrari's work, quartics, require the exponential function (whence the sine function) for their solution, while quintics require a new transcendental function, the modular function. The obvious question, raised by Kronecker among others, was: is there a class of equations associated with a family of functions whose values may be assumed known, that solve every polynomial equation? The affirmative answer was given by Jordan in his famous *Traité* of 1870 (see pp. 380–382). His solution took a polynomial equation of the form

$$a_0x^n + a_1x^{n-1} + \cdots + a_n = 0,$$

which is assumed to have n distinct roots and solved it with functions drawn from the theory of hyperelliptic integrals. More precisely, Jordan introduced the hyperelliptic curves

$$y^2 = x(x-1)(a_0x^n + a_1x^{n-1} + \cdots + a_n),$$

if n is odd and

$$y^2 = x(x-1)(x-2)(a_0x^n + a_1x^{n-1} + \cdots + a_n),$$

if n is even.

Each curve has a matrix of periods and a corresponding set of theta functions. Jordan then showed how the roots could be expressed as expressions in the null values of the theta functions.

In Jordan's presentation the hyperelliptic curve appears as the natural generalisation of the elliptic curve and its modular function. Nonetheless it illustrates an underlying philosophy which was at least in the German air by the time. This is that the roots the equation $a_0x^n + a_1x^{n-1} + \cdots + a_n = 0$ are among the branch points of the curve, the branch points can be taken as moduli for the curve, and they in turn are to be expressed as theta null values; but the details require a careful exposition.³ This philosophy underlies Thomae's paper on theta functions and was applied explicitly to Jordan's work by Lindemann in two later papers.⁴ On either view, their application to the solution of equations was a deep illustration of the importance of the theory of elliptic and hyperelliptic functions.

This success for the modular function did not, of itself, promote a geometric viewpoint on questions about complex functions. Nonetheless, different questions

²Klein (1879) showed that the Galois group of the modular equation was much smaller than the Galois group of the general polynomial equation of degree 7, but the reduction process would solve any 7th degree equation whose Galois group was that of the modular equation, thus explaining Kronecker's result.

³Here suppressed, see Jordan (1870) and, for a modern treatment, Umemura, in (Mumford, 1984, 3.261–3.272).

⁴See Thomae (1870a); Lindemann (1884) and Lindemann (1892).

about it did.⁵ To see why, it is helpful to look back at Riemann's ideas about algebraic functions and the problems they were causing. As we saw in Chap. 5, in his lectures Riemann advocated studying elliptic functions by thinking of them as defined on a torus and then looking at a corresponding region of the complex plane. By cutting open the torus a parallelogram was obtained and paths on the torus opened out to paths which extended into copies of this parallelogram. These copies tiled the plane, the vertices of the parallelogram defined a lattice, and functions on the torus lifted to doubly periodic function functions on the plane. In his celebrated memoir on Abelian functions he proposed that any algebraic curve of higher genus be cut by curves into a polygon which could be thought of as lying in the complex plane. Paths on the surface were then to be thought of as paths in the polygon whose sides were glued together along the cuts. The idea that copies of this polygon could be taken which tiled a region of the plane was not pursued. Indeed, Riemann's ideas were difficult to follow in any given case, and for a time they were rejected in favour of more rigorous algebraic approaches, until Klein and Dedekind independently sought to revive them in the late 1870s. The spur was the formidable difficulties that otherwise confronted anyone seeking to analyse questions about elliptic functions, and it is striking how mathematicians found themselves driven to the same ideas that Gauss had come to more than 50 years before and which were shortly to be published.

8.2.2 Dedekind's Modular Function

Dedekind's motivation in his (1877) was precisely to elucidate the theories of Hermite and Kronecker. He wished to free this theory from the theory of elliptic functions themselves, which was up to this time the only way in which it had been approached. He succeeded and showed how the theory depended only on a function v , which he called the *valenz* function, and which had simple invariance properties:

$$v\left(\frac{\alpha\omega + \beta}{\gamma\omega + \delta}\right) = v(\omega), \quad (8.1)$$

where $\begin{pmatrix} \alpha & \beta \\ \gamma & \delta \end{pmatrix}$ is any matrix with integer entries and determinant 1. He showed that the v -function takes every value once and only once on the region R_1 , called its fundamental domain, depicted in Fig. 8.1:

$$R_1 = \omega : |\omega| > 1, -1/2 < \operatorname{Re}(\omega) < 1/2, \operatorname{Im}(\omega) > 0, \quad (8.2)$$

together with the boundary points for which $\operatorname{Re}(\omega) = 0$ (Fig. 8.2).

⁵Discussed at length in (Gray 2000a, Chap. 4).

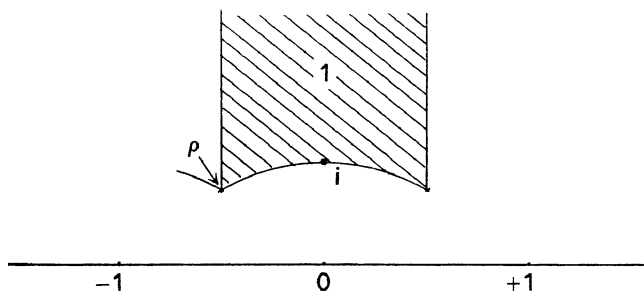
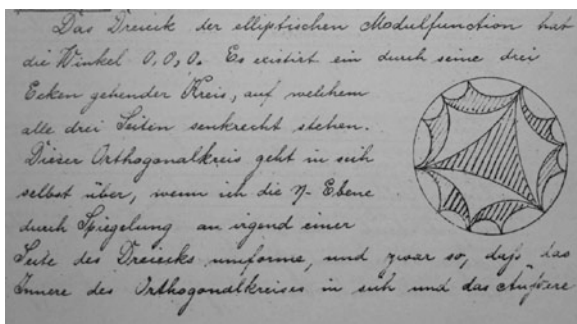


Fig. 8.1 A fundamental domain for the J function (Schoeneberg *Elliptic modular functions*, Springer ©1974, p. 17)

Fig. 8.2 The triangle for the elliptic modular function (Klein, *Ueber die hypergeometrische Function. Vorlesungen 1893/94*, p. 531)



Not sharing the hostility of the older generation to his late friend Riemann's ideas, which he had just been involved in editing, Dedekind deduced the existence of such a function from the Riemann mapping theorem, which he applied to the region R_1 and extended to the whole of the upper half plane by the Schwarz reflection principle. Invariance considerations allowed him to obtain the differential equation that the function v satisfies, and he established (1877, 190) that $\omega = \text{const.} v^{-1/3}$. $(1-v)^{-1/4} \left(\frac{dv}{d\omega} \right)^{1/2}$ satisfied the hypergeometric equation

$$v(1-v) \frac{d^2 \omega}{dv^2} + \left(\frac{2}{3} - \frac{7}{6} v \right) \frac{d\omega}{dv} - \frac{1}{144} \omega = 0.$$

So the invariance idea led Dedekind to a thorough grasp of his new function, in terms not unlike those of Gauss. Dedekind then introduced a function nowadays called after him, the Dedekind eta function:

$$\eta(\omega) = \text{const.} v^{-1/6} (1-v)^{-1/8} \left(\frac{dv}{d\omega} \right)^{1/4} \quad (8.3)$$

for a suitable constant, which he determined so that $\eta(\omega) = e^{2\pi i\omega/24}$ at $\omega = \infty$. It was the properties of the function η that enabled Dedekind to recapture the moduli k and k' as functions of ω and to show that k can be written as the familiar Jacobian series in $q = e^{\pi i\omega}$. But, he said with regret (p. 195), he still needed the theory of theta functions to represent η as a function of ω .

On this basis, Dedekind gave an elegant account of how two elliptic functions whose moduli are simply related are themselves related, answering such questions as: what happens if the ratio of their periods is doubled or multiplied by any prime number? These were traditional questions in the subject, but Dedekind's approach was the first direct one and rapidly became standard. To study the modular equations, for example, he took for each integer, n , an integer matrix $\begin{pmatrix} a & b \\ c & d \end{pmatrix}$ of determinant n and defined

$$v_n(\omega) = v\left(\frac{c + d\omega}{a + b\omega}\right). \quad (8.4)$$

There are, he showed, $\psi(n) = n \prod_{p|n} \left(1 + \frac{1}{p}\right)$ of these matrices, and

$$f(z, v) = \prod_{r=1}^{\psi(n)} (z - v_n)$$

is a polynomial in z of degree $\psi(n)$ with coefficients that are single-valued functions of $v = v(\omega)$. This is the multiplier equation for the integer n .

Klein's contribution in his (1879) was to change the name of Dedekind's function from v to J , the name it retains to this day, and to stress the group theoretic aspect of all of this material. Matrices of the form $\begin{pmatrix} \alpha & \beta \\ \gamma & \delta \end{pmatrix}$ with integer entries and determinant 1 form a group (called the modular group and denoted Γ) and there are many subgroups to be obtained by imposing simple congruence conditions on the integer entries. For example, Gauss's matrices (see Eq. (1.69)) for which α and δ are odd and β and γ are even form the subgroup of matrices

$$\begin{pmatrix} \alpha & \beta \\ \gamma & \delta \end{pmatrix} \equiv \begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix} \pmod{2}. \quad (8.5)$$

He also discussed how the matrices act geometrically on the complex plane. The effect of the modular group is to move points around as described by the formula

$$z \mapsto \frac{az + b}{cz + d}, \quad \begin{pmatrix} a & b \\ c & d \end{pmatrix} \in \Gamma.$$

It therefore moves its fundamental domain around en bloc and tessellates the upper half plane with it. Klein described in some detail how subgroups have fundamental

projective plane ($x^3y + y^3z + z^3x = 0$).⁶ It follows that the curve is mapped to itself by a subgroup of $SL(3; \mathbb{C})$, the group of 3×3 matrices with complex entries and determinant one, because this group acts on the plane. This group, the group of symmetries of the curve, is isomorphic to the quotient group Γ/Γ_7 and is therefore finite, so Klein had obtained a finite subgroup of $SL(3; \mathbb{C})$. It happened that Jordan in [Jordan \(1878\)](#) had recently attempted to classify all such groups by a vigorous use of Sylow theory, but an arithmetical error had led him to miss this one, so Klein had actually found a new group. “Poor Camille Jordan!” Klein commented ironically in a letter to Brioschi on May 5, 1878. “In his latest enumeration of the finite groups of ternary linear substitutions he has once again forgotten the most interesting one, the one with 168 substitutions”.⁷ Klein therefore analysed the group in considerable detail, listing all its subgroups and the orders of its elements. Among the subgroups he wrote down was a well-known group of order four later called after him by his students, but he missed the most interesting property of the group itself, namely that it is simple.

In 1881 Klein’s gifted student Adolf Hurwitz celebrated his 22nd birthday by obtaining his doctorate with a thesis on elliptic modular functions (see [Hurwitz 1881](#)). The thesis showed a confident mastery of Klein’s new ideas and applied them to questions about elliptic functions that go back to Eisenstein’s (1847a), a work Hurwitz explicitly recalled, noting that it included Weierstrass’s theory of the $\wp$ function. Hurwitz had earlier studied under Weierstrass (see Sect. 6.5) and Klein regarded him as the messenger, bringing the news of the current version of Weierstrass’s theory to Leipzig where Klein was in 1881. His thesis is also notable for the confidence with which it uses both Riemannian and Weierstrassian ideas, and for some of its trenchant historical comments.

[Hurwitz \(1881, 18\)](#) defined a modular function to be a single-valued analytic function (in the sense of Weierstrass, i.e. admitting a power series expansion) which is unaltered when the argument is transformed by any element of a given group. In symbols: $h\left(\frac{\alpha\omega+\beta}{\gamma\omega+\delta}\right) = h(\omega)$ for all $\begin{pmatrix} \alpha & \beta \\ \gamma & \delta \end{pmatrix} \in \Gamma$. A modular form of dimension k is a single-valued analytic function of dimension k in two variables ω_1 and ω_2 which remains unaltered under the corresponding transformations of the variables: $h(\alpha\omega_1 + \beta\omega_2, \gamma\omega_1 + \delta\omega_2) = h(\omega_1, \omega_2)$.

The quotient of two modular forms of the same dimension will be a modular function in $\omega = \frac{\omega_1}{\omega_2}$. Hurwitz defined a particular family of modular forms

$$G_k = \sum_{m=-\infty}^{m=\infty} \sum_{n=-\infty}^{n=\infty} \left(\frac{1}{m\omega_1 + n\omega_2} \right)^{2k}, \quad k > 0,$$

⁶See [Gray \(2000a\)](#).

⁷Between 1877 and 1878 Klein and Brioschi corresponded intensively on the solution of the quintic equation and related matters. Klein’s letters to Brioschi are kept in the library of the Politecnico in Milan.

where the term with $m = 0 = n$ is to be omitted. The functions $G_{1/2}$ and G_1 are not modular forms, because the convergence of the series for $G_{1/2}$ and G_1 is conditional on the order of the terms, but for $k = 2, 3, \dots$, the G_k are modular forms of level $2k$. Hurwitz now applied what he learned from Klein to what he had heard from Weierstrass the previous year. Elegant manipulations, modelled on Eisenstein's, enabled him to write the G_k as power series in $q = e^{i\pi\omega}$, in the sense that, for example,

$$3G_1 = \left(\frac{\pi}{\omega_2}\right)^2 \left(1 - 24 \sum k \left(\frac{q^{2k}}{1 - q^{2k}}\right)\right).$$

From such series Hurwitz (1881, 22) deduced that $g_2 = 60G_2$ and $g_3 = 140G_3$, where g_2 and g_3 are Weierstrass's invariants for elliptic integrals of the first kind. Moreover, $G_1 \cdot \omega_2$ is, up to a factor, the period of an elliptic integral of the second kind which Weierstrass had denoted 2η . In this way Hurwitz incorporated Weierstrass's theory of elliptic functions in the Kleinian framework.

He then investigated, in a long argument that started from some simple formulae that Eisenstein had introduced for the same purpose, the way the function G_1 transformed under the generating elements of the modular group. He concluded that the function

$$\Delta(\omega_1, \omega_2) = \left(\frac{2\pi}{\omega_2}\right)^{12} q^2 \prod_{k=1}^{\infty} (1 - q^{2k})^{24},$$

first studied by Dedekind, is a modular form (of level 12). It is closely related to Dedekind's η , which, raised to the 24^{th} power is $q^2 \prod_{k=1}^{\infty} (1 - q^{2k})^{24}$.

Hurwitz now turned to geometric arguments about the tessellation of the upper half plane by triangles under the action of the modular group and appealed to the Riemann mapping theorem ("Riemann's intuitions", as he (1881, 27) put it, offering Dedekind's argument, (Dedekind 1877, 183) for comfort, to deduce the existence of the J -function. He then showed that $\frac{g_2^3}{\Delta}$ is a constant multiple of the J -function and that $\frac{g_3^2}{\Delta}$ is a constant multiple of $J - 1$, whence he obtained the familiar relationship

$$g_2^3 - 27g_3^2 = \Delta.$$

This freed Dedekind's theory from its one point of dependence on the theory of theta functions. Because the J -function was well understood, Hurwitz had in principle completed the process of obtaining elliptic functions with arbitrary periods ω_1 and ω_2 (provided, of course, that ω_1/ω_2 is not real) thus resolving a problem that had been left open by Jacobi (see Sect. 4.2.1 above). However, his appreciation of this point is unclear and is best indicated by his reworking of the material in his (1904). This is a much shorter presentation, based on essential simplifications he had discovered in the intervening 23 years. He quickly established that $\wp^2(u) = 4\wp^3(u) - g_2\wp(u) - g_3$, and observed (1904, 594) that it

was fundamental in the theory of the Weierstrass function to show that periods ω_1 and ω_2 can always be found so that the invariants g_2 and g_3 take prescribed values (such that $g_2^3 - 27g_3^2 \neq 0$). Usually, he said (Sect. 6), this is established directly from the theory of the differential equation for the Weierstrass function, however, it is deduced more quickly and simply from the theory of the J -function. Indeed he showed that it is enough to be able to solve the equation

$$J(\omega) = \frac{g_2^3}{g_2^3 - 27g_3^2},$$

but it is well known that this equation has a solution, and indeed one in every copy of the fundamental domain of the modular group.

8.2.3 Picard's Theorems

Geometric function theory speedily proved its worth in other aspects of complex function theory. Its most celebrated success was in the elucidation of an essential singularity. The distinction between types of singularity had been introduced by Weierstrass in the 1860s and shown to be of fundamental importance (see Sect. 6.7.1). The Casorati–Weierstrass theorem showed that in any neighbourhood of any of its essential singularities a function comes arbitrarily close to any pre-assigned value, and so no value can be attached by a limiting argument to the reciprocal of the function at the essential singular point. In a very short paper (1879a) Picard showed, using the techniques of Dedekind's geometric function theory, that an entire function cannot omit two distinct finite values without reducing to a constant (Fig. 8.4). This result has become known as Picard's little theorem. In his (1879b) he refined the result to show that an entire function that is not a polynomial takes every finite value infinitely often except perhaps for one. Then he showed in his (1879c) that the little theorem can be deepened considerably to show that a function with only finitely many essential singularities takes every value infinitely often in any neighbourhood of an essential singular point, with the exception of at most two values. This result has become known as Picard's big theorem.⁸

Picard's theorems were to be a source of great inspiration to a generation of French mathematicians among whom he stood as first among equals. He proved the little theorem by arguing by contradiction. If a non-constant entire function existed that did not take the values a or b , then the function $G(z) = \frac{F(z)-a}{b-a}$ would be an entire function never taking the values 0 or 1. Compose it with the modular function $\omega = h(z)$ where ω is k^2 considered as a function of the ratio $\frac{iK'}{K}$. Away from the

⁸The following year, Picard published an extended version of this material in his (1880).

Fig. 8.4 Émile Picard (*Acta Mathematica Table générale des tomes 1–3, 1882–1912, p. 163*)



points $z = 0, 1$ (and ∞) h is never zero and its imaginary part is always positive. So the composite $hG(z)$ always has positive imaginary part. Although h is a many-valued function, the composite hG is single-valued. For, if it were not there would be a closed path in the z -plane the image of which under G would be a closed path surrounding either 0 or 1. But then by contracting the first path its image would contract onto a path containing 0 or 1, and by assumption G does not take those values. So hG is single-valued, and by composing it with a Möbius transformation that maps the upper half plane onto a disc one has a bounded function. But, by Liouville's theorem, such a function is constant. This is a contradiction, and so the theorem is proved.⁹

To prove that a function f with finitely many essential singularities takes every value (except perhaps two) infinitely often in any neighbourhood of any of these essential singularities, Picard first considered, as he may, a neighbourhood where the function had no poles. This he reduced to the situation of the little theorem, and he deduced that there is at most one finite value that is not taken infinitely often. When the function does have infinitely many poles, Picard used the Weierstrass representation theorem to write the function as a quotient of two entire functions G_1 and G_2 having an essential singularity at the point $z = \alpha$ but that are otherwise never infinite in a neighbourhood of that point. He supposed that the function f does not take the values a and b infinitely often. Then

$$G_1(z) - aG_2(z) = (z - \alpha)^m e^{P(z)}, \quad G_1(z) - bG_2(z) = (z - \alpha)^n e^{Q(z)},$$

and so, after a little algebra,

⁹Picard missed the trick of using Liouville's theorem and proved a similar theorem about harmonic functions instead.

$$f(z) = \frac{ae^{Q(z)} - b(z - \alpha)^{m-n}e^{P(z)}}{e^{Q(z)} - (z - \alpha)^{m-n}e^{P(z)}}.$$

But this expression takes any value $c \neq a, b$ infinitely often, because the equation $f(z) = c$ can be written in the form

$$(z - \alpha)^{n-m}e^{Q(z)} - P(z) = \frac{b - c}{a - c},$$

and the expression belongs to the class already studied (and cannot vanish). Therefore $\frac{b-c}{a-c}$, and therefore c , takes infinitely many values.

As Picard himself pointed out, the importance of this theorem is that it divides entire functions with isolated essential singular points into three kinds: those for which the function takes every value in a neighbourhood, or every value except one, or every value except two.¹⁰ As examples of the latter phenomena he cited $\frac{1}{\sin 1/z}$, which is never zero in a neighbourhood of $z = 0$, and $e^{1/z}$, which never takes the values 0 or ∞ in a neighbourhood of $z = 0$. Indeed, his theorem opened up the whole subject of the local study of an analytic function near its essential singularities, and we shall see in Sect. 8.5 below how rich this area soon became.

8.2.4 Poincaré and Automorphic Functions

Another indication of the power of geometric function theory was Poincaré's increasingly profound elucidation of the theory of algebraic curves, culminating in his conjecture (Poincaré 1883d) that all but the simplest of them arise, like Klein's curve $x^3y + y^3z + z^3x = 0$ mentioned above, from decompositions of the upper half plane into a tessellation by polygons. By simultaneously introducing new classes of complex functions defined on the upper half plane and taking the same value at all corresponding points of the tessellation, Poincaré made it plausible that the corresponding algebraic curve could be parameterised. This extended the theory of elliptic functions to cover every curve and so suggested the existence of a large class of new complex functions. Formidable difficulties nonetheless stood in the way of making the new functions useful and comprehensible (Fig. 8.5).

Poincaré had been led to his conjecture by work on the theory of linear ordinary differential equations that he was writing up for a prize competition of the Académie des sciences in Paris. As we saw in Sect. 5.8, Riemann wrote a highly innovative paper on the hypergeometric differential equation:

$$z(z-1) \frac{d^2w}{dz^2} + (c - (a+b+1)z) \frac{dw}{dz} - abw = 0. \quad (8.7)$$

¹⁰Picard, *Traité d'analyse* 2, 122.

Fig. 8.5 Jules Henri Poincaré (*Acta Mathematica* Table générale des tomes 1–3, 1882–1912, p. 164)



This was the first time the equation had been studied from the complex point of view, apart from an unpublished study by Gauss. Group theory remained implicit in Riemann's presentation, but was soon to be made explicit. Since the hypergeometric equation is of the second order, its solutions are expressible as a linear combination of two basic solutions. The analytic continuation of these two around each singular point yields more branches of the solution that depend linearly on the original choice. This dependence is captured by a matrix of constants. The group generated by these matrices at the three singular points is called the monodromy group of the equation; it is a quotient group of the fundamental group of the Riemann sphere punctured at the three singular points. Riemann's work therefore established that the monodromy group determines the differential equation and its solutions.¹¹

These ideas were taken up by Poincaré, to whom explicit mention of group theory is due.¹² He was responding to papers by Fuchs, who, as we saw in Sect. 7.3.1, had generalised Riemann's theory to a class of differential equations with solutions that have no essential singularities. Fuchs had then become interested in the behaviour of the quotient of two solutions to a second order linear differential equation. He asked himself when, in particular, does the quotient have a single-valued inverse on some domain? Such behaviour would extend the theory of elliptic functions. His

¹¹Hilbert's 21st Paris problem asks: given a quotient group of a fundamental group of a punctured Riemann surface, is there a differential equation having its singular points only at the punctures and the given group as its monodromy group? Although it was answered affirmatively by G.D. Birkhoff in his (1913a) and by Plemelj (1908b) and their work stood for over 70 years, it was shown to be flawed by Bolibruch. See Anosov and Bolibruch (1994) and for an indication of the history, (Gray 2000b, 139–140).

¹²First published as (Poincaré 1881a,b,c), see Gray (2000a).

published account was sometimes wrong, as Poincaré speedily saw, and an amicable correspondence between Poincaré and Fuchs ensued.¹³ For the hypergeometric equation Poincaré could show that there was a single-valued quotient whenever the quantities a , b , and c determined monodromy matrices of suitable finite orders. The inverse of the quotient function then defined maps of a triangular region onto a half-plane, and analytic continuation showed that, depending on their angles, the triangles tessellated either the sphere, the plane, or the disc. Such conclusions had been reached earlier by Schwarz, as we saw in Sect. 7.3.2, but the young Poincaré was quite poorly aware of current literature.¹⁴ Instead, because he wanted to acknowledge the influence Fuchs's work had had on him he called the functions he had discovered Fuchsian functions.

In another of his writings (1908b) Poincaré sketched out the process of discovery. He pursued the analogy with theta functions and parallelograms in the plane. A theta function succeeds in being periodic in one direction and fails in a precise way in the other direction defined by the sides of the parallelogram. So Poincaré looked for functions, which he called theta-Fuchsians, that would have similar properties. If Γ denotes the group generated by reflections in the sides of the non-Euclidean triangle, and γ is a generator of Γ , then he looked for functions, $F(z)$, such that $F(\gamma z)$ was equal to $F(z)$ multiplied by some factor depending on γ . He obtained such functions by writing them down as power series of the form $p(z) = \sum_{\gamma} g(\gamma z)$ where g is some function defined on the original non-Euclidean triangle and the summation is over all elements of the group Γ . These are trivially invariant, $p(z) = p(\gamma z)$, but will not usually converge. To make them converge he multiplied them by a factor $h(z)$ which went to zero fast enough as the non-Euclidean triangle moved out towards the boundary of the disc. Now the series $\sum_{\gamma} g(\gamma z)h(z)$ was no longer invariant, but quotients of products of them could be manufactured which were. Thus were created what Poincaré called Fuchsian functions, and likewise a class of groups, Γ , that he called Fuchsian groups.

There the matter rested for a while in 1880. Poincaré wrote up his work and submitted it to the Académie and resumed his activities as a teacher at the École des Mines in Caen. To show that the circular-arc triangles in the disc did not overlap Poincaré had used a simple geometrical projection that mapped them onto straight-sided triangles in another disc. This picture must have popped into his mind when on a field trip, and changing buses at Coutances, as he was later to explain (1908b, 51): “The idea came to me without anything in my previous thoughts having prepared me for it that the transformations I had used to define Fuchsian functions were identical with those of non-Euclidean geometry”. The realisation was surely that he had seen the straight-sided figure before: it is the Beltrami (1868a) model of the non-Euclidean “plane”. Consequently his simple geometrical transformation applied in reverse means that the circular-arc triangles are depicted conformally as triangles

¹³See Poincaré, *Oeuvres* 11, 13–25.

¹⁴He knew of Fuchs's work because Fuchs was a friend of Hermite.

in the non-Euclidean disc. This was an important breakthrough in the study of non-Euclidean geometry, and one that, however surprisingly, Klein had not made.

The introduction of non-Euclidean geometry enabled Poincaré to think much more productively about his new functions, and over the summer he wrote three lengthy supplements to his original memoir.¹⁵ Another sudden flash of inspiration, this time while walking on the cliffs, enabled Poincaré to show that more general polygons arise in the same way from differential equations with more singular points.¹⁶ Now for the first time it was clear that just as elliptic functions could be studied by passing to a parallelogram that tiled the plane, whole new classes of functions could be defined by looking at polygons that tiled the non-Euclidean “plane”. All that he had done for non-Euclidean triangles could be done for polygons. Poincaré’s introduction of the tools of non-Euclidean geometry led him to the recognition that Riemann surfaces arise whenever a suitable group acts on either the sphere, the plane, or the non-Euclidean disc in such a way that a polygon is moved around en bloc and never overlaps itself except neatly along its edges. By identifying the edges which overlap in this way, the polygon is made into a Riemann surface. When the sphere is chosen, surfaces of genus zero are obtained, when the plane is chosen tori are obtained, and when the disc is chosen surfaces of all higher genera are obtained.

An even deeper result was to follow. As Riemann had shown, the surfaces of genus g are topologically equivalent but are not analytically equivalent. Instead, if $g > 1$, there is a space of inequivalent surfaces of dimension $3g - 3$. What about the polygons? A polygon of $4g$ sides equal in pairs will yield a surface, and a simple counting argument showed Poincaré that there are $3g - 3$ inequivalent polygons. So he conjectured that every Riemann surface arises in this way. This is a remarkable result, which for the first time conferred an intrinsic geometry on every Riemann surface. But a naive counting argument is far from a proof, as even Poincaré recognised, and although he gave a highly insightful sketch of a proof in 1883, the theorem was not to be rigorously proved until Koebe and Poincaré independently managed to do so in 1907 (see Sect. 8.3). By then the question had acquired a further importance, because it was connected to attempts to prove the Riemann mapping theorem. In view of the obscurities in much of this work it is interesting to see that before any of the attempts at a proof were published, and with little more than (Poincaré 1882a) to go on, Weierstrass wrote to Sonja Kovalevskaya on 14 June 1882 to say that among the young French mathematicians “Poincaré is in my opinion the most given to mathematical speculations, if only he could not fragment his extraordinary talent too much and let his researches ripen. The theorems on algebraic equations in two variables, and on linear, differential equations with algebraic coefficients that he has published in the *Comptes rendus* are truly impressive; they open new paths in analysis that will lead to unexpected

¹⁵First published as (Poincaré 1997).

¹⁶See (Poincaré 1908b, 52). He still did not win; the prize went to Halphen for a memoir on differential equations and invariant theory (Halphen 1884).

results” (see Bölling 1993, 267 and Mittag-Leffler 1923b, 183). We shall discuss these matters shortly, but first we shall see how Klein responded to these new ideas as they began to emerge in notes in the *Comptes rendus*.

When Poincaré began to publish his discoveries, they rapidly caught the attention of Felix Klein, and a correspondence and something of a one-sided competition began between the two. This has often been written about¹⁷ and the correspondence published in many places.¹⁸ But it is a story that bears repetition because of its importance in the history of mathematics. In 1881 Klein was 32, at the height of his powers and emerging as the leading German mathematician of his generation. He was extremely ambitious, had been a full professor already for 9 years by then, and had cultivated his connections with many of the leaders of his profession in Germany and abroad. If relations with the grand old men in Berlin were cool, time was on his side and they had no obvious successor of their calibre.¹⁹ Poincaré was 26 and for all his striking gifts he had published nothing except his doctoral thesis; Klein, for one, had never heard of him.

The two differed in other ways. Klein was the very model of the well-read German professor, Poincaré had large gaps in his education, one of which, it emerged, was the Riemann mapping theorem. One must suppose that at some stage he had decided to immerse himself in his own thoughts, but Klein sought out the stimulus of other people’s work. Klein set himself high standards and measured others by them, which was one reason why he was to pass slighting comments on Fuchs all his life. He deplored the fact that Poincaré had called functions Fuchsian that should, with more justice, have been called Schwarzian. Poincaré was more casual and more generous.²⁰ And moreover, in 1881 Klein’s research on modular functions was blocked and he was looking elsewhere for problems to solve. Poincaré was just waking to the full flood of his invention.

In the letters Klein emerges as the teacher, Poincaré as the keen student. What is your definition of genus of an algebraic curve, he asked in one letter, only to write the next day “I see you have defined it further on in a paper of yours” (Letter VIII, 5 July 1881). Never one to be ashamed of his ignorance he wrote openly about his work and Klein was startled as much by how little Poincaré knew as by how much he had discovered. For once Klein, who was always open to the criticism that he had too great a distaste for details, found himself up against a mathematician of much

¹⁷See Gray (2000a) for an account and Poincaré (1885a) for an English translation of Poincaré’s major papers (1882c,d,e) and an interesting commentary by J. Stillwell.

¹⁸See, e.g., Klein, *Ges. Math. Abh.* 3, 587–621 and Poincaré, *Oeuvres* 11, 26–65.

¹⁹Frobenius was a late developer.

²⁰Mittag-Leffler raised the subject of Schwarz’s feelings in a letter he wrote to Poincaré on 18 July 1882, where he observed that although Fuchs was full of admiration for Poincaré’s “beautiful discoveries” Schwarz was almost “suffocating with anger”. Poincaré replied on 27 July 1882 that he had re-read the relevant papers and concluded that he could do nothing to calm Schwarz down, because Schwarz was really angry with himself “for having had an important result in his hands and not profiting from it. And I can do nothing about that”. As for the choice of name, Poincaré felt he had nothing to add. See Nabonnand (1999), 100–101.

greater imprecision and capacity for downright error. But the German's erudition seems to have kept him back. Having struggled hard to master Riemann's theory of algebraic curves as surfaces spread out over the Riemann sphere, and to connect that to the projective geometry of his mentor Clebsch, he had not seen that the pictures he drew of many-sided polygons in the disc were non-Euclidean pictures. It was Poincaré who saw first that this was the way to generalise the theory of elliptic functions to functions on an arbitrary Riemann surface. Nor had Klein thought of differential equations in this connection, which were always for Poincaré an integral part of the story.

Once this insight about non-Euclidean geometry was conveyed to him, Klein moved fast to catch up. He solicited an article from Poincaré on his ideas (Poincaré 1882d) which he accompanied with another (Klein 1882b) showing how it connected to Klein's own. One wonders if Poincaré appreciated this; certainly he never published in Klein's journal, the prestigious *Mathematische Annalen*, again. Klein continued to write to Poincaré, speculating about the limits to which their new insight could take them.

For the circular-arc polygon to be a non-Euclidean polygon, it must have its sides perpendicular to the boundary of the non-Euclidean disc. What, he asked Poincaré, of a circular-arc polygon that was not of this form? No problem, came the reply a few days later in the form of a note to the *Comptes rendus* (Poincaré 1881d). Think of the polygon as drawn on the surface of a sphere which bounds the ball carrying three-dimensional non-Euclidean geometry. It will be cut out by spherical caps meeting this boundary sphere at right angles, and these represent non-Euclidean planes. Technical difficulties aside, everything that has been said so far will simply generalise to the three-dimensional case. There will be a group moving around the non-Euclidean polyhedron bounded by the spherical caps and generated by reflections in its sides. There will be complex functions definable initially on the polygon on the boundary sphere for which these reflections define analytic continuation to a larger domain, and suitable combinations of these functions will have the property of being invariant under the action of the group. Then, in an impish moment, Poincaré saw how to respond to all Klein's carping about the name Fuchsian: in his (1881d, 22) he called the new functions Kleinian "because it is to M. Klein that one owes their discovery". Klein objected, of course, but, replied Poincaré (letter XIX, 4 April 1882, quoting the famous Gretchen scene from Goethe's *Faust*, "name is sound and smoke").²¹

Poincaré was struck by the connection between non-Euclidean polygons and Riemann surfaces. Was the coincidence of the number of free parameters in each case just that, a coincidence? The same group may be obtained from many different polygons, and it is not at all evident that the spaces of all (equivalence classes of) groups can be properly defined. If it can, and if it has the same dimension as the space of all inequivalent Riemann surfaces of the given genus (another object requiring careful definition), it still would not follow that the two spaces were

²¹Fuchsian and Kleinian functions are collectively called automorphic functions.

isomorphic. It is easy to go from polygons and groups to surfaces, but to go from surfaces to groups is not so straightforward. Can one be sure that this way round is a surjective map? In what sense, if any, is it analytic? Writing in 1884, Poincaré pointed out that an analytic map between complex varieties of the same dimension will be an onto map provided the surfaces have no boundaries. But what if the space of all groups has a boundary? “This is a difficulty one cannot overcome in a few lines”.²²

Nonetheless, both men were able to push their ideas as far as acquiring the moral certainty that the correspondence was indeed an isomorphism and so the uniformisation theorem was true, and both published suitably conjectural versions of this important claim. But for Klein, this was the end of the road. The effort of the previous 15 months cost him his health. In September 1882 he suffered a nervous collapse; colleagues who saw him push himself through his teaching commitments urged him to take a much needed rest. In the end he did, but he never recovered his original fertility as a researcher. Instead he directed ever more ambitious programmes of teaching, built up the best mathematics department in the world for a generation when he finally took control in Göttingen, edited the excellent and truly international *Encyclopädie der mathematischen Wissenschaften*, and was a major influence on the growth of mathematics in both Britain and America (see Parshall and Rowe 1994).

Poincaré was left to pursue these ideas alone, but what had been just about within his reach in the two-dimensional case, the Fuchsian functions and their associated Fuchsian groups, was beyond even him in three dimensions. This situation was to persist for two generations. Other mathematicians, notably Picard and Appell, caught on to the Fuchsian case, and even found remarkable extensions of it to complex functions of several variables, but the Kleinian case guarded its secrets until after the Second World War. What could have been the start of a whole new theory of functions that generalise elliptic functions was to wait for new tools and new approaches, and these were to be developed not in France, but in Germany. The bibliography in Ford’s *Automorphic Functions* (1929) shows very clearly how much of what was done was due to Klein, on his recovery from his mental exhaustion, and his able assistant Fricke. But even so, the theory that emerged was a theory of considerable generality but much less detail. Fuchsian and Kleinian functions of particular kinds did not enter the lexicon of functions available to express solutions to a variety of problems. Despite Poincaré’s claim that these new functions solved all types of linear differential equation they remained largely unused.²³

What little work was done with Poincaré’s ideas only serves to underline the point. In 1886 Heinrich Weber was the first mathematician to take up Poincaré’s ideas. He discussed Poincaré’s work using an example where the fundamental

²²In (Poincaré 1884a, 332).

²³And perhaps little understood. For example, Mittag-Leffler wrote to Hermite on 13 October 1884 that “I have not understood Poincaré at all for a very long time and he really expresses himself in a very obscure manner”. Quoted in (Dugac 1984, 203).

domain is the region outside $2n$ circles which are all outside each other. The group is generated by n substitutions that identify these circles in pairs. The space that results when these identifications are made is an $(n + 1)$ -fold connected Riemann surface, so it carries n everywhere finite many-valued functions, corresponding to the n integrals of the 1st kind. In 1891 the English mathematician William Burnside took up this example and showed that these n integrals yield n quasi-automorphic functions on the z -plane, which he obtained in terms of theta-Fuchsian functions (Burnside 1891). This connected the theta-Fuchsians with the integrals of the first kind. He then derived similar results for integrals of the 2nd and 3rd kinds. He was also able in many cases to establish the convergence of the appropriate Poincaré series, which Poincaré himself had said do not converge. This led him to define a new kind of theta function and to study its properties under the action of the corresponding Fuchsian group. He studied their integrals, basing his approach on Neumann's (1884) and built up a theory of automorphic functions in this setting. Burnside then wrote a second paper (Burnside 1892), in which he compared his work with that of Weber and showed how it connected to Klein's theory of *Primformen*, which we discuss below in Sect. 8.3.3.

In the absence of any conclusive evidence it is at least plausible to suggest that these papers helped Burnside towards what became his consuming interest and his major contribution to mathematics, the representation theory of finite groups, which he took up in 1893. At Cambridge, even though he had graduated second Wrangler in his year in 1875 and won a Smith's Prize, he was best remembered for his rowing.²⁴ In 1887, by which time he had published only four papers, he obtained the very well paid position of professor of mathematics at the Royal Naval College, Greenwich, where he remained until his retirement in 1919. The College offered him the chance to take up research at an international level in topics remote from his teaching (and for that matter any nautical application) and it was firmly seized. From 1887 he published on average 4 papers a year and also wrote the two rather different editions of his book *Theory of groups of finite order* (1897 and 1911).

The third mathematician to take up Poincaré's ideas was Ferdinand Lindemann in Lindemann (1899). He observed that the Weierstrassian theory of doubly periodic functions starts with an integral of the 2nd kind, by integration gets the σ -function, and by differentiation the Weierstrass $\wp$ -function. By contrast, Poincaré had started with theta-Fuchsian functions, which raised problems of convergence (if only in the simplest case). Lindemann, like Burnside before him but independently, showed that these problems could be overcome (by Eisenstein-type arguments). This opened the way to integrals of the second kind and a theory of algebraic functions, and thence by integration to integrals of the first and third kinds. Lindemann throughout confined his attention to finite polygons, but took his account all the way to Poincaré's rather obscure theory of Zeta-Fuchsian functions and their role in the solution of linear ordinary differential equations.

²⁴His *Times* obituary dwelled on this point to the exclusion of mathematics.

One further result is worthy of mention. In 1887 Picard proved results about the singular points of Fuchsian functions used to parameterise algebraic curves of genus greater than 1. He gave two arguments to show that such functions cannot have an isolated essential singular point.²⁵ The first of these proceeded via an analysis of the elements of the corresponding Fuchsian group, the second of these gave a more direct argument in the hyperelliptic case and then used an argument about the branching of a general Riemann surface that Picard had learned from Hurwitz.

On the other hand, the uniformisation theorem and the Riemann mapping theorem, conjectured both of them still in 1883, were to be a potent stimulus for future research, as we shall now see.

8.3 The Riemann Mapping Theorem and Uniformisation

In Sect. 21 of his paper of 1851, Riemann had claimed that every simply connected domain with at least two boundary points can be mapped one-to-one and conformally onto the interior of the unit disc, in such a way that the map extends to a map on the boundaries. It becomes unique when it has to send a given interior point of the domain to the centre of the disc and a specified point on the boundary to a specified point on the unit circle bounding the disc. His motivation in proclaiming this theorem was to extend Dirichlet's general existence theorem to functions with singularities. The consequences of the theorem for his theory were profound, for they established that every such domain is the domain of complex functions, and that moreover there are functions on such a domain that cannot be extended beyond the boundary of the domain.

Riemann's claim was usefully broken into two. The interior Riemann mapping theorem states that the interior of a simply connected region having at least two boundary points can be mapped one-to-one and conformally onto the interior of a unit disc. The unrestricted Riemann mapping theorem is the claim that the map so obtained extends to a continuous map of the boundaries. As put, this claim requires an immediate restriction that the boundary of the region is a simple closed Jordan curve. As we shall see, it was only with the work of Osgood and Carathéodory that clarity about the nature of the boundary was obtained. Schwarz's alternating method left the nature of the boundary vague, and Poincaré's method made certain simplifying assumptions about it. Riemann, one must presume, had only considered fairly ordinary boundary curves. As investigations into the Riemann mapping theorem deepened, so too did mathematicians' appreciation of topological ideas.

The uniformisation theorem says that if $f(z, w) = 0$ is a many-valued function of z , then there is a uniformising parameterisation $z = z(t)$, $w = w(t)$ such that

²⁵The first had already appeared in [Picard \(1883\)](#). He returned again to this theorem in [Picard \(1912\)](#).

$f(z(t), w(t)) = 0$, and moreover that the domain of the uniformising functions is, simple cases aside, conformally equivalent to a disc. So it is closely related to the Riemann mapping theorem. It was first tackled by Poincaré, in a short but remarkable paper (1883d). In this paper Poincaré outlined the construction of the simply connected surface that can be mapped onto the Riemann surface of the many-valued function that is to be uniformised. This topological exercise (to use more modern language) leads to the problem of mapping this new surface conformally onto a disc, and for this Poincaré made use of the theory of harmonic functions and Green's functions, thus assuming a solution of the Dirichlet problem in a difficult form. His argument seems to have been admired from a distance—no-one reviewed it the *Jahrbuch*, for example—but as we shall see Hilbert made some shrewd criticisms of it in his Paris ICM address on mathematical problems in 1900 when he called for the problem to be tackled anew. Hilbert also held out the hope that his approach to Dirichlet's principle could be extended to more general boundary value problems²⁶ and reminded his audience that the Riemann mapping theorem and the uniformisation problem were intimately connected.

We shall see that both the Riemann mapping theorem and the uniformisation theorem raised issues in the theory of harmonic functions and in the emerging subject of point-set topology, and we shall pick these up as we proceed. More precisely, much of the subsequent work on this topic can be divided into those investigations that make no substantial distinction between the methods of complex function theory and those of potential theory, and those that seek an authentically complex function theoretic approach to problems in complex function theory. By 1890 the situation was that rigorous potential theoretic methods had been developed for solving the Dirichlet problem in a range of settings, of which the best was Harnack's account of 1887, as we saw earlier (Sect. 7.7.2). By this time, also, a number of young function theorists had come to feel that a truly function theoretic proof was still required.

As for the latent topological aspects of the Riemann mapping theorem and the uniformisation theorem, a good indication of the growing sophistication of the topological investigations is provided by Hurwitz's address to the International Congress of Mathematicians in Zurich in 1897 on the recent development of the theory of analytic functions. As an eminent function theorist working in Zürich, a former undergraduate student of Weierstrass and a graduate student of Klein's, he was presumed to be even-handed in the treatment of the subject in adjudicating between Riemann's and Weierstrass's approaches, and between French and German styles. And, in the event, he was.

In his address, Hurwitz considered a number of questions in complex function theory from a distinctly abstract point of view. Having reminded his audience that Weierstrass defined the domain of a function as a family of overlapping discs, he asked what are the possible shapes of such a domain D (even in the simple case

²⁶Such as those where a condition on the derivative is specified, or where it is not a potential function that is involved, but, say, the minimal surface equation. See Gray (2000b).

where the function is single-valued). This led him to consider the nature of point sets in the plane or on the sphere. He distinguished three possibilities for a point P with respect to the region: there might be discs containing the point P that lie entirely inside the region D ; there might be discs containing the point P that lie entirely outside the region D ; or every disc containing the point P might contain points in the region D and points not in the region. The point P is, respectively, said to be an interior, exterior, or boundary point of the domain D . Hurwitz now observed that the domain D consists entirely of interior points and that any two points P and Q in D can be joined by a chain of points $\{P_j, j = 1 \dots n\}$ such that $P = P_1, Q = P_n$ and the distance between P_j and P_{j+1} is less than any pre-assigned and arbitrarily small amount. (The second condition is a weaker condition than connected, it is satisfied by the plane of complex numbers with the real axis removed). Such a point set, said Hurwitz, is called a continuum.²⁷

The next question, he then suggested, was whether every continuum is the domain of some analytic function.²⁸ An affirmative answer had been given by Mittag-Leffler, later simplified by Runge (1885a) and Stäckel (1893). What could be said about the set of boundary points? Hurwitz observed that this question had been broached by Weierstrass in 1876, but a fuller answer required Cantor's theory of point sets, as extended by Bendixson and Phragmén. This involved Hurwitz in a quick explanation of ordinal numbers and derived sets, after which he quoted Cantor's result that the singular sets of an analytic function either form a countable set or an uncountable set. Every countable set can be the singular set of an analytic function (as methods first deployed by Mittag-Leffler enabled one to show), but the same was not true of uncountable sets. In that case the set was the union of a finite set and a perfect set (one, Hurwitz explained, that is equal to the set of its limit points). Hurwitz remarked that no theorem analogous to the Picard theorems was known about non-isolated singular points. As for analytic, multi-valued function Hurwitz suggested the best way forward lay with Riemann surfaces and Poincaré's ideas on uniformisation.

When Hurwitz turned to the Cauchy–Riemann approach he began by commenting that the theory had only recently been given anything like the necessary degree of rigour. The crucial theorem in this approach was the Cauchy integral theorem, which he stated in this form: the integral of a holomorphic function defined on some continuum is zero on any closed curve bounding a disc that lies entirely in the continuum. Then, he said, certain questions arise at once: what is a simple closed curve, what is a curve, what is a closed curve, and are all or only some closed curves to be admitted in the statement of Cauchy's theorem? He found these questions all the more urgent now that Peano (1890) and Hilbert (1891) had exhibited space-filling curves, and noted that partial answers had been given by Jordan, who had dealt with for rectifiable curves in his *Cours d'analyse*, and later by Schoenflies.

²⁷This conception of a continuum Hurwitz had learned from Weierstrass in 1878, see Sect. 6.5.3 above.

²⁸In later terminology, this asks about what sets can be a domain of holomorphy.

Another central theorem was of course the Riemann mapping theorem. Hurwitz did not discuss it in any depth, but left it open after observing that the theorem had been proved for a wide class of surfaces by Neumann, Schwarz, Harnack, Poincaré, and others.²⁹

The overlap of methods in the study of both the Riemann mapping theorem and the uniformisation theorem, and the web of personal influences centred on Göttingen, make it difficult to keep their stories apart. However, because the history of the Riemann mapping theorem in the period after 1900 is largely that of an attempt made by several mathematicians to reclaim for pure function theory what had already been shown using potential theory, while the implications of the uniformisation theorem strike deeply into the theorems of Riemann surface theory, it makes sense first to pursue the mapping theorem to its end in 1914. We shall then take the story of the uniformisation theorem from its formulation by Poincaré in 1883 to the first rigorous proofs published by Poincaré and the young, ambitious, Paul Koebe independently in 1907.

8.3.1 *The Riemann Mapping Theorem*

Between 1898 and 1900 the Riemann mapping theorem attracted the attention of Osgood. William Fogg Osgood had been a student of Klein's in Göttingen when a number of distinguished Americans formed a significant part of the student body there.³⁰ He returned to Harvard and became one of the most influential complex analysts of his day, writing a series of short but penetrating papers and being an astute critic of the work of others. He read Harnack's work carefully, and in 1900 he wrote a short paper for the first issue of the *Transactions of the American Mathematical Society* in which he established the existence of a Green's function for any simply connected plane domain, T . By this he meant a single-valued function that vanishes on the boundary and is harmonic in the interior except for one point where it had a logarithmic pole (like $\log(1/r)$ as $r \rightarrow 0$). His proof was succinct. He divided the plane into suitably small squares of width $1/n$, let C_n be the union of such squares lying inside T , and let u_n be a Green's function on C_n . Then he showed by Harnack's theorem (discussed above) that u_n converged to an harmonic function on T . If T was finite, then the sequence u_n was dominated by a Green's function for a region C containing T . If T was infinite, Osgood had an argument modelled on Poincaré's use of the modular function.

Interestingly, Osgood found that the boundary behaviour was even subtler than had been suspected (see [Gray 1994](#)). He was at pains to point out that his proof did not require that the boundary curve be a Jordan curve (and he took care to define

²⁹For a survey of complex function theory as presented at the ICM's up to 1932, see ([Bottazzini and Gray 1996](#)).

³⁰See [Parshall and Rowe \(1994\)](#) for details.

the idea of boundary values carefully). In this respect his work was a significant advance on Harnack's. Indeed, Osgood pointed out that his proof was valid even for the region of the upper half-plane bounded by the real axis to which has been added unit verticals at every point of a perfect nowhere dense set. This is an example of a set with prime ends, and it is original to Osgood. It does not seem to have been discussed previously by those working on Cantor's problem of characterising the continuum; Carathéodory in his (1913b, 24) attributed the term "prime end" and perhaps the concept to [Schafheitlin](#) (1908, 176).

So Osgood established the existence of Green's functions on arbitrary domains, and thereby resolved the Riemann mapping theorem on the interior of any simply connected domain. The next year, 1901, Osgood finished his article for Klein's *Encyclopaedia*. There (1901, 56) he distinguished between two types of simply connected domains. The first type had boundaries that were (simple) Jordan curves (they may be called Jordan domains); the second did not. He observed that if the boundary was a curve with a continuously varying tangent, then it was certain that the conformal map on the interior extended to a continuous map on the boundary. However, he had established the existence of a Green's function for all Jordan domains so he said it was probable that there would still be an affirmative solution to the Riemann mapping problem. For domains of the second type, as he remarked, it did not make sense to ask about behaviour on the boundary.

That Osgood was fully aware of the problems posed by the boundary of even a Jordan domain is shown by one of his most famous discoveries, made in 1902. This is the existence of simple closed Jordan curves of finite area (see [Sagan 1994](#) and [Sagan 2000](#)). Jordan had discussed the result that now carries his name (the Jordan curve theorem) in a note appended to vol. 3 of his *Cours d'analyse* (1882–1887).³¹ There he outlined a proof, which began by passing from the curve to an arbitrarily close polygonal approximation, but this was criticised by Schoenflies for its lack of clarity. As a result, others offered proofs: [Schoenflies](#) (1896) himself for curves satisfying a smoothness requirement, [Veblen](#) (1905), who gave the first generally accepted proof, and Brouwer in 1910.

In 1909 Hilbert returned to the theme of potential theory discussed above in Sect. 7.7.5 and published a paper that, he said (1909, 73), was essentially one he had given as a lecture with Poincaré in the audience when Poincaré had visited Göttingen in April that year, although he thanked Richard Courant for making the proof more precise in his soon-to-appear Göttingen dissertation.³² The method used, he said, was a considerable generalisation of his earlier argument about the Dirichlet problem. Consider, he began (1909, 73–74), solving this boundary value problem; given a simply connected domain Ω in the (x, y) -plane bounded by a simple curve, find a potential function $u(x, y)$ that:

³¹See ([Jordan 1882–1887](#), 3, 587–594). In the 2nd ed. of the *Cours* (1893–1896) this note, essentially unchanged, was moved to vol. 1, pp. 92–100.

³²This refers to [Courant](#) (1912).

1. Is regular everywhere in Ω except at one point, which can be taken as the origin, and which, on a neighbourhood of the origin, can be written as

$$u(x,y) = \frac{x}{x^2+y^2} + R(x,y),$$

where $R(x,y)$ is a regular potential function at the origin.

2. The conjugate potential function $v(x,y)$ has a constant value on the boundary of Ω .

This problem was, he showed, equivalent to that of minimising a modified Dirichlet integral. To set it up he supposed the origin was contained in an open disc K that lay inside a closed square Q that lay inside Ω . The region of Ω outside Q Hilbert called A . He now defined a function $\Phi(x,y)$ as being equal to $\frac{x}{x^2+y^2}$ in K , zero in A , and given by an otherwise arbitrary function in between so that Φ was twice continuously differentiable in Ω except at the origin. From this function, and the functions

$$C(x,y) = \frac{x}{x^2+y^2} - \Phi(x,y),$$

$$\gamma(x,y) = \frac{\partial^2 C}{\partial x^2} + \frac{\partial^2 C}{\partial y^2},$$

Hilbert constructed two functions α and β that were identically zero in A and satisfied the equation

$$\frac{\partial \alpha}{\partial x} + \frac{\partial \beta}{\partial y} = \gamma(x,y)$$

everywhere in Q .

The minimum problem was then to find a function $\varphi(x,y)$ that minimises this modified Dirichlet integral taken over Ω :

$$D^*(\varphi) = \iint \left(\frac{\partial \varphi}{\partial x} - \alpha \right)^2 + \left(\frac{\partial \varphi}{\partial y} - \beta \right)^2 dx dy.$$

His earlier (1900 and 1904) argument, he said, was immediately applicable and yielded the sought-for function—thus making this paper no better than his earlier ones which do not fully meet modern standards—and after giving the derivation Hilbert went on to show that both these problems are equivalent to this problem: Show that the interior of Ω can be mapped conformally onto a plane bounded by slits (straight lines). He was particularly pleased that the method of the proof extended immediately to a domain of arbitrary connectivity and with finitely or infinitely many leaves.

Hilbert gave three applications for his theorem, the first of which was the uniformisation theorem, which he derived in this form (1909, 77): He took for the domain Ω a simply connected domain spread over the (x,y) -plane with arbitrarily

many leaves and arbitrary boundary points, and he deduced that the interior of Ω can be mapped conformally onto a plane without slits or a plane with one slit that may or may not be reducible to a point. Since the plane with one slit of non-zero length³³ can be mapped conformally onto a half plane in a well-known way, it follows, he observed, that Ω can be mapped conformally onto either the whole plane, or the plane with a point removed, or a half plane. This, he said (1909, 78), was a theorem first proved by Poincaré and Koebe and was the basis of their proofs that arbitrary algebraic curves admit uniformising functions whose domain is a disc.

As a result of Hilbert's intervention, the Riemann mapping theorem was taken up by several students at Göttingen: Bieberbach, Courant, and Carathéodory. It was also discussed in Eduard Study's influential book (Study 1913). All of these men have become famous, but at the time they were all working for their doctorates.³⁴ Carathéodory was a former student of Klein's who was encouraged by Klein and Hilbert to work on questions in complex function theory. The result was his series of three papers on the Riemann mapping theorem.

In his paper of 1912 Carathéodory divided the Riemann mapping problem into two parts: the interior problem; and the existence of a continuous extension of that map to the boundaries. In the first of the papers of 1913, Carathéodory gave two reasons why the Riemann mapping theorem had once again become worth studying: there had been an important realisation that the sensible way to specify the sought-for conformal map so that (were it to exist) it would be unique, was to specify both the value it took at an internal point and also the sign of the derivative there. Riemann had spoken about points on the boundary, but it is exactly the behaviour of the map on the boundary that is hard to understand. This realisation led various mathematicians to contemplate a process of pushing out the conformal map from the interior to the boundary (as we saw with Osgood and Poincaré, and as was still more the case with Koebe's and Bieberbach's approach). Carathéodory's second reason was the advent of Lebesgue's theory of integration, and in particular consequences drawn from it by Fatou in a paper of 1906.

In the first of his papers, Carathéodory solved the interior problem, i.e. the Riemann mapping problem for an arbitrary simply connected domain having at least two boundary points. His is the first truly function theoretic proof. He eliminated Osgood's use of Harnack's theorem by an appeal to what he was the first to call the Schwarz lemma. This says that a holomorphic map f of the unit disc to itself that satisfies $f(0) = 0$ must satisfy $|f(z)| \leq |z|$ and $|f'(0)| \leq 1$, and if either of these inequalities is an equality, then $f(z) = az$ for some complex number a of modulus 1. Carathéodory's substantial argument was based on the resolution of the following question: Let (G_n) be a sequence of infinitely many domains in the u -plane that all contain $u = 0$ as an interior point, and all lie in the disc $|u| < M$, and let $f_n(z)$ be a sequence of analytic functions that represent the domains G_n conformally on the interior of the unit disc in such a way that the points $u = 0$ and $z = 0$ always

³³It helps to think of this slit as going to infinity.

³⁴Bieberbach's membership of the Nazi Party later made him notorious.

correspond and $f'(0)$ is always real and positive. What necessary and sufficient conditions must the domains G_n satisfy for the functions $f_n(z)$ to converge with increasing n to a limit function and what then will be the properties of this function? He was proud of the fact that his solution relied entirely on purely function-theoretic methods, rather than those of potential theory.

Carathéodory began by following Poincaré in reducing the proof of the claim that the interior problem has a unique solution satisfying the constraints on $f(0)$ and $f'(0)$ to a simple application of Schwarz's lemma. He then turned to the above question about G_n and f_n . Montel,³⁵ he said, had proved in 1907 that if the sequence of analytic functions converged inside the unit disc, then it converged uniformly on any smaller disc and so converged to an analytic function; and that every subsequence of the sequence (f_n) has a subsequence that converges inside the unit disc. Carathéodory thereupon showed that the limit function was everywhere a conformal and one-to-one map of some domain Γ onto the interior of the unit disc.

What could be said about Γ ? Carathéodory showed that any closed domain H lying inside Γ and containing the point $u = 0$ necessarily lay inside all G_n for some suitably large n . Moreover, if Γ_1 was another domain such that any closed set it contained also necessarily lay inside all G_n for some suitably large n , then Γ_1 lay inside Γ , so Γ was the largest domain with this property. Carathéodory called it the kernel of the sequence of domains (G_n) and observed that its definition was entirely set-theoretical (we should say topological). Plainly, the limit function $f(z)$ of the previous paragraph provides a conformal representation of the kernel onto the interior of the unit disc. To deduce the solution of the Riemann mapping problem by purely function-theoretic means, Carathéodory then showed how any simply connected domain can be realised as a kernel, including planar domains bounded by simple closed Jordan curves, and domains such as one described by Brouwer, in which one originally disc-shaped domain is wrapped infinitely many times around a fixed disc. He also proved the theorem for domains given as branched coverings.

In the first paper of 1913 he proved the conjecture of Osgood's that the conformal map extends to a homeomorphism of the boundaries if and only if the boundary is a simple Jordan curve. In Carathéodory's opinion, when Osgood made his conjecture a proof might have seemed unattainable. That a proof could now be given, he went on, was due to the far-reaching discovery of the Lebesgue integral and the theorems that Lebesgue had been able to prove with its help (Lebesgue 1902). Carathéodory began proving two lemmas. The first relied on this theorem of Fatou's: given a single-valued analytic function f defined and bounded on the interior of the unit disc, there is an everywhere dense set of points p on the boundary of the disc with the property that as z tends to p along a radius, $f(z)$ converges to a definite value (see Fatou 1906).

He deduced that if f is not constant then it takes at least three values on any boundary arc. In his proof (1913a, 308) he assumed first of all that the function is not constant. On the circle of radius ρ the function

³⁵See Montel (1907) and Montel (1910). Montel's work is discussed below, see Sect. 8.6.

$$F(\rho, \theta) = \int_0^\theta f(\rho e^{i\theta}) d\theta = \int_\rho^{\rho e^{i\theta}} \frac{f(z)}{iz} dz$$

is well defined because $\frac{f(z)}{iz}$ is analytic. Moreover, by Schwarz's theorem, this quantity is also bounded by M , say, for $|z| < 1$. Consequently,

1. $|F(\rho + \Delta\rho, \theta) - F(\rho, \theta)| \leq 2M\Delta\rho$
2. $|F(\rho, \theta + \Delta\theta) - F(\rho, \theta)| \leq M|\Delta\theta|$

Consequently, $\lim_{\rho \rightarrow 1} F(\rho, \theta) = F(\theta)$ exists for every θ and the convergence is uniform, so $F(\theta)$ is continuous and the second condition implies that it even satisfies a Lipschitz condition (it has a bounded differential quotient everywhere).

A Poisson integral argument then showed that at every point at which $F(\theta)$ was differentiable, as $z = re^{i\theta_0}$ tended to $e^{i\theta_0}$ along the radius, $f(z)$ tended to $F'(\theta)$. It was a recent theorem of Lebesgue's that a real function with bounded differential quotient is differentiable almost everywhere and (up to a constant) equals the indefinite integral of its derivative. Evidently the function $F(\theta)$ is such a function, so the function $f(z)$ converges almost everywhere to a definite value on the boundary, thus proving the first half of the theorem. To prove the corollary, Carathéodory assumed that along the arc $\theta_1 < \theta < \theta_2$ the function $f(z)$ took only two values, α and β . Then the function $g(z) = i \frac{f(z) - \alpha}{\beta - \alpha}$ takes the values 0 and i only. A Poisson integral argument then showed that the real part of $g(z)$ is analytic along the arc $\theta_1 < \theta < \theta_2$ and so, by the Schwarz reflection principle the functions $g(z)$ and $f(z)$ are analytic, which contradicts the assumption on the number of values of $f(z)$. The converse is elementary.

Carathéodory's second lemma was that if the boundary of the domain contains a free arc, then a conformal map of this domain onto a disc necessarily maps this arc continuously onto a piece of the unit circle and leaves the end-points distinct. (A free arc is one for which a sufficiently small circle around any point of the arc is divided by the arc into two sectors one of which lies wholly in the domain.) He derived this result from the Schwarz reflection principle.

Carathéodory then considered regions bounded by a simple closed Jordan curve. He referred to Brouwer for the proof that such a curve divides the plane into two parts, of which one (called the interior) is finite in extent (see [Brouwer 1910](#)). He thought of the finite part as lying in the plane of the complex variable u , let O be an arbitrary point of the interior, and let f be the unique conformal map of the interior onto the interior of the unit disc, sending the point O to the point $z = 0$ and having a real positive derivative there. He then showed (by a Fatou-type argument) that the conformal map associates with each point A of the Jordan curve a point A_1 on the boundary of the unit disc with the property that to any sequence (u_n) of points tending to A the corresponding sequence of points (z_n) tends to A_1 . It follows that the map of the boundaries is continuous. Next Carathéodory showed that the map on the boundaries is one-to-one. This followed from his observation about free arcs. Thus he established that a conformal map between a type one domain and the disc

extends to an invertible continuous map (and therefore a homeomorphism) of the boundaries.

In his third paper he discussed what can happen when the boundary is not such a curve—this is the theory of prime ends. His paper is often taken to inaugurate the theory of prime ends, although, as we have seen, the origins of such a theory are to be found in the work of Osgood and, as Carathéodory was happy to acknowledge, related ideas were to be found in the book by Study and Blaschke (see [Study 1913](#)). But it is the first systematic coverage of the topic. Having prepared in this way for every topological subtlety, Carathéodory proceeded to give the first thorough investigation of the boundaries of two conformally equivalent domains, one of which, for simplicity, is the unit circle. He was able to characterise those points on the boundary of a simply connected domain with the property that the map of the interiors provided by the Riemann mapping theorem extends to a one-to-one map on these boundary points. This was the final resolution of a problem originally raised by Riemann. In its mixture of ideas drawn from topology and complex function theory it is in many ways closer to the spirit of the original than had been many of the intervening papers.

Carathéodory's success rapidly drew a response from others in the area. Koebe wrote a three-page note (1913) arguing that the matter was very simple and the introduction of Lebesgue's ideas was not only a significant complication but also far from being essential as, he said, Carathéodory had implied. Instead he generalised a theorem of Schwarz which said that an analytic function defined on some domain above a segment of the real axis and with the property that its value tends uniformly to a constant at points on this segment is also analytic on the segment and the function reduces to a constant. Carathéodory himself published a short note (1913) showing that a special case of Fatou's theorem could be proved without Lebesgue's theory, but he generally remained of the opinion that progress required it.

Next, Bieberbach in Berlin wrote a short paper (1913) to show that Carathéodory's reliance on Schwarz's lemma was excessive, and the theory could be simplified and extended by using only Montel's theorem. The next year, reversing his criticisms of only a year before, he showed ([Bieberbach 1914](#)) how the theory could be freed of any reliance on Montel's theorem. Koebe found once again that he could not resist the opportunity provided by Carathéodory's papers to go back to some old ideas of his own and extend them ([Koebe 1912a](#)). He now offered what he called his "squeezing method" (*Schmiegunungsverfahren*) for solving the Riemann mapping theorem by nothing more than the repeated taking of square roots. This was to prove his most acceptable presentation, being in some sense entirely elementary. Carathéodory incorporated these criticisms into his paper for the Schwarz *Festschrift* (1914), which was to remain his final account until the newer methods of Perron were introduced.

The squeezing method was described most accessibly in Bieberbach's monograph on conformal mappings ([Bieberbach 1915](#)). The problem is first reduced to the case of an initial domain G that lies inside the unit disc D and has the origin, $z = 0$, as an interior point. The method then proceeds iteratively, squeezing out the image until it covers the interior of D . Let the point $z = a$ be one of the boundary

points of G nearest to the origin. Consider a piece of a 2-leaved Riemann surface R_1 that has $z = a$ as its sole branch point is spread out over D and whose boundary covers the unit circle twice. The domain G will be thought of as lying entirely in one leaf of this surface. The quadratic map (see [Bieberbach 1915](#), 99)

$$z = \sqrt{\frac{\bar{A}}{A}} w, \frac{A-w}{1-\bar{A}w}$$

maps the domain G onto a new domain G_1 that also lies in D . By the Schwarz lemma applied to the inverse function, every boundary point of G_1 lies further out than the corresponding boundary point of G . The argument is then repeated with G_1 in place of G and so on, and a detailed argument, also using the Schwarz lemma, allowed Bieberbach to show that the limiting map is onto D .

The last thing to check is that the inverse map from D to G is onto. Bieberbach remarked that it would be possible to show this by repeated application of the Schwarz lemma, but that it seemed preferable to use Koebe's distortion theorem (see below, Sect. 8.3.2.1). That theorem establishes upper and lower bounds on the quotient of the derivative of a map at two points. The function is supposed to be one-to-one (also called univalent) on the unit disc, and the distortion theorem says that

$$\frac{1-r}{(1+r)^3} \leq |f'(z)| \leq \frac{1+r}{(1-r)^3} \text{ if } |z| \leq r.$$

Bieberbach, by fixing one point as 0 and letting the other vary, interpreted the distortion theorem as giving upper and lower bounds on the ratio of the lengths of an arc and its image. So if the inverse map from D to G is not onto then some path can be found that shrinks indefinitely as n increases while its image is bounded. This contradicts the consequence of the distortion theorem, and so the Riemann mapping theorem is proved. It should be noted, though, that the precise bounds given above are due to Gronwall in 1916; Bieberbach had to work with much less explicit estimates.

The book Study and Blaschke published on the subject in 1913 is further evidence of the nature of Koebe's impact and the intense discussions that it provoked. They circulated the manuscript to Carathéodory, Schmidt, and Koebe, who approved it, and it contains ideas that these authors were often themselves only in the process of putting into print. The approach taken in the book was essentially Koebe's, and so potential-theoretic but avoiding Harnack's theorem. Domains were defined very carefully as point sets such that every point of the set lay in a disc lying entirely in the domain, and which had only finitely many branch points, all of finite order. Study and Blaschke described some of the profusion of boundary curves that any theory would have to account for: von Koch curves, nowhere differentiable Jordan curves, various types of spirals, and, for example, a rectangle from which the left-hand edge has been removed altogether and two disjoint sets of vertical lines added. One set hangs downwards and is attached at a discrete set of points accumulating at the left-hand end. The segments grow in length from right to left.

The other set is similar but is attached on the bottom edge and points upwards. The result is that the “missing” left-hand edge is in the closure of the set of all these lines. Thus motivated, they discussed Carathéodory’s theory of ends. Finally, towards the end of the book they gave what they described as essentially Osgood’s proof of Koebe’s distortion theorem.³⁶

Independent of these German authors, Osgood returned to the topic in a paper he wrote with E.H. Taylor (Osgood and Taylor 1913). Their proof of the interior Riemann mapping theorem relied on the theory of logarithmic potential functions. They claimed little novelty for it, except that their proofs were rigorous, but they also claimed that they had been led by intuition, “an harmonic function being thought of as the temperature in a flow of heat or the potential in a flow of electricity” (1913, 277). The chief result concerned the nature of the mapping at accessible points on the boundary. They called a point “accessible” if it could be approached by a curve lying entirely in the domain. They showed that if A was an accessible point of the boundary that was approached by a curve C lying in a domain S and this domain was mapped conformally and one-to-one onto a domain S' , then the image of the curve C is a curve such that if a point P travels along the curve C to the point A , then the image of P travels along the image of C to a point on the boundary that may be considered as the image of A . It follows that distinct accessible points are mapped to distinct accessible points and that if the boundaries are simple Jordan curves then the conformal map of the domains extends to a continuous one-to-one map of the boundaries. However, even if there are inaccessible boundary points, the map of the boundary may be one-to-one on the accessible points, as Osgood had noted before (in his *Funktionentheorie*, 2nd ed., 154.)

8.3.2 Uniformisation

As we saw earlier, in Sect. 8.2.4, the origins of the uniformisation theorem lie in the work of Klein and especially Poincaré in 1881–1882 on automorphic functions. In 1883 Poincaré extended it to all many-valued analytic functions, although, as we shall see, the idea of Fuchsian groups and functions was never far from his mind. In his (1883d) Poincaré let y be any analytic function of x which is not single-valued, and claimed that one can always express x and y as single-valued functions of a complex variable z . To prove this, he first constructed, in a very imprecise way, what later mathematicians would call the universal cover, S , of the Riemann surface that corresponds to the many-valued function y . Poincaré considered loops in the x plane based at a fixed point (as it might be, the origin). He took a point O on the Riemann surface of the many-valued function $f(x, y) = 0$ as the base point of a curve on that Riemann surface, so it is one of the points for which $x = 0$, and he considered the curves C on the surface that correspond to loops in the x -plane based

³⁶We discuss this theorem in Sect. 8.3.2.1.

at the origin. These will be curves starting at O . Some of them will finish at O and some will not, depending whether all the values of y return to their original values or not. Among the loops that start and finish at O some can be shrunk to a point, and some cannot. Poincaré distinguished between these sorts of curves. In one class he put those for which at least one of the functions y_i does not return to its initial value when continued analytically along the loop; these are not loops on the surface $f(x, y) = 0$. In a second class he put the loops based at the point O , distinguishing between those where the loop C can be shrunk to the point O , and those where it cannot.

He then constructed what he called (somewhat confusingly) the Riemann surface of x and today would be called the universal cover of the surface $f(x, y) = 0$ as follows. He assigned the initial and the end points of C to different leaves if the contour is of the first sort (an open curve), or if it is a loop that cannot be shrunk to a point. Poincaré did not, however, introduce any equivalence relation between what today would be called homotopically equivalent curves; it is remarkable that no-one seems to have commented publicly on this omission, which makes his paper very hard to follow. Read with hindsight, each distinct non-contractible loop yields a copy of the point O , and the entire surface $f(x, y) = 0$, he asserted without proof, is turned into a series of polygonal regions that fit together to form a surface which topologically, is a disc. Here readers had to supply the argument that each loop in the x -plane that opens out to a curve with distinct endpoints provides you with a copy of all of the surface $f(x, y) = 0$; imagine, for example, that each point of the surface has been joined to O once and for all by a curve. Because these closed loops yield curves with different endpoints as a result of this process, one speaks of opening out the loops on the Riemann surface. Without giving any supporting argument, Poincaré remarked that the surface S was topologically equivalent to a disc.

Poincaré now claimed that the Riemann surface S was simply connected and topologically equivalent to a disc, thus neglecting the other two possibilities that can arise. He supposed it filled out by a nested, non-overlapping set of circles C each containing a particular point O , from which he chose an infinite subset C_n . He was to use these circles to define a sequence of suitable functions u_n , where u_n has specified properties inside and on C_n .

Poincaré then looked for a suitable Green's function. He took the elliptic modular function ϕ which is holomorphic except at 0 , 1 , and ∞ and which maps the plane onto the upper half plane. It helps to recall, as Poincaré did not, explicitly, that the upper half plane is conformally equivalent to a disc.

He then defined a new function ψ by the formula

$$\psi(x) = \frac{\phi\left(\frac{ax+b}{cx+d}\right) - i}{\phi\left(\frac{ax+b}{cx+d}\right) + i}, \quad (8.8)$$

where a, b, c , and d are constants, which is defined and holomorphic except at the points

$$x = -\frac{b}{a}, \quad x = -\frac{d}{c}, \quad \text{and} \quad x = \frac{d-b}{a-c}. \quad (8.9)$$

He then chose b and d so that $\varphi\left(\frac{b}{d}\right) = i$ and $\psi(0) = 0$. Without loss of generality he set $y_m = \psi$; if need be it is enough to add the function ψ to the list of y 's. Then defining the function t to be $\log\left|\frac{1}{\psi}\right|$, Poincaré obtained a function t which was essentially positive, logarithmically infinite at certain points, and was harmonic, i.e. it satisfied $\Delta t = 0$ away from those points.

He then defined the sequence of functions u_n . To do this he considered a family of circles C_n that are concentric circles with centre the point O —the base point of the opened-out loops—and that gradually fill out the disc S . He then appealed to Schwarz's solution of the Dirichlet problem to say there is a function u_n that is harmonic inside a disc bounded by the circle C_n except for a logarithmic singularity at the point O , and which vanishes on the circle C_n itself.³⁷ Using the function t , he showed by a careful argument about uniform convergence that the sequence of u_n s tends to a continuous function u , because the sequence of u_n s is constantly increasing but bounded above by the function t . Indeed, he went on to show, the limit function u is harmonic away from the point O where it is logarithmically infinite; this is because, wherever it is defined, $t > u$.

This argument breaks down at points O_j where the function t ceases to be holomorphic. To deal with this obstacle, Poincaré argued that it is possible to surround the point O_j with a small disc and to construct a harmonic function t_j that agreed with the function t on the boundary of the disc and was holomorphic inside the disc. The function $t_j - u_n$ would then be positive inside the disc including the point O_j and so t_j would be the upper bound for u_n even there.

Poincaré could now conclude that the function u has a harmonic conjugate v making $u + iv$ a holomorphic function (except at the point O). Finally, he showed that the function $z = e^{-(u+iv)}$ is one-to-one everywhere. It followed that the functions y_i are uniformised by z , because the surface S was constructed in such a way that they have a unique value at each point of S .

It is not just the topological considerations that are difficult to understand in this obscurely presented paper. In proving that the function u is harmonic away from the points where it is logarithmically infinite, Poincaré assumed that the Dirichlet problem was always solvable, i.e. that there always exists an harmonic function on a simply connected domain having prescribed continuously varying values on the boundary. Since the boundary in question could be taken to be a circle, his claim was not to be contested, but the circles were only topological circles and so strictly speaking lay outside the range of known solutions of the Dirichlet problem. More troubling were the infinitely many points that arise where the modular function is not defined, and for which the behaviour of the function u has to be sorted out by a different argument. Poincaré wrote two notes at the end of the paper where he argued that these points lay outside the Riemann surface S , but the claim is weak and in his Paris address, Hilbert stressed that it was extremely desirable to check that the uniformising map was indeed surjective.

³⁷This recalls Riemann's proof of the Riemann mapping theorem, Sect. 5.2.4.

Poincaré began his (1907a) with a review of his and other earlier attempts, which had sought to overcome, or to avoid, the problems which had been raised, and mentioned (Osgood 1900; Johansson 1905, 1906a; Brodén 1905). He endorsed Hilbert's criticisms of his earlier attempt and gave two ways of dealing with it. He added that his original method left it uncertain whether the conformal map of S mapped it onto the unit circle or merely onto a part of it.

Then he outlined his new approach. As he had done in the 1880s, he saw the problem as a Dirichlet problem for a Riemann surface with infinitely many leaves. But now he used a new mixture of his mastery of the methods of harmonic functions and of power series techniques (which had been very important in his work on celestial mechanics) to make the concept of a Riemann surface both more precise and more supple. He obtained the domain that covers the Riemann surface of the many-valued function by a consideration of Weierstrass's *Gebilde*, a net of overlapping discs subject to certain equivalence relations (avoiding his earlier discussion of loops on the surface). He extended Weierstrass's notion of an analytic element to include poles and ramification points. Then he constructed a Green's function for this surface, using Harnack's theorem (Harnack 1887) and a simplification of the sweeping out method from his (1890c).

Poincaré's method produced a sequence of functions u_n which are continuous on a region that grows with n but whose derivatives could be discontinuous on the boundary of the region. Harnack's theorem could therefore be used once it was known that there was a point, P , at which the sequence of function values $u_n(P)$ did not increase indefinitely. The sought-for convergence was ensured by introducing a suitable majorising function, which Poincaré found amongst the classes of Fuchsian functions he had studied at the start of his career.³⁸ It followed by repeated use of Harnack's theorem that the series $\sum(u_{n+1} - u_n)$ tended to a harmonic function u on D .³⁹

From u Poincaré produced the function he had called v in his memoir of 1883, and thence the function $z = e^{-(u+iv)}$ that mapped the domain D into the unit disc. Poincaré also showed that the different functions which could be used to this end are all linearly related: if instead of starting with an arbitrarily chosen point O another, O' say, was used, producing a function z' , say, then the functions z and z' are connected by a Möbius transformation. Finally, he gave two proofs that the conformal representation was indeed *onto* the interior of the disc, one following (Osgood 1900), and one of his own; his paper of 1883 had left this point obscure.⁴⁰

³⁸In this way Poincaré took care of the arbitrariness introduced by the sweeping out.

³⁹Poincaré also took care of the way sweeping out had to be used, which complicates the definition of the limiting process.

⁴⁰The memoir also analysed the cases when a given simply connected region is to be represented conformally not on a circle but on the whole plane and showed how these cases could be distinguished. In modern terms, he dealt with the case where the simply connected covering space is not the disc but the plane. This is the famous recognition problem later much discussed by Ahlfors and others.

8.3.2.1 Koebe

Koebe, the other person to prove the uniformisation theorem, was a student of Schwarz's in Berlin. He wrote his doctoral dissertation in 1905 and then embarked on a lengthy series of papers which unashamedly sought, and secured, the attention of the leading German mathematicians. After all, the uniformisation theorem was the 22nd of Hilbert's problems, and they were beginning to attract notice.⁴¹ He published at length in both the Göttingen and the Berlin journals, seeing to it that his papers were presented to the *Göttingen Nachrichten* by Hilbert and Klein, as well as in the *Comptes rendus*. He not only solved the problem of uniformising algebraic and analytic curves, but also he re-proved the Riemann mapping theorem and its generalisation to non-simply connected domains, and even sought to rescue the old continuity method of Klein and Poincaré. All this work led to his invitation to speak to the International Congress of Mathematicians in Rome on the subject, quite an honour for a man of 26.

The main result Koebe established in (Koebe 1907b, 192–193) was stated by him, none too clearly, in this way:

Given an arbitrary, 'monogene', analytic curve $(x_1, x_2, \dots, x_n)$, where $x_1, x_2, \dots, x_n$ denote the coordinates of a variable point on the curve, then only one of these $x_1, x_2, \dots, x_n$ is an independent variable. That is to say, there is a function

$$t = t_1(x_1) = t_2(x_2) = \dots = t_n(x_n) = t(x_1, x_2, \dots, x_n)$$

that has the following property: at all interior points of the analytic domain (*Gebilde*) $(x_1, x_2, \dots, x_n)$, i.e. at all points where the analytic domain has the character of an algebraic domain, the function $t(x_1, x_2, \dots, x_n)$ has the character of a rational function of the variables $(x_1, x_2, \dots, x_n)$. The variables $(x_1, x_2, \dots, x_n)$ are single-valued analytic functions of the variable t . Likewise, every analytic function that at every interior point $(x_1, x_2, \dots, x_n)$ of the domain has the character of a rational function of $x_1, x_2, \dots, x_n$ is a single-valued function of the variable t .

He then went on to specify the image of the function t : it will map the interior of the domain conformally onto a simply connected domain covering the slit t -sphere. Furthermore, this domain can be either the whole sphere, the sphere with a point removed, or a disc. So, put more clearly, the claim is that the interior of any Riemann surface may be mapped one-to-one and conformally onto one of the three regions on the sphere: the entire sphere, the sphere minus a point, or a disc. In a footnote (1907b, 193) he added that the case of the sphere only arises when the genus of the curve is zero, and the entire burden of the paper is distinguishing between the case of the sphere with a point removed and the disc. Poincaré's (1883d), he added, only dealt with the disc case. Once that problem is resolved, the function t is determined up to a linear (i.e. a Möbius) transformation.

Koebe connected his proof with the ideas involved in proving the Picard theorems, such as those developed by Landau, Hurwitz, Schottky, Boutroux, and

⁴¹ See Gray (2000b) for the references cited there.

Carathéodory—a parade of names that, if not merely self-aggrandising, may recall the fact that the paper was close to his doctoral thesis. The key idea, he said, was the theorem that any simply connected domain B spread over the z -plane with finitely or countably infinitely many leaves and branch points can be mapped conformally onto one of the three domains just mentioned. He proved this result by considering a nested series of domains B_n that exhaust B and showing that they can be equipped with a series of Green's functions u_n sharing a common logarithmic singularity at a point O (which is not one of the pre-assigned branch points) and such that each u_n vanishes on the boundary of the corresponding B_n . He then argued that away from the point O the sequence of functions u_n has an upper bound. He noted that this had almost been proved by Poincaré in 1883 by finding a majorising function, but there had been three troubling points, and that a better majorising function had been found by Johansson (1906a).

To resolve the issue over whether the appropriate domain is the sphere with a point removed or a disc, Koebe first proved that each function u_n can be written in a neighbourhood of the logarithmically singular point O in the form

$$u_n = \log \frac{1}{r} + c_n + R_n,$$

where R_n is a function⁴² that vanishes at O , and the constants c_n are increasing functions of n . Two cases then arise: either the sequence of c_n s is bounded above and tends to a limit, or it is unbounded. In the former case Harnack's theorem implies that the sequence of functions u_n tends to a harmonic function u which is harmonic away from O , in which case, denoting a suitable harmonic conjugate of it by v , the function e^{u+iv} provides a conformal map of the domain B onto a disc. In the latter case Koebe showed that it is necessary to modify the functions u_n and show that the modified functions converge uniformly outside an arbitrarily small neighbourhood of the point O . The limit function in this case can be made, as in the former case, to yield a conformal map of the domain B , in this case onto a punctured sphere.

The paper concludes with the interesting comment that

The idea of an analytic domain (*analytische Gebilde*) with an independent variable is connected by Weierstrass with the representation of infinitely many uniformly convergent series with rational terms The fundamental problem is to find a selection of these uniformly convergent series of rational functions which represents the whole domain. Here it is shown that this can always be done. In this way a problem which one might say belongs to the Weierstrassian mode of analysis is solved by principles which belong to the Riemannian circle of ideas (Koebe 1907b, 210).

Koebe's papers are notable for a string of comparisons with the work of other mathematicians, whom he clearly saw as rivals in a competition to establish the uniformisation theorem. When he compared his approach with that of Poincaré

⁴²Presumably analytic, but Koebe did not say.

(1907a), which he did in his next paper, he noted that he avoided the use of modular functions entirely, unlike Poincaré and others such as Osgood, which he felt led to a notable simplification of the argument (Koebe 1907c, 644). He had to admit that, like Poincaré, he had relied on Schwarz's methods, and had made the modest use of Harnack's theorem, upon which Poincaré had relied heavily, but this inspired him to give a new proof of his theorems the next year, completely avoiding Harnack's theorem (Koebe 1908). When Hilbert revived Dirichlet's principle by establishing a less than rigorous, and somewhat different, minimising principle, Koebe responded with an explanation of how these ideas lay close to his own (Koebe 1910c). When others gave simpler proofs using techniques drawn only from complex function theory, Koebe showed that he too could operate in that way.

Faced with so much to read, few disputed the rigour of Koebe's methods. His work caused such a stir that a special meeting of the *Deutsche Mathematiker-Vereinigung* was held (in Karlsruhe, 27 September 1911) to discuss recent work on automorphic functions, and a report was published the next year (*Jahresbericht* 21, 153–166). Bieberbach reported briefly on single-valued automorphic functions, and Hilb on many-valued ones. Klein (in his summing-up) urged the view that “one must learn to calculate with single-valued automorphic functions as easily as one can calculate with elliptic functions” (1912, 747), and referred in this spirit to his old paper on *Primformen* (1890a). Drama came with the clash between Brouwer, then at his most profound as a topologist, and Koebe. Brouwer (1912b) described his proof of the invariance of dimension under a one-to-one, continuous map, with reference to the Fuchsian case. Koebe (1912b) replied that he had been able to extend this to other cases, and gave a Schottky-type example, where, he said, Poincaré's methods could not work, and he then gave his own report on the uniformisation theorem for both analytic and algebraic curves. A fierce disagreement ensued between Koebe and Brouwer about the extent to which Koebe's work on the continuity method was valid.⁴³ Koebe thought it was completely valid, Brouwer that it was restricted to just the cases covered less precisely by Poincaré. A subsequent exchange of letters between these two forceful personalities revealed that Koebe repeated his claim but could not come up with a detailed proof, and it was a long time before he was willing to accept the justice of Brouwer's position. The debate raised doubts in some people's minds, and it was not until 1923 that Klein acknowledged the essential contribution that Brouwer made to the vindication of the continuity method he and Poincaré had introduced some 40 years before.

The chief response of several authors despite, or perhaps because of Koebe's work, was the desire to give short direct proofs of what lay buried under the torrent of his lengthy papers. In this spirit Osgood summarised just one of Koebe's papers; “a task”, he dryly noted, “of some labour” in his (1913, 144) where he observed that he had given a simpler treatment in second edition of his *Lehrbuch der Funktionentheorie*, vol 1).⁴⁴ Where Koebe had proved the uniform convergence of a

⁴³See (Klein *Ges. Math. Abh.* 3, 731–741) and (van Dalen 1999–2005, 180–193).

⁴⁴Fubini (1908) is likewise critical.

sequence of functions to the required uniformising function directly, Osgood simply invoked Montel's theorem (from [Montel \(1907\)](#)). This theorem establishes that the limiting function is analytic, uniquely defined at each point of the limit domain, and maps it onto a single-leaved region of the plane with a discrete boundary. Courant in his (1912) eliminated potential theory in favour of the theory of conformal mappings. Weyl's influential *Die Idee der Riemannschen Fläche* (1913) contains his version of Koebe's work, and this was taken up by Bieberbach, who drew on his study of Koebe's distortion theorem to write his (1918). There he gave a proof of the uniformisation theorem, using Weyl's idea of triangulating the Riemann surface, which he claimed placed it firmly in the spirit of Weierstrassian function theory and avoided not only potential theory but also considerations of topology. The modern approach is due to Teichmüller in the late 1930s and was remarkably reformulated by Thurston in the late 1970s.

Mention should also be made of another chain of ideas that started with Koebe's distortion theorem of 1909. Koebe showed in (1909b) that an analytic function that maps the unit disc one-to-one onto a domain and is normalised as above always covers a disc of a certain radius k , whatever function is considered (see [Koebe 1909c](#), 73 and [Koebe 1910a](#), 50). The upper bound for k is $1/4$, as Bieberbach showed ([Bieberbach 1916](#)), and he also observed that is attained by the function $f(z) = \frac{z}{(1+z)^2}$. Bieberbach then conjectured that if $f(z) = a_1z + \sum_{n \geq 2} a_n z^n$ is holomorphic and univalent in the unit disc, then $|a_n| \leq n$. It is true because of Schwarz's theorem for $n = 1$ and Bieberbach proved it for $n = 2$. Despite the attention of many eminent mathematicians, progress on the so-called Bieberbach conjecture was patchy until it was proved by de Branges in 1984.⁴⁵

8.3.2.2 Fricke and Klein: Automorphic Functions

Volume Two of Fricke and Klein's *Vorlesungen über die Theorie der automorphen Functionen* was published in 1912, 15 years after the first volume, and it represents their response to the work of Poincaré and Koebe. In the preface, Fricke explained that the original plans for a second volume had ground to a halt after 1901 because of the pressure of other work. However, it had now become possible to make a virtue out of necessity and profit from the work of Koebe on the fundamental existence theorem in the subject, which the second volume had always been intended to address. The lively interest that Koebe's work had engendered was, wrote Fricke, the stimulus that brought him back to finish the work. Fricke then briefly reviewed the story so far, from the first work of Klein and Poincaré in the early 1880s to Poincaré's proof of the uniformisation theorem (here called the *Grenzkreistheorem*) to Koebe's many papers and the contributions of Brouwer. He then thanked everyone

⁴⁵For an account of de Branges' work and the eventual resolution of Bieberbach's conjecture, see the eye-witness account ([Fomenko and Kuz'mina 1986](#)).

who had helped create the mathematics: Klein, of course, Poincaré, Brouwer, and above all Koebe for many letters and personal communications.

The uniformisation theorem was proved in Sect. 2, Chap. 2. Fricke explained that there were two direct approaches to the problem. In one, the first problem was to map a Riemann surface of genus $p > 1$ conformally onto a surface of constant negative curvature. When the curvature is equal to -4 , which involves no loss of generality, this reduced to solving the equation

$$\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} = 8e^u,$$

as Picard had shown in [Picard \(1890\)](#) and subsequent works, and as Poincaré had explored and generalised in [Poincaré \(1898b\)](#). The insight here is that solutions to this equation provide—for reasons there is not space to explain here—solutions to the ordinary differential equation

$$\frac{d^2 w}{dx^2} + \varphi(x, y)w = 0,$$

where $\varphi(x, y)$ is a rational function and x and y are related by an algebraic equation $f(x, y) = 0$. The solutions to this differential equation, which is defined on the Riemann surface corresponding to the equation $f(x, y) = 0$, have the property that x can be represented by non-trivial quotients of them. The advance Poincaré made over Picard was in coping with a greater variety of singular points in these functions.

Fricke then turned to the second method, which was the approach to the uniformisation theorem taken by Poincaré and Koebe. First some results were obtained about the conformal representation of simply connected domains. A Green's function was constructed on a connected surface B and boundary R with finitely many leaves. The surface is assumed to have only finitely many branch points. This function gives rise, by a convergence argument that Fricke credited to ([Osgood 1907](#), 1, 571), to a logarithmic potential⁴⁶ on the surface B . In the case where the boundary R is in one connected piece that consists of finitely many analytic curves, the level curves of the Green's function fill out the surface B simply and without gaps. Consequently the function gives rise to a conformal map of the surface B onto a slit disc that, moreover, maps the logarithmically singular point of the Green's function to the centre of the disc and is one-to-one and onto on a small neighbourhood of the singular point.

Next, these results were connected to many-valued functions, such as those that define a Riemann surface. A given and suitably general Riemann surface is taken and cut up to form a simply connected surface F_1 with boundary R_1 . If the Riemann surface has genus $p > 1$ and n branch points, then the boundary R_1 will be made up

⁴⁶A function that is harmonic away from finitely many points where it is infinite like $\log r$ and which vanishes on the boundary R of the domain B .

of $2n + 4p$ arcs. Copies of the surface F_1 are then stitched together into a net that forms a surface F_∞ by joining sides that correspond to the same cut on the Riemann surface. This surface, with infinitely many leaves, is to be the covering surface of the given Riemann surface, and it has to be shown that it is conformally equivalent to a disc. Fricke showed that it is simply connected, and then, by considering the Green's function on more and more of the net, he showed, using Koebe's distortion theorem (Koebe 1907a and 1909a) and the earlier results, that the covering surface can be mapped conformally onto a disc. This completed his proof of the uniformisation theorem.

8.3.3 Prime Functions and Prime Forms

The last specific topic in geometric function theory that we shall look at is the theory of what were called prime functions. The purpose of introducing prime functions was first stated explicitly by Weierstrass in his (1876b), who drew on the examples of such functions in the theories of rational and elliptic functions. The simplest and indeed canonical example of a rational function is the first degree polynomial $z - a$, which is best to think of defined on the Riemann sphere. By the fundamental theorem of algebra every rational function is a quotient of products of terms of this form: $\prod_{j=1\dots m, k=1\dots n} \frac{(z-a_j)}{(z-b_k)}$. Elliptic functions, the rational functions on the torus or Riemann surface of genus 1, are likewise obtained as products and quotients of theta functions, in the Jacobian theory, and sigma functions in the Weierstrass form of the theory. However, the corresponding theory for rational functions on a Riemann surface of genus g greater than one was for a long time unclear, not least because the theory of Abelian integrals led directly to the study of complex functions of g variables.

Weierstrass's solution to this problem had been to use functions on the Riemann surface that have several essential singularities. An approach due to Schottky in his (1887) was to prove more down to earth. Since that paper goes over much of the same ground as Schottky's paper of 1877 on the function theory of non-simply connected domains, it is convenient to describe both papers here, although, for reasons of space, it is only possible to describe Schottky's (1877) briefly.⁴⁷

His aim in writing the first of these papers was to describe the system of rational functions on a non-simply connected domain. He began (1877, 301) by quoting a

⁴⁷ After graduating from the University of Berlin, Schottky taught there as Privatdozent until 1882 when he was appointed to a chair at the Eidgenössische Polytechnische Schule in Zürich. Ten years later he moved to the University of Marburg, and after ten more years he returned to his home University in Berlin where he stayed until his retirement in 1922. His work dealt mainly with complex function theory and cannot be better summarised than Freudenthal (1975b, 213) did in his biography in the *DSB*: "His work is difficult to read. Although he was a student of Weierstrass, his approach to function theory was Riemannian in spirit, combined with Weierstrassian rigor".

result from [Schwarz \(1870c\)](#) which says that if $p(z)$ is a rational function defined on a simply connected planar domain A with no poles on the boundary of that domain, then there is an entire rational function $F(z)$ with no poles in A and which has the same imaginary part as $p(z)$ on the boundary of A . Schottky used this result to study functions defined on a multiply connected domain A bounded on the outside by a closed curve L_0 and on the inside by $q - 1$ more closed curves $L_1, L_2, \dots, L_{q-1}$. He defined a function $p(z)$ by the equation

$$p(z) = R(z) + i \sum_{j=1} C_j \log R_j(z) = p_1(z) + ip_2(z),$$

where the R 's are rational functions with no poles on the boundary of A . It follows that the function $p_2(z)$ is finite and continuous on every boundary curve L_j . He then chose n arbitrary constants $c_0, \dots, c_{n-1}$ and defined a function ζ by the rule that on L_j it is equal to $c_j + p_2(z)$, $j = 0, \dots, q - 1$. By Schwarz's theorem, this implies that there is a harmonic function $\phi(z)$ inside A that agrees with ζ on the boundary curves.

The harmonic conjugate of ϕ Schottky called ψ , observing that it was not single-valued but that its differential was. It was therefore possible to define a complex function $f(z) = \phi(z) + i\psi(z)$ on A with a single-valued differential, and on integrating this differential round a fixed but arbitrary system of loops L_j it would have periods $2\omega_j$. The function $F(z)$ defined by $F(z) = p(z) - f(z)$ accordingly behaved inside A like the "logarithmic-rational function" $p(z)$; its imaginary part was a constant on each of the boundary curves of A ; and its periods were all real.

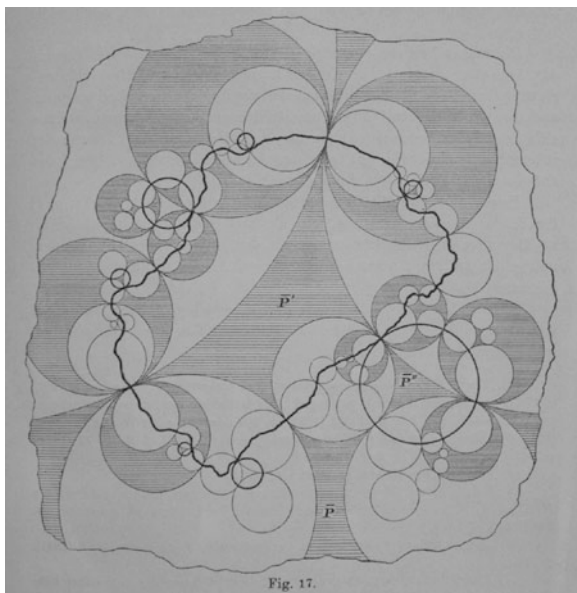
By starting with the special case where the function $p(z)$ has a single pole of some order in A , Schottky showed (in Sect. 4) that there are functions $u(z)$ and $v(z)$ with real coefficients such that every single-valued function in A with real coefficients that behaves like a rational function in A and has purely real values on the boundary of A is likewise expressible as a rational function of u and v .⁴⁸ Moreover, u and v are connected by an algebraic equation with real coefficients (because u and v take real values on the boundary of the domain), and all complex functions F on A are real-integral functions of u and v .

The algebraic equation, $G(u, v) = 0$, Schottky next observed, is of degree $\rho = n - 1$. So ρ is identical to what Riemann had defined as the genus of the curve $G(u, v) = 0$. Schottky showed explicitly that if $\rho = 1$ then the curve $G = 0$ can be put in the form $v^2 = 4u^3 - g_2u - g_3$ for some constants g_2 and g_3 . He then looked at the cases $\rho = 2$, where the corresponding curve $G = 0$ can be written in the form $v^2 = \text{quintic}$; and $\rho = 3$ where he found three possible forms for the equation $G = 0$.

In the next section of his paper Schottky showed how to obtain a symmetrical domain for A and hence to move all these considerations onto a closed surface with no boundary (informally, this is the topological double of A obtained by identifying corresponding points on the boundaries of A and a copy of A). This surface is also

⁴⁸In fact, as he showed, one can take $u(z) = p(z)$.

Fig. 8.6 A boundary curve for a Schottky group (Fricke “Automorphe Funktionen”, *EMW*, IIB4, Figure 17, p. 383)



defined by an algebraic equation and has the property that if this surface has rank or genus ρ there is a $3\rho - 3$ parameter family of such surfaces. If the new surface is simply connected then its rank is zero, and if of genus 1, then elliptic functions enter the story. He also obtained results about the general case, illustrative of Weierstrass's theory of algebraic functions, but we shall not follow him here.

Schottky's work interested Klein, who apparently noticed quite quickly that there was a considerable degree of similarity between what he had done and a particular case of Schottky's work (see [Klein 1921–1923](#), 3, 573) and accordingly wrote to him to ask about how he had come to his ideas. Schottky replied ([Schottky 1882](#)) that had come to them without knowing of Riemann's ([1876b](#)), which was only published a year after he had submitted his Dissertation, where the results first appeared. He had, however, drawn on ideas in potential theory, which he had learned from Helmholtz, as well as on unpublished lectures by F. Neumann. His mathematical task had been made easier, he said, by Weierstrass's lectures on Abelian functions and Schwarz's published study of the representation problem for simply connected surfaces. However, on the advice of his revered teacher Weierstrass he had deleted all this information from his (1877) except for the acknowledgement of Schwarz's work.

In his (1887) Schottky showed that the Schottky function is an automorphic function for a Schottky group (Fig. 8.6). This group is obtained as follows. An arbitrary number, m say, of disjoint circles are drawn in the complex plane each lying in the exterior of every other circle. Let these circles be denoted $C_1, C_2, \dots, C_m$. For convenience and without loss of generality these circles are assumed to lie in the upper half plane, and their reflections in the real axis are denoted $C'_1, C'_2, \dots, C'_m$.

Schottky was interested in the space $\mathbb{D}$ exterior to all the circles, which he called the null surface (*Nullfläche*). Inversion in the circle C_j will be denoted γ_j . The transformations Schottky considered are $\rho\gamma_j$, where ρ is reflection in the real axis. Such a transformation maps the exterior of circle C_j to the interior of circle C'_j . We denote such a transformation γ'_j . The effect of performing first γ'_j and then γ'_k is found by observing that the effect of γ'_k is to map the circle C_j inside the circle C'_k and the interior of the circle C_j to the interior of the image circle, $\gamma'_k(C_j)$. So a point P of the null surface $\mathbb{D}$ is now inside the image of C_j inside C_k . Note that this is not what happens to P after performing first γ'_k and then γ'_j . It is clear, in fact, that there is no combination of transformations that maps P to P other than a trivial chain a transformations and their inverses of the form $t_1 t_2 \dots t_n t_n^{-1} \dots t_1^{-1}$. So the group generated by the transformations γ'_j is the free group on m generators (to import a more modern concept into Schottky's discussion).

The null surface $\mathbb{D}$ is mapped around by the group in a way that almost, but not quite, covers the complex plane.⁴⁹ The original domain $\mathbb{D}$ can be completed by identifying the edges of corresponding circles C_j and C'_j , in which case an m -fold torus (a sphere with m handles) is obtained. Meromorphic functions on the torus with the corresponding complex structure lift to meromorphic functions on the complex plane that are automorphic with respect to the group just constructed. These are all composed out of a simplest one, which is the Schottky function. We now give Schottky's definition.

First, he let x, y, ξ, η be four variables that can take every value but the exceptional ones and considered the product

$$(x, y; \xi, \eta) = \prod_j \frac{(x - \xi_j)(y - \eta_j)}{(x - \eta_j)(y - \xi_j)}, \quad (8.10)$$

where ξ_j and η_j are the images of ξ and η , respectively, under some element of the group, and the product is taken over all elements of the group. This converges when the sum of the radii of the circles bounding the images of the null surface is finite (this is guaranteed by our use of the real axis, but Schottky had to assume it). The appearance of a cross-ratio in Eq. (8.10) already hints at its invariance properties.

Schottky now (p. 242) defined his prime function $E(x, \xi)$ so that

$$(x, y; \xi, \eta) = \frac{E(x, \xi).E(y, \eta)}{E(x, \eta).E(y, \xi)}. \quad (8.11)$$

The function E is defined as follows:

$$E(x, \xi) = (x - \xi) \prod_j \frac{(x - \xi_j)(\xi - x_j)}{(x - x_j)(\xi - \xi_j)}, \quad (8.12)$$

⁴⁹We omit Schottky's account of the exceptional points. They arise from the fixed points of the transformations γ'_j .

where the product is taken this time over “half” the group, omitting the identity transformation and every transformation for which the inverse transformation has already been used. The function is independent of the choices made.

The function $E(x, \xi)$ vanishes to the first order if and only if $x = \xi$ or any value of the form ξ_j , and it is infinite only when $x = \infty$ (but not at any of the transforms of ∞), where the function has a simple pole. The function satisfies $\lim_{x \rightarrow \xi} \frac{E(x, \xi)}{x - \xi} = 1$, and its square is equal to the function $(x - \eta)(\xi - y)(x, y; \xi, \eta)$ in the limit when $x = \eta, y = \xi$, a fact that explains Eq. (8.11) and enabled Schottky to regard his function as some sort of a square root. Baker, in his account of Schottky’s work (Baker 1895, 360, Sect. 233) observed that “Thus the function may be expected to generalise the irreducible factor of the form $x - a$, in the case of rational functions, and the factor $\sigma(u - a)$ in the case of elliptic functions, and to serve as a prime function for the functions of x now under consideration”. Indeed, that was exactly what Schottky showed, but the inevitable technical complexity of his arguments forces us to break off our discussion here.⁵⁰

Schottky’s approach was, as he himself said, modelled on the approach initiated by Poincaré, but the explicit character of his work enabled him to avoid any commitment to the uniformisation theorem. Klein’s response a couple of years later similarly skirted that difficult issue. In his major paper of Klein (1890a) dealt with this problem through his theory of prime forms, which integrate to give the functions on a Riemann surface. In modern terms, and in keeping with his preference for geometry, he used the existence of a basis of p holomorphic 1-forms on a Riemann surface of genus $p > 1$ to create the canonical embedding of the Riemann surface in complex projective space $\mathbb{CP}^{p-1}$. He then created a differential form analogous to Schottky’s prime function.

He began by taking an Abelian integral of the third kind, $\Pi_{\xi, \eta}^{x, y}$, with logarithmic singularities at the points $x = \xi, x = \eta$. The Abelian integral is chosen so that $\Pi_{\xi, \eta}^{x, y} = \Pi_{x, y}^{\xi, \eta}$. He then chose two holomorphic differentials dw_x, dw_y and defined his prime form (*Primform*) as follows:

$$\Omega(x_1, x_2; y_1, y_2) = \lim_{dx=0, dy=0} \sqrt{(dw_x dw_y \exp(-\Pi_{x, y}^{x+dx, y+dy}))}.$$

This vanishes if and only if $x = y$. Conversely, if one has a function such as Ω , said Klein, one can not only define the function Π , one can write down expressions for integral of the first kind on the Riemann surface. So Ω has a good claim to being the 1-form from which all rational functions on the given Riemann surface can be generated.

Klein’s prime form was naturally written by Klein in homogeneous coordinates. That aspect aside, Klein (1890a, 401) remarked that his prime form and Schottky’s prime function were “as good as identical”. However, Klein preferred his form

⁵⁰For an accessible modern account, see Hejhal (1972).

because it had better invariance properties—specifically concerning when it was infinite—that in his view enabled him to obviate the many technicalities in Schottky’s paper. Klein never liked arduous technical work, and by the 1890s was happy to delegate it to his assistants, such as Robert Fricke.

Klein then handed over the topic of prime forms to his gifted assistant Ernst Ritter. Ritter published a number of papers on the subject before dying of typhus at the age of 28 shortly after reaching America in 1895 on his way to take up a position at Cornell University. His papers developed the theory of functions on an arbitrary Riemann surface as far as a generalisation of the Riemann–Roch theorem for the new classes of forms on a Riemann surface, and discussed the way automorphic functions vary as their fundamental domain is continuously altered. It seems nonetheless that this line of research was ultimately unproductive. For example, volume two of Fricke and Klein’s *Automorphen Functionen*, 1912 has only one chapter on this approach and is throughout much more in line with ideas that go directly back to the work of Poincaré. But this may also be because in the intervening years Koebe and Poincaré had given rigorous proofs of the uniformisation theorem, thus reliably re-opening the route to the function theory on a Riemann surface that Klein and Poincaré had contemplated back in the early 1880s.

Osgood’s criticism of Klein’s work in his Madison Colloquium Lectures in 1913 (Osgood 1914, 91) was that in his attempt to find functions with suitable invariant properties, Klein had lost sight of the aim of defining a generalisation of the elliptic theta function. Osgood therefore proposed to adapt Klein’s approach to give a solution to that task, and to do so he turned to the theory of several complex variables to provide the proofs that were lacking in Klein’s theory.⁵¹ Osgood argued that the uniformisation theorem allowed one to turn questions about functions on a Riemann surface (of genus > 1) into questions about automorphic functions on a suitable domain $\mathbb{F}$ in the unit disc $\mathbb{D}$. He modified Klein’s construction of the prime form to give a function of two variables that was defined on $\mathbb{F} \times \mathbb{F}$. He dealt with the delicate existence questions about the function, which like Klein’s was defined as a limit, by explicitly using the theory of several variables at this point, and came up with a function $X(t, \tau)$ defined on $\mathbb{D} \times \mathbb{D}$. This function had many symmetries that Osgood listed, and which allowed him to define the prime function as one (in fact either) of the square roots of $X(t, \tau)$. He then showed in some detail, and more fully in his *Lehrbuch* that this function had all the properties required of a prime function.

8.4 Weyl’s Concept of a Riemann Surface

In the winter semester 1910–11 Hermann Weyl lectured at Göttingen on function theory. This course was followed by another, in the winter semester 1911–12, that became the basis of his highly successful book *Die Idee der Riemannschen*

⁵¹He later developed these ideas at greater length in his book (Osgood 1907 and subsequent editions).

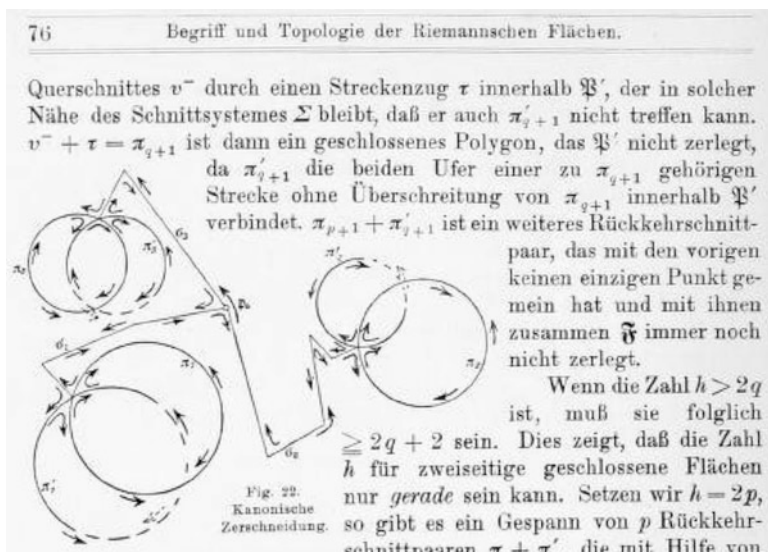


Fig. 8.7 A “canonical dissection” of a Riemann surface (Weyl *Die Idee der Riemannschen Fläche*, p. 76)

Fläche.⁵² The first course has recently been published as (Weyl 2008), and quite apart of its importance in opening the way into the later course it is of independent interest (Fig. 8.7).

It is divided into five chapters: on linear (Möbius) transformations and stereographic projection; on the definition and elementary properties of an analytic function, the Cauchy–Riemann equations and incompressible fluid flow; on the Cauchy integral theorem; on power series expansions of analytic functions, the classification of their singular points, and applications of the Cauchy residue theorem; and finally on many-valued functions, functions on a Riemann surface, and Weierstrass’s analytic continuation. With the exception of the topic of the Dirichlet problem, which Weyl was to deal with in the second course, the presentation is very close to the one Ahlfors was to give in his *Complex analysis* from 1953.

The second chapter is notable for a lengthy account of flows in the plane and the connection with complex functions. This is an old story, going back to the work of d’Alembert but here presented in a way very reminiscent of Klein’s treatment in his booklet on Abelian functions and their integrals (Klein 1882a). It is likely that Weyl was paying his respects to Klein here in acknowledging, as he did on p. 106, that “These kinds of considerations have the advantage of being intuitive”. The passage

⁵²See the essay by Remmert in (Weyl 1997), who rightly notes the remarkable influence of Weyl’s book.

serves the further purpose of giving an intuitive reason for believing the Cauchy integral theorem, which was the subject of the next chapter.

The most interesting thing about the course is Weyl's sensitivity to recent developments in analysis and topology that had made it impossible, as he was among the first to see, to rely upon a naive proof of the Cauchy integral theorem. Instead, his takes 40 pages. He began with a very careful definition of a parameterised path, and he notes that a good definition would exclude the case of a filled-in square being considered a path. But (see p. 143) he was not clear how to exclude the Peano-type space-filling curves from consideration, and to preserve the idea that a curve is one-dimensional he fell back on piecewise differentiable curves. He then explained how to parameterise these curves by arc-length. Then he explained what an integral of a function is along such a curve, C :

$$\int_C (Pdx + Qdy) = \int_{t_0}^{t_1} \left(P(f(t), g(t)) \frac{df(t)}{dt} + Q(f(t), g(t)) \frac{dg(t)}{dt} \right) dt,$$

where the parameterised curve C is given by suitable functions $f(t)$ and $g(t)$ on the interval $t_0 \leq t \leq t_1$.

He obtained what he called a generalised mean-value theorem for such an integral

$$\left| \int_C (Pdx + Qdy) \right| \leq M\ell,$$

where M is the maximum value of $\sqrt{P(x,y)^2 + Q(x,y)^2}$ along C and ℓ is the length of C . He also showed, conversely, how to use this result to define the integral as the limit of a sum over an approximation to C provided by polygonal approximations to the curve. Then he showed how to extend the definition to integrals along curves in the complex domain, and finally to analytic functions.

Weyl could now state the theorem that the integral around a closed path of an analytic function that is the derivative of a regular analytic function is zero. This, he observed, is “almost” the Cauchy integral theorem. However, as he immediately noted, the theorem needed further precision about the domain of the function before it could be proved.

He proceeded to give two proofs. The first proceeded by analogy with the theory of incompressible flows in the plane. The components $u(x,y)$ and $v(x,y)$ of such a flow satisfy the first Cauchy–Riemann equation: $\frac{\partial u}{\partial x} = \frac{\partial v}{\partial y}$, and the analogy shows that under suitable conditions on the boundary

$$\int_C (udy + vdx) = 0.$$

If the function f is analytic, then the second Cauchy–Riemann equation allows one to conclude that

$$\int_C f(z)dz = 0.$$

This led Weyl to give what he called a pure mathematics proof using Gauss's (Green's) theorem for closed curves C that do not self-intersect and that are cut by curves parallel to the x or y axes in only two points. He then extended this argument to curves that are cut by lines parallel to one or another axis in finitely many points, but declined to deal with curves that oscillate infinitely because that difficulty was better dealt with in the next proof. Instead, he looked at curves that cross themselves. Here he noted a topological problem that previous authors (he singled out Osgood) had missed: given two curves joining a point A to a point B there may be no third curve joining A to B that crosses neither of these curves, but again he refused to enter these particular difficulties.

All this led Weyl to the recognition that the Cauchy integral theorem would be true on a suitable domain for a function that was the derivative of an analytic function, and that conversely a function that satisfies the Cauchy integral theorem is the derivative of an analytic function. The proof of the first observation is straightforward, and for the second it is enough to show that the integral

$$F(z) = \int_{z_0}^z f(\zeta) d\zeta$$

defines a function of z that is independent of the path of integration within the domain and that satisfies the Cauchy–Riemann equations. This follows directly from the definitions and the Cauchy–Riemann equations.

The substantial problem Weyl now faced in order to state the theorem properly was to characterise the domain on which the function is defined. It must be simply connected, as the example of the function $f(z) = \frac{1}{z}$ on the punctured disc shows. Now, since Riemann's time a domain had been called simply connected if any curve joining two points on its boundary cuts it into two parts, but it was no longer possible to regard this as a clear definition. It can be proved, said Weyl, by using the Jordan curve theorem, and that in turn can be proved when the curves under discussion are restricted to being polygonal arcs. But the accumulation of these topological subtleties led Weyl to say (p. 161) that his first proof was unsatisfactory, and so he proposed to offer a second proof, which he gave after a short sections on the uses of the Cauchy integral theorem.

For the second proof, Weyl considered domains which can be contained in a rectangle that (without loss of generality) can be assumed to have its sides parallel to the axes. These domains are simply connected in the weaker sense, he said, that any polygonal curve joining two of their boundary points divides the domain into two parts. This device allowed Weyl to avoid the Jordan curve theorem. His proof proceeded by constructing for the anti-derivative F defined above. Weyl subdivided the domain bounded by C into a net of squares, and then proved the theorem first for regions in the domain that are composed of squares, and then by a limiting argument until the path of integration was C itself. The first stage can assume the existence of the anti-derivative on a single interior square and only requires a special consideration for an arbitrary finite number of squares when the path of integration runs along the side of a square. The second stage follows from the observation that

every point in the interior of the domain lies inside some square that lies inside the domain as the sides of the squares are successively halved, and the fact that the functions F defined on one net and the next have the same derivative and so can differ only by a constant, which ensures convergence in the limit.

In the next winter semester, 1911–12, Hermann Weyl lectured at Göttingen on the fundamental ideas of Riemannian function theory. The lectures were soon published and promptly became a classic: the definitive rigorous presentation of these ideas for a generation. In *Die Idee* geometric function theory reaches a climax and is brought into a harmonious union with Weierstrassian methods in one of the few great research books in mathematics. It is very natural, therefore, to conclude our account of this branch of complex analysis with a detailed look at this work.

In this book, which Weyl dedicated to Klein, he set out to meet, as he put it in the preface, the modern demand for rigour by using concepts from topology not to give intuitive plausibility but to provide set-theoretically exact proofs. He had looked, he said, for the simplest and most natural proofs and found that he often departed from Neumann's classic presentation of 1865 and had been greatly influenced by the fundamental work of Brouwer. Accordingly he had been led to think carefully about the proper meaning of such terms as curve and function—although he did feel that some recent work was unhealthy in its pursuit of details and had lost touch with the living flow of science. But, he said, all this logical subtlety, in which beginners could get lost, was clearly not fundamental here, rather it was but a net with which to bring pearls out of the sea (1913, iv).

A Riemann surface was not, for Weyl, just a suggestive idea: it was an indispensable part of the theory—the prius, the mother earth (1913, v), as we said in Sect. 5.2.1. Riemann's original presentation had not been clear, he admitted, and had been much clarified by Klein, so that it was now easy to think of a Riemann surface as a surface in three-dimensional space. But this was no longer adequate. So here Weyl set out to explain precisely:

1. The relationship between an analytic function and an analytic *Gebilde*.
2. What a surface is and what a Riemann surface is.
3. What the theorems in topology are that are necessary for Riemann function theory, in particular for the uniformisation theorem.

That done, he could turn to the more analytic side: the Dirichlet principle after Hilbert's reformulation, the existence theorems of the subject and above all the uniformisation theorem itself, recently proved by Koebe. At this point, he said (1913, vi), "We enter the temple in which the divinity (if I may use this image) returns to itself from the earthly custody of its individual realisations: the original image of the Riemann surface itself will appear (so far as that is possible) pure and freed of all obscurities and accidents in the symbol of the two-dimensional non-Euclidean crystal". For that reason, Weyl thought this result was the appropriate point to end the book.

Weyl's account of the relationship between an analytic function and its analytic *Gebilde* was an improvement on earlier treatments in acknowledging that the function elements must be shown to make up the same function, and that the

irregular ones (those that describe branch points) are only countably infinite at most (which is implied by the Poincaré–Volterra theorem).⁵³ Important though this is more originality attaches to Weyl's account of a surface, and we shall proceed immediately to that topic.

Weyl noted that informally one wants to be able to say of a surface that it is a two-dimensional manifold, and, since that was another ill-defined concept at the time, this means that one wants to be able to say that one has a set, $\mathcal{F}$, of points (the points of the surface) each of which has neighbourhoods, and each neighbourhood is somehow two-dimensional. Weyl took the idea of a set of points as unproblematic and explained the dimensionality condition on a neighbourhood by requiring that it have an invertible map onto the interior of a Euclidean disc. The concept of a neighbourhood itself was trickier. A neighbourhood U_0 of a point p_0 is a subset of $\mathcal{F}$ containing p_0 such that (1913, 18):

1. If p is a point of U_0 and U a neighbourhood of p that lies entirely in U_0 , then the map of U_0 onto the disc maps p to an interior point of the disc.
2. If U_0 is mapped onto the disc K_0 and K is a disc lying entirely inside K_0 , then the pre-image of K in U_0 is a neighbourhood of every preimage of every point of K .

The first of these conditions captures the idea that a surface is locally two-dimensional because it is locally like a disc, and the map from a neighbourhood to its corresponding disc is continuous. The second of these conditions establishes that there are enough neighbourhoods for a surface to have something like the expected topology. But it does not go far enough to show that the surface has a Hausdorff topology: this condition was added to the definition of a manifold by Hausdorff in 1914. As Scholz (1999, 47) has pointed out, following (Remmert, 1998b, 217), Weyl was reluctant to incorporate the Hausdorff condition, only coming to appreciate it in the mid-1920s. He added it without comment to the much revised third edition in 1955, the one that was translated into English in 1964, which may be why some historians and mathematicians credit Weyl with even more than he did in helping to create the modern concept of a topological manifold.

The next few stages of Weyl's argument were then clear. Following the work of Brouwer and the essay by Dehn and Heegard (1907) in the *Encyclopädie der mathematischen Wissenschaften* Weyl argued that every surface can be triangulated, and then presented examples of surfaces.⁵⁴ These included the Möbius band and the projective plane, which are non-orientable (one-sided in the terminology of the day) and are therefore examples of surface that cannot be Riemann surfaces. He then showed how to consider analytic *Gebilde* as surfaces, using triangulations to handle the details. Essentially, the domains of the *Gebilde* are treated as the neighbourhoods of the surface.

To explain how a function on a surface may be said to be analytic, Weyl redefined the concept of a Riemann surface. On his definition (1913, 34), a Riemann surface

⁵³An observation Poincaré had made in his (1907a).

⁵⁴Triangulation of a Riemann surface was only established properly in (Radó 1925).

is a surface for which each point p has a neighbourhood U that is parameterised by a regular analytic function. That is, there is an invertible continuous single-valued map τ say, of the neighbourhood onto a disc in the complex plane, which provides a local uniformising parameter. A function is analytic at p if the map $f\tau^{-1}$ is analytic. In this way Weyl hoped that every local statement about analytic functions in the plane could be carried over to analytic functions on a Riemann surface.

The new presentation in 1955 went into much more detail here, describing the different structures that can be placed on a two-manifold, a topic left undiscussed in the original version. As a result, it is not clear in the original version, despite an extensive discussion, how a surface acquires a complex structure and how it may be said to admit complex functions defined upon it. If it were not too bad a pun, one might say that in 1913 these details were left under the surface. It is clear enough that a complex function gives rise to a surface with the right sort of structure locally, the converse, which would describe the structure abstractly and show that complex functions could then be defined locally, was significantly enriched in 1955 with a discussion of the overlap maps of overlapping neighbourhoods.

Then came a discussion of covering surfaces, simple connectivity, a proof that Riemann surfaces are orientable, and the definition via homology of the genus of a Riemann surface, after which the genus is shown to be the only topological invariant of a Riemann surface. In short, a modern rigorous treatment of the topological aspects of Riemann surface theory; something like a heart–lung transplant for the subject which had been ailing by comparison with the rigours of the Weierstrassian approach and relying on the drugs of naïve intuition for too long. Once again, Weyl updated this material significantly in his 1955 account.

The second part of the book was given over to function theory. The fundamental theorem is the vindication of the Dirichlet principle. Weyl gave his own simplification of the approach used by Hilbert and others (presumably Carathéodory and Courant) to rescue the principle and claimed (1913, v) that the proof was now as transparent as the standard textbook methods of Neumann and Schwarz, if not more so.⁵⁵ Weyl showed that the usual Poisson integral defines a harmonic function on the disc with prescribed continuous values on the boundary and, following suggestions of Hadamard and Zaremba, that this harmonic function is a minimum among functions which are continuous on the disc, continuously differentiable in the interior of the disc, and have a finite Dirichlet integral. The trickiest point is the last one; Weyl's method foreshadows the direct method of Courant.

To obtain harmonic functions on a Riemann surface which are not constant it is necessary to let the functions have singularities (a Riemann surface is compact). This ushered in a discussion of the types of differential on a Riemann surface. Weyl gave an argument using the properties of the Dirichlet integral and a normalisation of the elementary integrals, rather than dissections of the Riemann surface, to obtain the symmetry laws.⁵⁶ All the classic features of the theory then followed in short

⁵⁵He repeated this argument “essentially unchanged” in his (1955, 95).

⁵⁶In his (1955, 131) he wrote that he regarded this “as an essential improvement of the method”.

order: the theory of divisors (in the terminology of [Hensel and Landsberg \(1902\)](#)), the Riemann–Roch theorem, the Brill–Noether theorem, and Abel's theorem.⁵⁷ Weyl also discussed the more advanced theories of forms and differentials developed by Prym and Ritter. He remarked in a footnote that “the principal significance of the inversion theorem today (1913, 128) and (1955, 144) lies primarily, not in its intrinsic value, but in the splendid developments created by Riemann and Weierstrass in their efforts to solve the problem”—a view this book may well be taken to endorse.

The book moves to its end with an account of the uniformisation theorem. In Weyl's view (1913, 141) “In the theory of uniformisation the circles of ideas of Weierstrass and Riemann grow to a complete unity” while above all Klein, Poincaré, and Koebe are to be thanked if today “the theory of uniformisation, which can be said to play a central role in complex function theory, stands before us a mathematical building of exceptional harmony and generosity” (1913, 142).

The proof Weyl offered constructed the uniformising parameter as a function on the covering surface by using the Dirichlet principle to find a Green's function U with a logarithmic singularity at one point O and that in turn allowed him to obtain a differential $d\tau$ on the covering surface. Since this surface is simply connected this differential must be the differential of a function $\tau = U + iV$ that is regular everywhere on the covering surface except at the point O , where it has a simple pole. He attributed the elegance of this argument to Koebe's response to Hilbert's paper of 1909 ([Koebe 1910c](#)). It then remained to show that the function τ mapped the covering surface invertibly and conformally onto either the sphere, the sphere with a point removed, or the sphere with a slit. Weyl concluded that the uniformisation theorem required the solution of two problems: the topological problem of finding the covering surface, and the problem in conformal mappings of mapping the covering surface onto a planar domain. This, he said, agreed with what he called Koebe's general uniformisation principle (in [Koebe \(1910b\)](#)).

Weyl moved to the end of this treatment with a consideration of the role of non-Euclidean geometry and the group of non-Euclidean transformations of the disc, seen as the covering surface for all Riemann surfaces of genus greater than 1. A discussion of the Poincaré theta series and the result that a Riemann surface of genus greater than 1 admits only finitely many conformal automorphisms closed his remarkable book.

This last theorem is today usually associated with the name of Hurwitz, but, as Weyl observed in a footnote ([Weyl 1913](#), 165) Schwarz had proved that a Riemann surface of genus greater than 1 cannot have a continuous family of automorphisms in his (1879), as described in [6.8.1](#) above, after which Noether gave a proof in ([Noether 1882](#)) followed by [Hurwitz \(1883\)](#). Hurwitz himself noted ([Hurwitz 1888](#), 249) that Noether rederived Schwarz's result in his (1882) only to add a note from which the continuity condition could be dropped and the first proof that a Riemann surface of genus greater than 1 has only finitely many conformal automorphisms

⁵⁷The Weierstrass gap theorem was recalled only in the 1955 edition (p. 140).

be derived. He observed that a similar result could be found in (Poincaré 1885b, 19) and then gave his own proof of the theorem. In his (1893, 412) Hurwitz proved that the maximum number of automorphisms a Riemann surface of genus $g > 1$ can have is $84(g - 1)$, and noted that this number is realised by Klein's quartic curve (see Klein 1879). Hurwitz's argument, like Noether's, is based on a study of the finitely many points a Riemann surface of genus g may possess where a function can vanish to order g and vanish nowhere else.

8.5 French Approaches

The appearance of the book *Théorie des fonctions doublement périodiques* by Briot and Bouquet in 1859, which made Cauchy's theory of functions accessible, does not seem to have had the effect its authors might have hoped. Instead, the next 20 years marked a quiet period for complex function theory in France, and there was an upswing only as the 1870s drew to a close. In fact, the 1860s and 1870s were not an especially active time among French mathematicians. Jesper Lützen, in his biography of Liouville, ascribes the last flash of Liouville's genius to the year 1862. The emergence of Camille Jordan and with him of the theory of groups is marked by the publication of his *Traité des substitutions* in 1870, though he remained officially an engineer at the École des Mines until 1885. But Jordan and, of course, Hermite, remarkable mathematicians though they were, are not the same as a thriving school.

The defeat of France by the Prussians in the Franco–Prussian War of 1870–1871 provoked an intense debate in all circles of French life. How could the nation of Napoleon be forced to cede the territories of Alsace and Lorraine only 50 years later? The mathematicians had already begun to think about the state of their profession before the war and had found it wanting. Michel Chasles, in his official report published in 1870, noted that much excellent work was being done, but almost despite, rather than because of the institutional structures of higher education as administered in the Second Empire. His criticisms were echoed by various eminent scientists, Pasteur among them, who found their own fields similarly stunted. One may wonder if such cries would ever have been listened to had not the Prussians got so close to Paris and had not the Second Empire fallen. But with the new regime came a new attention to higher education.

In this period, numerous French intellectual societies were formed, the French mathematical society (*Société mathématique de France*, henceforth SMF) among them. The SMF was created in 1872, with Chasles as its first President. It created a new journal, the *Bulletin de la Société mathématique de France*, with the intention of keeping otherwise isolated researchers informed about current developments. This is close to the aim of Darboux's *Bulletin des sciences mathématiques*, founded in 1869, which, however, was slightly more open to publishing work done by foreign mathematicians. Darboux, writing to his co-editor Jules Houël, was clear that the Germans had already overtaken the French in mathematical terms, and that the Italians were poised to do likewise (see Gispert 1987). He wanted his journal to

compensate for the lack of foreign journals in French libraries, so for many years it carried an extensive section of reviews and abstracts. An early success for which his journal is remembered is bringing non-Euclidean geometry to France, through the translations by Hoüel of papers by Lobachevskii, Bolyai, and Beltrami.

The initiatives involving the SMF and the new journals doubtless had a positive effect on French mathematics, but it was swamped by the effect of doubling the budget on higher education between 1877 and 1883. Salaries of teachers went up, posts were created, and scholarships for students were introduced. Between 1876 and 1896 the number of students increased as a result by a factor of 30, and the number of staff went up by two and a half (Gispert 1991, 54). The new professorships were created exclusively outside Paris, and they boosted the status of the provincial universities and of research within them. At the same time, the *École Polytechnique*, with its orientation towards engineering, found its influence on the SMF declining, while that of the *École Normale Supérieure* increased. The *École Normale* established itself as a good choice, even the first choice, for the able student seeking a career in science or mathematics, and a growing number of students chose it over the formerly more prestigious *École Polytechnique*. Darboux had made such a choice in 1861, and he was followed by Jules Tannery, Goursat, Picard, Koenigs, Appell, Painlevé, Hadamard, Borel, Montel, Élie Cartan, Baire, and Lebesgue. Their influx into the SMF brought with it an emphasis on obtaining a doctorate and a subsequent commitment to research in mathematics.

The doctorate itself was a recent addition to the French system. By 1900 the successful candidate was required to write a thesis that should be original either in content or manner of presentation, and custom required that the supervisor should barely be involved. The American mathematician James Pierpont reported in his (1900) that the quality of the doctorates was considerably higher than that of the German dissertation. On the other hand, one may feel that if almost any reliance on one's supervisor was felt to be inimical to one's chances of a university career, then the system still relied heavily on brilliance and resisted the advantages of forming schools. It should also be noted that a survey of mathematical doctorates in 1893 reported that "since 1850 the twenty-one German universities have accepted 939 dissertations upon mathematical subjects, and that from 1850 to 1890 those accepted at Paris numbered about 144, and from the sixteen *Départements* about 24" (Blake 1894, 126).

Indeed, the generations of French mathematicians that emerged under the new system were not particularly coherent. It is particularly notable that Poincaré, who took his doctorate in 1879 from the *École Polytechnique*, was not the focus of anything like the attention that Weierstrass, Klein, or, later, Hilbert was to attract in Germany. This may have been due to Poincaré's habit of not expressing himself with sufficient rigour or offering adequate explanations. As Hadamard once put it, Poincaré's works "were admired, but no-one 'dared touch them'".⁵⁸ That said, one can easily imagine a mathematician happy to enter a field that Poincaré had

⁵⁸Quoted in (Maz'ya and Shaposhnikova 1998, 39).

discovered and surely left with much to do. The reluctance of the system to bring pupil and teacher together must have also played its part here too.

As we shall see, there were connections, but the threads are short and no more than one might expect in a world where only a few ideas appear propitious at any one time. One began with the work of Poincaré, who may be said to have been the first French mathematician to have a deep appreciation of Riemann's ideas. He made extensive and novel use of the idea of a Riemann surface in his early work, as, to a significant but lesser extent did Picard. But as we have seen, when Poincaré finished the fifth of his lengthy papers on Fuchsian and Kleinian functions in 1884, which created whole new classes of analytic functions generalising the elliptic functions, and then left the field, no French mathematician came forward to enter it. Picard's work extending the idea of Riemann surface to algebraic functions of two independent variables forms a major chapter in algebraic geometry that we cannot discuss here. But his work on the nature of a complex function in the neighbourhood of an essential singular point did not lead directly to work on power series and their singular points, unless one admits inspiration with a time lag of over a decade. It is true, however, that it joined with a third prominent thread begun by Laguerre, who brought Weierstrass's ideas of primary functions to France and began the theory of the genre of a function.⁵⁹ It was speedily taken up by Poincaré, and after him Hadamard and then Borel.

Jacques Hadamard could be described in the words Lebesgue used to describe Jordan, as a conservative innovator who worked more closely than his contemporaries in the traditional French field of applied mathematics. A graduate of the *École Normale Supérieure* in 1888 he eventually became a professor at the *Collège de France* in 1909 and at the *École Polytechnique* in 1912, the same year he was elected to the *Académie des sciences* in Paris. His first major result was his proof of the prime number theorem in 1896; later he worked on the functional calculus and the calculus of variations, and on topics in partial differential equations, most notably the propagation of waves. He had close relationships with Volterra, with whom he shared scientific interests and political views. Hadamard's mathematical seminar opened in 1913 and resumed after the First World War in 1920, when it quickly became one of the principal centres where mathematical research was presented and discussed, and had a considerable influence on the recovery of mathematics in France. The War in many ways marked a break in Hadamard's life because he lost his two sons in 1916 "after which no joy was really pure" as he said once when he was 70.⁶⁰

It is not therefore possible to speak of a specifically French school of function theorists. There was no identifiable movement, group, seminar or school that derived from Hermite, Poincaré, or Picard. Nor, in the period covered by the book at least, was there one led by Hadamard. The first time such a group emerged was

⁵⁹The modern term is *genus*, but we have kept the French to avoid confusion with the concept of the genus of a Riemann surface.

⁶⁰See (Maz'ya and Shaposhnikova 1998, 114) which is the definitive biography of Hadamard.

when Borel set up the *Collection de Monographies sur la Théorie des Fonctions*, which began to appear in 1900 and soon published works by many of the leading French function theorists and a number of their foreign colleagues. It is evident from the mutual citations and references that these people (Adhémar, Baire, Borel, Fréchet, Lebesgue, Montel, Zoratti) actively exchanged ideas. In fact, it seems to have been the case that “after Borel’s own pattern, the tracts were pieced together from published works, usually by pupils of the titular author”, which does at least argue for a collective enterprise (Young 1939, 88).

Émile Borel was the natural focus for this group. A man of abundant energy and purpose, he was for almost all his adult life one of the stars of the École Normale Supérieure, which he entered as a student in 1889 and returned to as a professor in 1896. In addition to his many papers and books he used money from his mathematical prizes to found the journal *La revue du mois* in 1906. This was a progressive intellectual journal that lasted until 1920. For a few happy years after 1910 he was in charge of the science students at the École Normale Supérieure, but the terrible losses during the War ended that period of his life, and he moved to the Sorbonne and turned his interests to mathematical physics, probability theory and game theory. He was elected to the Académie des sciences in Paris in 1921 and became its President in 1938. He was the founding Director of the Institut Henri Poincaré in 1928 and remained active almost until his death at the age of 86. In spite of his tremendous importance in French scientific and intellectual life, there is no definitive biography of him.⁶¹

Darboux commented on this new focus of attention in his report to the International Congress of Mathematicians in Rome in 1908, when he began by saying (Darboux 1909, 105)

A profound change has been brought about before our eyes in the orientation of mathematical studies in the last 30 years. The theory of functions, as one must call it, today attracts the youngest, the most active, and the most inventive among us with an irresistible force. A new spirit animates them: they have already given us some beautiful and profoundly original discoveries, and they promise even more for the future.

The two discoveries that initiated this movement were Weierstrass’s theory of the decomposition of an entire function into its primary factors, and Picard’s two theorems on essential singularities. Picard’s theorems were a revelation, suggesting as they did the possibility of proving remarkable new properties of entire functions. Painlevé hailed them as “Two capes of an unknown continent discovered by some brave navigators, suggesting a mysterious world, a world so vast and so rich that 50 years of exploration would not exhaust its secrets” (quoted in (Julia 1933, 9)).

It is usually an important event in the history of mathematics when a class of objects is isolated and shown to have a rich general theory, and so it is in this case. The eventual effect of Picard’s work was to stimulate much research on entire functions. Laguerre introduced the concept of the genre of an entire function, and

⁶¹ Readers may at least consult (Collingwood 1959; Fréchet 1965; May 1970).

Poincaré showed in an influential paper (1883a) that it illuminated the growth of the maximum modulus function and the asymptotic behaviour of the coefficients of the Taylor series. The young Hadamard then greatly developed these ideas and connected them to the study of the Riemann zeta function, while Borel established the basis of a theory of the zeros of entire functions and gave an elementary proof of Picard's theorems.

We shall limit ourselves here to describing the initial discoveries, leaving a description of Painlevé's continent to the experts (see, for example, Segal (1981)). The elements of complex function theory being established, one sees here the erection of a rich new theory on that general groundwork. But the technical aspects of the work are considerable, and we wish the richness to stand out, rather than the intricacy. The central feature of this work is the question of what the coefficients of a convergent Taylor series tell you about the function it defines. It follows from Cauchy's theorem that the circle of convergence passes through at least one point where the function fails to be analytic. The function defined by the series may or may not be capable of analytic continuation beyond some arcs of the circle, although there must be at least one point through which it cannot be continued.

While it is true that the presence of a pole on the circle of convergence means that the series does not converge there, the converse is false. It can be that the Taylor series converges at every point of its circle of convergence $|z| = 1$, as for example when $|n^{1+\varepsilon}a_n| < A$ for some constant A . An interesting result at the other extreme to Weierstrass's discovery of highly non-analytic behaviour on the boundary was exhibited by Fredholm and publicised by Mittag-Leffler in the form of a letter to Poincaré published in *Acta mathematica* 15 (1891). Here Fredholm exhibited the Taylor series $\sum_{n=0}^{\infty} a^n z^{n^2}$, where $0 < a < 1$ is a real parameter, and argued that it has the unit circle as a natural boundary beyond which it could not be continued, yet the function and all its derivatives exist and are continuous on the boundary. This makes the result one of the first examples of a gap theorem, relating behaviour of the coefficients to behaviour on the circle of convergence, and it was taken up in (Pringsheim 1893). Fredholm's result is correct, but his proof relied on a misinterpretation of Kovalevskaya's theorem on partial differential equations.⁶²

A Taylor series $\sum a_n z^n$ may diverge at every point of its circle of convergence, as for example when a_n does not tend to 0, or converge at some points and diverge at others, as for example $\sum \frac{z^n}{n}$, which diverges at $z = 1$ but converges everywhere else on $|z| = 1$ and an example—too complicated to give here but due to Sierpinski in 1916, see (Dienes 1931, 464)—of a series which converges at $z = 1$ but diverges everywhere else on $|z| = 1$. It emerged from all this work, in particular the paper (Leau 1899), that a regular point on the circle of convergence cannot be isolated and instead that regular points form arcs. The series may diverge on a dense set of points. Lusin in his (1911) gave an example of a series $\sum a_n e^{ni\phi}$ for which $a_n \rightarrow 0$ but the series diverges for every value of ϕ .

⁶²The proof was corrected in (Khavinson and Shapiro 1994).

Such considerations can be applied to entire functions exactly as Cauchy had observed for polynomials: if $f(z)$ is an entire function, then its reciprocal $g(z) = 1/f(z)$ has poles precisely where $f(z)$ has zeros, and so the radius of convergence of the Taylor series for $g(z)$ locates the modulus of the smallest zero of $f(z)$. By Liouville's theorem, an entire function which is not constant must either have a pole at $z = \infty$ or fail to be defined there. If it has a pole the function is a polynomial function, so the interesting case is where the function has an essential singularity at infinity. On the face of it, there should be entire functions which grow at infinity not much faster than a polynomial, but others that grow much faster, and this is the naive insight that the idea of genre was designed to explore.

8.5.1 Genre and Order: Poincaré, Hadamard, and Borel

Laguerre's original motivation (more akin to that of Weierstrass than Picard) was to show that some transcendental functions could be thought of as very like polynomials, so much so that the theorems of Rolle and Descartes about the location of their zeros applied to them.⁶³ To this end he drew attention to primary factors of the form $e^{x/a}(1 - \frac{x}{a})$ which he called of "genre" 1, those of order zero being functions with no exponential factor. He also showed how one might determine the order of a given entire function. But he published only three short notes on the matter in the *Comptes rendus* for 1882 (and a later one in 1884) before leaving the field to Poincaré. They were, however, to be much appreciated by Borel, who savoured Laguerre's habit of giving precise and interesting results without any systematic presentation of the underlying ideas (see [Borel 1900](#)).

Poincaré, however, picked up the baton at once. In ([Poincaré 1882b](#)) he defined an entire function to be of genre n if its primary factors were of the form $e^{P(x)}(1 - \frac{x}{a})$, where $P(x)$ was a polynomial of degree n . He then considered functions of order zero and showed that if F is such a function and α is such that $\exp(\alpha re^{i\theta})$ tends to zero as r increases (θ being fixed), then $\exp(\alpha re^{i\theta})F(re^{i\theta})$ likewise tends to zero. One can paraphrase this as: if F is of genre zero and $e^{\alpha x}$ tends to zero along a ray, then it tends to zero more strongly than F tends to infinity; or, even more shortly, that $e^{\alpha x}$ dominates F . As Poincaré noted with regret, this and some other properties he presented did not characterise functions of genre 0. It was true, however, that if F was of genre n , then $\exp(\alpha x^{n+1})$ dominated F .

More troublingly, as he noted in a longer but inconclusive paper the next year, ([Poincaré 1883a](#)), it seemed very difficult to establish such basic results as:

1. The sum of two functions of genre n is also of genre n .
2. The derivative of a function of genre n is also of genre n .

⁶³Among the functions that behave very like a polynomial is Riemann's ξ function.

Indeed, he said, one could not be sure that the results were true. He was right to register a doubt: in 1902 Boutroux, a nephew of Poincaré, showed that pairs of certain types of function of genre n had a sum of genre $n + 1$. Borel was among those who were surprised, and impressed, by Boutroux's example, which exploited the fact that some functions of genre p grow like functions of genre $p - 1$ and order p , (order is defined below) as the remarks in his *Fonctions méromorphes*, p. 113, attest.

Blocked in this direction Poincaré turned aside, publishing in April on another topic pioneered by Weierstrass, the theory of lacunary spaces, and in May on the uniformisation theorem. Only in the 1890s did the explosion described by Darboux finally encompass the theory of entire functions.

Indeed, in some sense it was Darboux who started it off. He had published a paper in 1878 on the approximation of functions in which he drew attention to the way in which a power series $\sum_{n=0} c_n x^n$ can become infinite on its circle of convergence (Darboux 1878). In particular, he showed that if a function has a unique singular point z_0 on its circle of convergence, then $z_0 = \lim_{n \rightarrow \infty} \frac{c_n}{c_{n+1}}$.

The converse was claimed, incorrectly, by Lecornu in a paper of 1887 that forms the springboard for Hadamard's own much deeper investigations in his doctoral thesis. Lecornu's mistake stimulated Hadamard to choose as the topic of his doctoral thesis of 1892 the properties of a function defined by a Taylor series (Hadamard 1892). He considered power series whose radius of convergence was unity and investigated how the nature of its singularities was related to the order of growth of its coefficients. He found a necessary and sufficient condition for $z = 1$ to be a simple pole of a function $f(z)$ is that $\frac{|f^{(n)}(t)|}{n!} > \left(\frac{1-\varepsilon}{1-t}\right)^n$, where t is a real number between 0 and 1. This implies Pringsheim's theorem, which says that if the radius of convergence is 1 and the coefficients of the Taylor series are non-negative, then the point $z = 1$ is a singular point for the sum of the series. But Hadamard did not notice this, and in later life listed it as one of his failures.⁶⁴

Hadamard was led to define the order of a function on its circle of convergence as

$$1 + \limsup_{m \rightarrow \infty} \left(\frac{\ln |a_m|}{\ln m} \right).$$

Rather than explain how Hadamard was led to this definition, let us point out the results he was able to deduce by its means. He showed (Sect. 48) that if a function defined by a power series has finite order it can be represented by a series of polynomials that converge not only inside the circle of convergence but at every non-singular point on the circle of convergence. Other results in the thesis gave necessary and sufficient conditions on the coefficients for a given point on the circle of convergence to be a singularity.

⁶⁴See (Maz'ya and Shaposhnikova 1998, 306). Others working this area independently were Fabry and Lindelöf.

Chief among the results in the thesis was what has become famous as the Hadamard gap theorem, when he showed that if there is a constant $\lambda > 1$ such that for all k , $\frac{n_{k+1}}{n_k} \geq \lambda$, then the series defines a function that has the circle as a natural boundary.⁶⁵ In 1896 Fabry generalised this to what are called lacunary series, $\sum_{k=1}^{\infty} a_k z^{n_k}$, $a_k \neq 0$, in which the majority of the coefficients are zero (more precisely, $\lim_{k \rightarrow \infty} \left(\frac{k}{n_k}\right) = 0$). We also note that in 1901 Hadamard updated his account of the connection between the limiting properties of the coefficients of a power series and singular points on the circle of convergence, and the implications for analytic continuation in his little book of barely 100 pages, *La série de Taylor et son prolongement analytique*.

In 1893 Hadamard published his famous paper on entire functions for which he was awarded the Grand Prix of the Académie des sciences. He began by recalling the definition of genre. If an entire function $F(x)$ decomposes into primary factors thus:

$$F(x) = e^{G(x)} \prod_{p=1}^{\infty} \left(1 - \frac{x}{\xi_p}\right) e^{Q_p(x)}, \quad (8.13)$$

then F is of genre E if all the polynomials Q_p are of degree E and the entire function $G(x)$ is a polynomial of degree E at most. Then he set himself the task of proving that if the coefficient, a_m , of x^m in the Taylor series for F is less than $\frac{1}{(m!)^{1/\lambda}}$, then the function is in general⁶⁶ of genre less than λ . Hadamard was therefore refining the simple observation (Hadamard 1893, Sect. 6) that since the function F is entire, the root test for convergence establishes that $\lim_{m \rightarrow \infty} |a_m|^{1/m} = 0$. In so doing he was also proposing a partial converse to a result in (Poincaré 1883a, 136). The crowning glory of his paper was the rigorous proof that Riemann's function ξ which arose in his study of the zeta function, was indeed of genre 0 as a function of z^2 . As we explained in Sect. 5.10, Riemann had offered only an insufficient proof of this assertion.

8.5.1.1 Gap Series and Singular Sets

In 1896 the French mathematician E. Fabry took up the pioneering work of Hadamard and investigated the behaviour of a function defined by a power series convergent in the unit disc. Using little more than the root test for convergence, he was able to show that in general such a function has the unit circle as a natural boundary. The root test says that the power series $\sum_k a_k z^k$ converges inside a disc of radius equal to the reciprocal of the upper limit of the sequence $\sqrt[k]{a_k}$. Fabry now took two real numbers λ and t satisfying $0 < \lambda < t < 1$ and sequences of positive integers m, p, v such that $v > \lambda m$ and $\lim p/mt = 1$. He then defined the function

⁶⁵Hadamard's series are lacunary series because $n_k > k - 1$.

⁶⁶Hadamard explained that when λ is not an integer the function is of genre E , where $E + 1$ is the least integer greater than λ , but if λ is the integer $E + 1$, the genre may be either E or $E + 1$.

$$\varphi_m(t) = \sum_{k=-v}^v a_{m+k} t^k \cdot \frac{\Gamma(p+1)\Gamma(m+k+1)}{\Gamma(m+1)\Gamma(p+k+1)}$$

and used it to define a power series $\sum_m \varphi_m(t) z^m$. The root test shows that if the upper limit of the sequence $\sqrt[m]{|\varphi_m(t)|}$ is less than 1 then $z = 1$ cannot be a singular point of the power series, but if the upper limit is 1, then z is a singular point.

Fabry was able to generalise this result and to show that, with certain restrictions, the converse of this result holds. He could then give very general examples of power series that have the entire unit circle as a natural boundary. In particular, power series of the form $\sum_k z^{c_k}$ that have a certain disc (of whatever radius) as their disc of convergence have the corresponding circle a natural boundary whenever $c_{k+1} - c_k$ tends to infinity with k . On the basis of these results he conjectured that most power series have their circle of convergence as a natural boundary. The next year, 1897, he gave criteria for a power series to have an isolated singular point at the point $z = 1$.

Meanwhile, in 1896, Borel had also published a paper (1896b) in which he argued that in general a function with a given circle of convergence has that circle as a natural boundary. He did this by arguing that if the circle is not a natural boundary then the function is summable on some arbitrary domain that goes beyond the circle. He could then deduce, for example, that the power series $\sum a_n z^{c_n}$, in which the c_n are an increasing sequence of positive integers and the a_n are arbitrary has the unit circle as a natural boundary if, for all n greater than some fixed number, $\frac{c_{n+1} - c_n}{\sqrt{c_n}}$ is greater than some fixed number k . More generally, he concluded his paper with a necessary condition for a Taylor series to have only isolated singular points on its circle of convergence, and a necessary condition for a Taylor series to have the circle as its natural boundary. A comparison of these two conditions permitted him to announce that the general rule was that the circle was a natural boundary.

This result was rederived in a simpler way in 1899 by Leopold Leau. He studied Taylor series with the unit circle as their circle of convergence by a simple argument involving forming the power series expansion of the given Taylor series about a point $b \neq 0$. The condition that the new disc of convergence goes beyond the unit disc turned out to reduce to a linear condition on a finite number of the coefficients of the original Taylor series, from which Borel's theorem followed. Bieberbach, in his (1955, 139), suggested that Leau's Ansatz was much more productive for later work than the general theorem of Hadamard upon which it drew. The reader is directed to the final 16 pages of his book where Bieberbach described a number of developments that we cannot enter into here. É. Le Roy also wrote at length on this issue, and on the analytic continuation of functions defined by Taylor series that do not have their circle of convergence as a natural boundary (see [Le Roy 1900](#)).

In his (1898) Borel responded to a paper published by Hadamard at the same time ([Hadamard 1899](#)),⁶⁷ but announced the year before in the *Comptes rendus*

⁶⁷This paper was actually printed on February 24, 1898, which is how Borel was able to refer to it.

(Hadamard 1897), in which Hadamard had developed a way of constructing power series with specific singularities. Hadamard showed that, given two power series $\sum_k a_k z^k$ and $\sum_k b_k z^k$ with finite radii of convergence, a third series defined by $\sum_k a_k b_k z^k$ can be studied, and in particular if the points $\{\alpha_k\}$ and $\{\beta_k\}$ are the singular points of the first two power series, then the singular points of the third series form a (possibly strict) subset of $\{\alpha_j \beta_k\}$. Borel then argued that the nature of a singular point $\alpha\beta$ depends only on the nature of the singular points α and β , and that the point $\alpha\beta$ will be singular unless there are points α' and β' such that $\alpha\beta = \alpha'\beta'$, in which case it can happen that the singular points cancel out.

Borel then investigated the case when the first two function elements define a single-valued function with isolated singularities in more detail and showed that they define a third series (as above) with the same properties. The more general situation had already been studied by Mittag-Leffler, he remarked, but Borel noted that it was possible to find single-valued functions for which Hadamard's construction produced functions that were not single-valued.

Borel's proof was criticised in 1907 in a paper by Faber, a former student of Pringsheim's in Munich. Faber showed that neither Hadamard's nor Borel's proofs allowed one to discuss the behaviour of the Hadamard function in its entire domain of definition, and that the claim that the nature of the singular point $\alpha\beta$ depends only on the nature of the singular points α and β is true only if at most one of α and β was a branch point. Faber's argument was further simplified by Pringsheim at the end of his paper (Pringsheim 1912). The difficulty here is that, as Leau showed, there are functions holomorphic in the star domain $\mathbb{R} - [1, +\infty]$ which are singular only at the two points $z = 1$ and $z = +\infty$, but can be analytically continued across the segment $(1, \infty)$ to a function that has the origin as a singular point. Hille (1962, 2, 43) gives the example of $g(z) = \sum_{k=0} \frac{z^k}{(k+1)^2}$.

Dienes' short book (1913) offers an interesting perspective on these developments as our chosen period comes to a close. He noted that systematic research into how the coefficients of a power series determine the singularities of functions had been opened up by Hadamard in his "already classical" memoir (1892) and that the most important results yet discovered were in Hadamard's (1901). So far, however, he went on, research had concentrated on finding sufficient conditions for a point on the circle of convergence to be singular, or for the circle to be a natural boundary. Moreover, seemingly different methods often led to the same results, because, as Hadamard had pointed out himself, only two cases were well understood: the entirely general power series and power series of very specific types. Hadamard had therefore tried to occupy a position between these extremes, and Dienes now proposed to follow him. He identified the crucial obstacle as the banal fact that the determination of the analytic continuation of a function from a power series representation was "mathematically very complicated and realised only logically"⁶⁸; we must conquer for mathematics this purely logical notion of a

⁶⁸"Logical" was the French term for formal and non-constructive.

function; in other terms it is necessary to represent all the content of this concept by means of the tools we have available in mathematical analysis" (Dienes 1913, vi). This task Dienes identified as the whole purpose of function theory, at least as it was presented by Méray and Weierstrass.

Dienes defined his middle ground as the study of arbitrary coefficients combined with a search for specific types of singular point. In this, he said, he followed the third part of Hadamard's fundamental memoir, which "has remained without influence" until the unexpected work of Borel and Mittag-Leffler on the representation of a function in a neighbourhood of its regular values. It was only by understanding that problem fully, he concluded, that progress could be made on a general theory of singularities.

Dienes went on to give a strikingly self-contained account of the singular points of functions defined by a power series, based on little more than Lebesgue (1906), in which he explained the concept of order, Borel's summability methods and Mittag-Leffler stars, and gave a range of examples of singular behaviour.

8.5.1.2 Order

In 1898 Emile Borel had begun publishing a series of important papers on complex analysis, within which his theory of entire functions and its applications to the location of their zeros and the elementary treatment of Picard's theorems stands out. Among Borel's contributions was the introduction of the concept of order, which as we shall see may be thought of as a sharpening and a simplification of the concept of genre.

Borel's starting points were Laguerre's work and the papers by Poincaré discussed above. He wrote an entire function of genre p in the form

$$e^{Q(z)} \prod \left(1 - \frac{z}{a_n}\right) e^{\left(\frac{z}{a_n} + \frac{z^2}{2a_n^2} + \dots + \frac{z^p}{pa_n^p}\right)},$$

where the series $\sum \frac{1}{|a_n|^{p+1}}$ is convergent. He observed that Poincaré had proved that if $f(z)$ is a function of genre p , then on the circle $|z| = r$, $\lim_{r \rightarrow \infty} f(z) \exp(-\alpha r^{p+1}) = 0$, for every positive number α . So denoting by $M(r)$ the maximum modulus of the function $f(z)$ on (and therefore inside) the circle $|z| = r$, he deduced that for r large enough $M(r) < \exp(\alpha r^{p+1})$ (Borel 1900, 53).

He then sought to investigate the sharper inequality $M(r) < \exp(\varepsilon r^p)$ where p was a quantity lying between p and $p+1$ and defined by the rule that for every $\varepsilon > 0$ the series $\sum \frac{1}{|a_n|^{p-\varepsilon}}$ diverges and the series $\sum \frac{1}{|a_n|^{p+\varepsilon}}$ converges. The number p he called the real order of the entire function (as opposed to its apparent order, a concept he also defined) (see Borel 1897 and Borel 1900, 26). The real and apparent orders turn out to be equal when the apparent order is not an integer; when the apparent order is an integer it is an upper bound for the real order.

Borel elucidated the concepts of order and genre by these examples of functions of genre 1 and various real orders:

1. e^z , of order 0,
2. $e^z \prod_1^\infty \left(1 - \frac{z}{n^2}\right)$, of order $1/2$,
3. $\prod_1^\infty \left(1 - \frac{z}{n}\right) e^{z/n}$, of order 1, and
4. $\prod_1^\infty \left(1 - \frac{z}{n^{2/3}}\right) e^{z/n^{2/3}}$, of order $3/2$.

He showed by a simple majorising argument that order satisfies this crucial property: the order of the derivative of a function of order h is at most equal to h . Delicate results of Hadamard's about the location of the zeros of entire functions and the properties of the concept of order then enabled Borel to give a proof of Picard's little theorem. These results dealt with the question: if the maximum modulus of an function is bounded above by a known function of r , say $e^{V(r)}$ for some given function $V(r)$, how many zeros of the entire function lie in the disc of radius r ? They had the pleasing consequence that the product formula for $\sin z$ could be obtained without any calculation (see [Borel 1900](#), 82).

Borel now gave a proof of Picard's little theorem, restricting his attention to functions of finite genre for simplicity, which he expressed in the form: an entire function which omits two distinct values is a constant. His proof showed by induction that if Picard's little theorem was true for functions whose maximum modulus was less than $\exp r^m$, then it was true for functions whose maximum modulus was less than $\exp(\exp(r^m))$. Nothing is lost, he observed, by restricting m to equal 1, because one can always take another exponential, but even the class of functions whose maximum modulus was bounded by a tower of exponentials of arbitrary height did not exhaust the class of entire functions. He gave this example: $F(z) = \sum_m \frac{\phi_m(z)}{\phi_m(m)}$, where $\phi_m(z) = \exp(\phi_{m-1}(z))$ and $\phi_1(z) = e^z$. His proof for the case of functions of infinite genre was significantly more delicate and did not suggest that the big theorem could be proved in the same manner.

In 1903 Borel extended his work on entire functions to cope with meromorphic functions, taking the opportunity to incorporate the simplifications provided by Lindelöf. This theory was eventually to be eclipsed by Rolf Nevanlinna's work in the 1920s, but it is intriguing in its own right.⁶⁹ Borel's generalisation of Picard's little theorem to meromorphic functions says that if there are three distinct values of a meromorphic function which are taken only finitely often, then the function is a quotient of polynomials. But if values are taken infinitely often, it can still be possible to distinguish a set of exceptional values that are taken less often (that is, to a set of smaller cardinality). However, for an entire function, there is no such set if the order of the function is not an integer, and if the order is an integer the exceptional values are picked out by a single equation.

⁶⁹On Nevanlinna's life, and for an introduction to his work, see the biography ([Lehto 2008](#)).

But if Borel had found an elementary proof of the little theorem, one for the big theorem was still lacking. As Landau wrote in 1906 “All attempts to find an elementary proof of the general theorem had been foiled” (Landau 1906, 268).

8.5.2 Schottky’s Theorem

There was a second route through some of this territory, which may be called the German path, and upon which the name of the Schottky theorem might be posted as a sign. Rich, if partial summaries of this material have been provided by some of the participants (Landau 1906; Lindelöf 1912; Lévy 1912). We have been greatly helped by an unpublished account (Young 1939).⁷⁰

Schottky’s theorem concerns functions which are analytic inside the unit disc and omit two values, without loss of generality 0 and 1. It asserts that, if $f(z)$ is such a function then $|\log f(z)|$ is bounded in the disc $|z| \leq r < 1$ by an explicitly given quantity that depends only the value $f(0)$ and r . It is remarkable that this bound is otherwise independent of the function considered. In view of the importance of such functions in this section, we shall temporarily call such functions “well-behaved”. To keep this account short and simple, we shall concentrate on the qualitative side of the story and omit a discussion of refinements of the quantitative estimate.

The result was first given by Schottky in his (1904), in the form of a response to a paper by his colleague at Berlin, Edmund Landau. Landau had been thinking through Borel’s proof of Picard’s little theorem and discovered a result about the radius of convergence of a power series of a well-behaved function. This result followed from Landau’s observation that a crucial constant in Borel’s proof depends only on the first two terms of the power series. The paper was presented by Schwarz to the Berlin Akademie in July 1904 and was immediately taken up by Hurwitz, who praised the result as a “very remarkable extension” of Picard’s little theorem. Schottky’s result was then presented, by Schottky himself, to the Berlin Akademie in October 1904. The paper in fact provides the first elementary proof of the big Picard theorem. Its main result, today called Schottky’s theorem, is this bound on the modulus of well-behaved functions taking the value $f(0) = p_0$ at $z = 0$: $\frac{2^{24}}{\sqrt{n}} \cdot \frac{1}{(1-r)^4}$, where n is the least value of $|\log p_0|, |\log(1 - p_0)|, \left| \log \left(\frac{p_0 - 1}{p_0} \right) \right|$. He also obtained a bound for the argument of $f(z)$. Schottky’s proof made essential use of the Hadamard–Borel inequalities for the maximum modulus. As Schottky remarked, his result immediately implies Landau’s, but we shall not go into these details. As Young showed, elementary arguments here and later proved to be closely related to, and on occasion dependent upon, results drawn from elliptic function theory. Not only was boundedness established by reference to the boundedness of certain

⁷⁰We are grateful for Ivor Grattan-Guinness for drawing our attention to this manuscript, and to the Archivist of the University of Liverpool for supplying us with a copy.

modular functions on certain domains, but the presence of the circular-arc triangle which is a domain for Klein's J -function shows up (as here) in the permutation of the cross-ratios of the quantities $0, 1, \infty, p_0$ ($0, 1$, and ∞ are precisely the values not taken by the function under consideration in the disc of radius r) and in the factor 2^{24} .

In his paper, Schottky went on to prove the big Picard theorem in a form equivalent to this: a meromorphic function defined on the unit circle with the origin removed, and omitting three values is meromorphic at the origin. This is a reasonably straightforward deduction from Schottky's theorem. The disc is slit along a radius, a branch of the given function is defined in the slit disc, and the bound is used to show that the function has at worst a finite pole there and so the function is indeed meromorphic. One might therefore suppose that great acclaim would have greeted the paper, but it was not to be. In a twist of fate, Schottky's theorem was rediscovered by Pierre Boutroux who, ignorant of Schottky's work, gave the discovery a fallacious proof (he was corrected by Landau).⁷¹ However, it was from Boutroux that Carathéodory learned of Schottky's theorem. Carathéodory took the opportunity to introduce what we call Schwarz's lemma in its full generality (special cases are due to Schwarz and, later, Poincaré; the name is due to Carathéodory). Henceforth, direct proofs of Schottky's lemma were all based on Schwarz's lemma, which Carathéodory regarded as equivalent to a more precise form of the Hadamard–Borel inequalities. This had the unintended effect in minimising Schottky's achievement.

In 1906 the German textbook by Vivanti and Gutzmer appeared (see Chap. 10) in which Schottky's paper was dismissed as a mere remodelling of Borel's proof. This provoked Schottky to the observation, mild in the circumstances, that he had had to add a whole new argument to reach a result that it was well known Borel himself had not been able to attain. Young observed (1939, 33) that in this debate Schottky appears as someone who prefers methods to results, whereas Landau prefers results to methods, and she speculated that this may be why Landau's accounts of the history seem "curiously ineffective". She had in mind (Landau 1906) which, under the guise of history, presents a masterly summary of recent developments organised around his own contribution. Interestingly, elliptic modular functions are brought back to provide the crucial estimates needed to derive the inequalities; hardly the way to derive an elementary proof. Schottky, in his (1906) gave an elementary and a transcendental proof of the Picard theorems, and he too based his arguments on the new Schwarz lemma.⁷²

A more geometrical approach was adopted a year later by Lindelöf (1908), where, however, no discussion of Schottky's theorem is to be found. He followed this up the next year with his survey of developments in Picard theory to the Congress of Scandinavian Mathematicians (see his (1912, 112–136)). There he

⁷¹The result was announced in *Comptes rendus* 141 (1905) 305–307. In the paper containing the fallacious proof, *Bull SMF* 34 (1906) 30–39, Boutroux acknowledged Schottky's priority.

⁷²He was to give a streamlined version of the paper in his (1917).

did discuss Schottky's theorem, and he credited Schottky with having been the first to give an elementary proof of Picard's big theorem, but when it came to details of the proof, Lindelöf fell back on his earlier approach, which he linked to ideas of Borel. In this approach, the crucial bounds are obtained as distances on a Riemann surface generated by an elliptic modular function. Young commented that the guiding principle is essentially Schottky's.

In 1910 matters took a new turn with the publication of a joint paper by Harald Bohr and Landau, in which they showed that all well-behaved functions $f(z)$ for which $|f(0)| < a$ have a common finite bound: $|f(z)| < \delta(a, r)$, whenever $|z| < r$. This improves the original formulation of the Schottky bound by showing that it is bounded as a function of $f(0)$ in the neighbourhood of 0 and 1.⁷³ In the next year it was given an unexpectedly direct proof in the Habilitationsschrift of Paul Bernays (later published as [Bernays \(1913\)](#)). Also in 1911 Carathéodory and Landau connected Schottky's theorem to the work of Montel. They showed in their joint paper (1911) that a series of well-behaved functions converging at an infinite set of points in the disc $|z| < r < 1$ converges uniformly on any compact subset of the disc.

The First World War serves generally as a terminus for this book, but mention should be made of three developments that occurred during it. In his (1916) Landau gave a similar account of the history of proofs of the Picard theorems, one that he modified somewhat in the introduction to the second edition (1929), where he gave more credit to Schottky, and which he re-wrote to base the presentation on the work of Bloch. In the same year the Viennese mathematician G. Pick published the short paper ([Pick 1916](#)) in which he introduced ideas of non-Euclidean geometry into Schwarz's lemma. On this interpretation, the lemma establishes that any analytic map of the unit disc into a part of itself shrinks the non-Euclidean distance between any two points. Pick then interpreted the Schottky bound as the maximum of a certain elliptic modular function. His argument was flawed and was later corrected by Ostrowski. And in 1917 the third edition of the second volume of Goursat's *Cours d'analyse* came out, in which, in an appendix, Goursat gave Schottky's elementary proof of 1906. As a result of these papers, the German tradition, as exemplified by Bieberbach in numerous writings, continued to give considerable prominence to Landau's work, but the French and the growing English tradition, as exemplified by [Valiron \(1949\)](#), began to exploit the quantitative side of Schottky's work.

This energetically pursued branch of complex function theory is not one that offers the historian a tidy ending, but one with a rich further development that we cannot pursue here. Sanford Segal has pointed out that the subject branched in two directions after 1924. One was initiated by André Bloch's work. In that year he gave the first truly elementary proof of the theorems of Picard, Landau

⁷³In this paper, Bohr and Landau showed that the zeta function $\zeta(s)$ is unbounded in the region where $\operatorname{Re}(s) > 1$, and applied Schottky's theorem to deduce several delicate properties of the zeta function.

and Schottky, proofs with not even a hidden appeal to elliptic modular functions.⁷⁴ In the same year, the other branch began when Rolf Nevanlinna took the Poisson–Jensen formula for the location of zeros of a function and started the modern theory of meromorphic functions with his work on value distribution theory.

8.5.3 Borel, Mittag-Leffler, and Analytic Continuation

The opening years of the twentieth century saw a long-running dispute between Émile Borel and Mittag-Leffler over the nature of analytic continuation and natural boundaries that is worth looking at briefly for the light it sheds on academic intrigues as well as the mathematics. Borel had graduated in 1894 with a thesis (examined and approved by Appell and Poincaré) in which he proposed a new definition of analytic continuation. The change was to allow analytic continuation along arcs, rather than strips, and on this definition it was possible to say that at least in certain identifiable cases analytic continuation was possible from one domain to another across what had previously been considered a natural boundary. Borel gave examples of functions f and g defined, respectively, inside and outside the unit circle and which could not be analytically continued in the usual sense across the unit circle, but where the circle was crossed by a dense set of rays along each of which both f and g had the same radial limits. So along these rays at least the continuation preserves the continuity of all derivatives. Poincaré praised the thesis, observing that although this was a topic that had occupied analysts for some time, Borel, he said, was the first to give a systematic treatment, and that in doing so he had established some very remarkable properties that by their nature would clarify ideas in one of the most delicate points of the theory of functions.

These ideas of Borel's do not seem to have been widely read, but they started what became a long-running series of disagreements with Mittag-Leffler that tended to surface at the International Congresses of Mathematicians. It began when Mittag-Leffler gave two talks at the ICM in Paris in 1900; one was on the life and work of Weierstrass (1902a), one on a topic in function theory (1902b). In this talk he raised and solved a problem about an analytic expression that was convergent inside a star-shaped domain but also converged outside the star. Was it possible, he asked, to find a new analytic expression that converged precisely inside the star? He answered that under suitable conditions it was. Even the pages of the *Proceedings* bristle with what happened next. Borel, they record, asked for the opportunity to reply. He went on to observe (we paraphrase slightly) that: after the publication of the beautiful discovery of the eminent Swedish geometer he, Borel, had given an extremely simple proof of the claim. Indeed, the proof, contrary to Mittag-Leffler's presentation, was extremely simple, being an exercise in the Cauchy integral theorem and Borel's theory of divergent series, which reduced the problem

⁷⁴For an account, see (Segal 1981, Chap. II) or (Landau 1929).

to the easy application of ideas due to Runge, Hilbert, and Painlevé. Moreover, Mittag-Leffler's theorem was false if analytic continuation in Borel's, rather than Weierstrass's sense, was allowed.

It is hard to know who would have been familiar with Borel's latest results since, plainly, Mittag-Leffler was not. Moreover, it is hard to understand the French selection of speakers and why Borel was not asked to speak at Paris. As Hurwitz (1897, 469) had indicated, Borel had already contributed a most important result in the subject: the first elementary proof of Picard's little theorem, following Hadamard's elementary proof of a special case.

In 1904 the by-then not-so-new French theory of entire functions got an airing at the ICM in Heidelberg. Boutroux (1905) reported on entire functions of finite integral order, and Mittag-Leffler (1905) on his *E*-functions, a class of entire functions that have particularly simple growth properties. In 1908 in Rome, Mittag-Leffler spoke (for the third consecutive time), as did Koebe (1909d), with uncharacteristic brevity, on his recent successful and very thorough proof of the uniformisation theorem. Mittag-Leffler's paper (1909) was the most closely devoted to the foundations of complex function theory. He returned to the idea of star domains that he had introduced a decade earlier and his *E*-functions. He defined an entire function

$$E_{\alpha}(x) = \frac{1}{2\pi i} \int_S \frac{1}{\alpha} \frac{e^{z^{1/\alpha}}}{z-x} dx,$$

where the closed path S is composed of two straight lines out of the origin at angles of $-\alpha(1+\varepsilon)\frac{\pi}{2}$ and $+\alpha(1+\varepsilon)\frac{\pi}{2}$ to the real axis, joined at their far ends by an arc of a circle of radius ρ^{α} , where ρ is arbitrarily large and $0 < \varepsilon < 2$. He found much to praise in Borel's way of opening up the question of whether the zeros of an entire function could be confined within certain angular domains, and by way of answering it gave six theorems to illustrate the importance of this and similar functions *E*-functions in the theory of functions of finite *genre*. For such work, he said, he relied on the Cauchy integral theorem—what he called “the incomparable instrument of higher analysis”. But he then went on to advocate the use of star domains as domains of convergence of series in explicit opposition to Borel's theory of analytic continuation, and to outline an interesting philosophy of what constitutes the best proofs in analysis, and thus to a defence of Weierstrass. We paraphrase slightly⁷⁵:

For some mathematicians it is a matter of indifference how a theorem was proved – provided it is actually proved – but this is not my opinion, still less had it been that of Weierstrass. He felt that it was wrong to use a higher-order theory, like integration, to prove a result that could be obtained directly. The pretended simplification that might seem to arise in that way would be a mirage.

Borel was only given the chance to reply at an ICM when he was invited to address the Cambridge Congress of 1912, and he chose, naturally enough, to

⁷⁵See (Mittag-Leffler 1909, 75).

present his ideas on analytic continuation. In his (1913) he began by reviewing the paradoxical state into which real analysis had been thrown by the insights of Fourier, who had shown that a continuous function given on an interval has an arbitrary continuation elsewhere. Borel contrasted this with the situation in complex function theory as laid down by Cauchy, Riemann, and Weierstrass, and then adduced a new paradox concerning analytic continuation and natural boundaries. He quoted a theorem of Poincaré which showed that it is possible to divide the unit circle into two arcs A_1 and A_2 and find two (single-valued) functions Φ_1 and Φ_2 with these properties: Φ_1 is analytic in $\mathbb{C} \setminus A_1$; Φ_2 is analytic in $\mathbb{C} \setminus A_2$; and the sum $\Phi_1 + \Phi_2$ defines a function F inside D , the unit disc, and a function G outside D . This means that the function F has an entirely arbitrary analytic continuation outside D to the function G .

Borel suggested that the resolution of this paradox was to notice that the function Φ_1 remains single-valued in Poincaré's sense when a function such as $\log \frac{z-a_1}{z-b_1}$ is added to it, a_1 and b_1 being points of A_1 . This is where the paradox was concentrated, but the sense of "single-valued" could only be determined adequately, he suggested, if one redefined analytic continuation so that some of Weierstrass's boundaries could be penetrated. He further noted that in Cauchy–Riemann–Weierstrass theory a function was complex differentiable (or monogenic) in a domain if and only if it was analytic, and that for this reason the word monogenic was disappearing from the literature. A consequence of his theory, Borel observed, was that it becomes possible to define non-analytic monogenic functions.

He gave this example

$$f(z) = \sum_{n=1}^{\infty} \sum_{p=0}^n \sum_{q=0}^n \frac{e^{-e^{n^4}}}{z - \frac{p+qi}{n}}.$$

This sum, he said, converges outside the square with vertices $0, 1, 1+i, i$. Inside, it has infinitely many poles (at the points with coordinates $(\frac{p}{n}, \frac{q}{n})$). But nonetheless the function is absolutely and uniformly convergent on the region that is left when the poles are surrounded by discs of radius $\frac{\varepsilon}{n^4}$, or, as he preferred, $\frac{1}{h}e^{-e^{n^2}}$ where h is any arbitrarily large fixed quantity. Borel let C_h be the region exterior to these circles, and said that the function $f(z)$ is monogenic in C_h . It was evident he said that $h < h'$ implies $C_{h'} \subset C_h$; and the limiting region he called C . It was, he said, a paradigm example of what he proposed to call a Cauchy domain.

Non-analytic monogenic functions arose, Borel went on to show, as the sums of series of analytic functions, and he sought a theory of the analytic continuation of such functions. There is nothing new to say if the series converges. The theory of divergent series is handled, he said, by the beautiful works of Mittag-Leffler (1909), as expounded at the ICM in Rome. However, the analytic continuation of the new monogenic functions cannot be handled in the language of Weierstrass. If one sticks with it, said Borel, Mittag-Leffler's theory will be nothing but a simplified calculus adding nothing new to the Weierstrassian theory. The vaguer theories of Cauchy and Riemann could, however, be extended, and Borel concluded in fine French

fashion (as he had begun, the Congress being in England, after all) by showing how Cauchy's theory of double integrals could be adapted to this purpose.

One wonders how Mittag-Leffler might have replied. He was to have organised the next Congress, which was scheduled for Stockholm, and he would presumably invited speakers from the strong Scandinavian and Finnish school of analysts, but that Congress was abandoned because of the First World War.⁷⁶

8.5.4 Goursat and the Cauchy Integral Theorem

The Cauchy integral theorem has a long history and was given many proofs in the nineteenth century. Almost every modern text asserts that an essential simplification was found by Goursat, who apparently showed that it was unnecessary to assume that the derivative is continuous. However, the actual nature of Goursat's achievement is more complicated, and most credit should be given instead to Pringsheim.⁷⁷

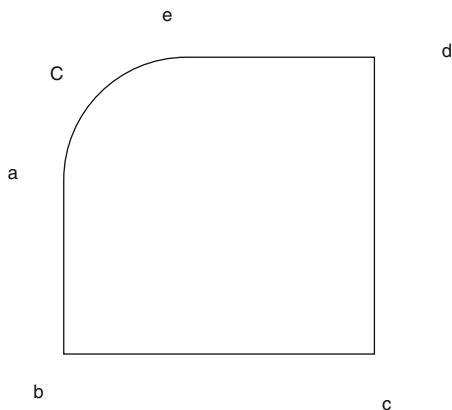
Attempts at a clear and general proof of the Cauchy integral theorem may be said to have started with Briot and Bouquet, who followed their book on the theory of elliptic functions, (1859), with a greatly revised and enlarged second edition in 1875. Here they tackled the Cauchy integral theorem, and their argument was rather ingenious, if at times imprecise. They considered a star-shaped region, R , of the plane, and supposed without loss of generality that any point z in the region can be joined to the origin by a line segment lying entirely inside R . They then considered a closed contour C_1 lying entirely inside the region R , and scale copies of the contour, here denoted C_α , $0 \leq \alpha \leq 1$, where C_α is the set of points z where αz is on the contour. They defined $\psi(\alpha)$ to be the integral $\int_{C_\alpha} f(z)dz$ and first showed that ψ is a continuous function of α . A slightly more complicated argument using the fact that the function f has a derivative independent of direction then allowed them to deduce that in fact the function ψ is differentiable and its derivative is zero. Therefore the function $\psi(\alpha)$ is constant, and its value is arbitrarily small when α is small, so this value must be zero. The Cauchy integral theorem is therefore proved. In view of discussions later in this section, the reader may enjoy discovering where the above proof relies, tacitly, on the continuity of the derivative of the function f .

Goursat gave his first proof in his (1884), where he quite correctly saw his result as illuminating the general definition of a complex function. He claimed that it was enough to use the mere definition of the derivative, and the fact that the theorem is true for $\int_C dz$ and $\int_C z dz$. He argued that a region A bounded by a simple or multiple contour C of finite length can be broken up into squares of side-length λ by two families of parallel lines. Let $C_i = abcd$ be one of these squares of area $a_i = \lambda^2$ and

⁷⁶A minor consequence was that a further Swedish Royal Prize, this one of 3,000 kr for a paper on the theory of analytic functions, was abandoned, see *Acta mathematica* 37 (1914).

⁷⁷Our thanks to Bob Burckel and Alan Beardon for their comments on a previous version of this material, (Gray 2000c). See also Bottazzini's *The Higher Calculus*, 179–180.

Fig. 8.8 A curve in a square
(Drawn by Jeremy Gray)



z_i a point inside it. If the square lies entirely inside C , then from the definition of the derivative it follows that

$$\frac{f(z) - f(z_i)}{z - z_i} = f'(z_i) + \varepsilon_i,$$

so

$$\int_{C_i} f(z) dz = [f(z_i) - z_i f'(z_i)] \int_{C_i} dz + f'(z_i) \int_{C_i} z dz + \int_{C_i} \varepsilon_i (z - z_i) dz.$$

Of these integrals, the first two on the right-hand side vanish trivially, and the remaining one satisfies

$$\left| \int_{C_i} \varepsilon_i (z - z_i) dz \right| < \frac{4\varepsilon_i a_i}{\sqrt{2}}.$$

If the square contains part of the boundary, as in the fashion of Fig. 8.8, then a similar argument shows that

$$\left| \int_{C_i} f(z) dz \right| = \left| \int_{C_i} \varepsilon_i (z - z_i) dz \right| < 4\varepsilon_i a_i \sqrt{2}. 4\varepsilon_i + \varepsilon_i \lambda \sqrt{2} \cdot \text{arc}(ae).$$

Adding these two kinds of contribution up, one finds that

$$\left| \int_{C_i} f(z) dz \right| < \eta \sqrt{2} (4A' + \lambda S),$$

where η is the maximum of the ε_i , A' is the area of the squares inside C and S is the total length of C . But this quantity can be made arbitrarily small, and so the

Cauchy integral theorem is proved—or so Goursat claimed. But there is a gap, which Goursat went on to discuss in this fashion. In truth, he said, the proof supposes that the length λ can always be taken small enough so that all the ε_i can be made less than some arbitrarily small number given in advance. But this is true, because if the derivative is continuous in A and on C , then given any $\sigma > 0$ there is a $\delta > 0$ such that $|h| < \delta$ implies $|\frac{f(z+h)-f(z)}{h} - f'(z)| < \sigma$ for all z inside or on C . This being the case, it is enough to take $\lambda < \delta/\sqrt{2}$, and the theorem is proved “with complete rigour”.

Goursat’s proof and many of its predecessors were speedily criticised by Pringsheim in his (1895a). He objected to the assumption that Cauchy’s theorem was valid for the simple integrals $\int_C dz$ and $\int_C z dz$, because this required a limiting argument no simpler than the general one he went on to present. The problem was that earlier writers, and Goursat in particular, had assumed that the differential quotient $\frac{f(z+h)-f(z)}{h}$ tended uniformly to the derivative, $f'(z)$, for all z in the region T bounded by the path C . But this must be proved, and it turns out to be equivalent to the continuity of the derivative. Goursat’s casual assumption at the end of his paper is equivalent to the uniform differentiability of the function f .

Pringsheim’s argument was very careful, entirely general, and backed up by a wide-ranging historical analysis. He started by defining a path integral in a way that did not require the path to be differentiable, merely continuous. In particular, there was no restriction to rectifiable curves. Instead, he let $P(\xi, \eta)$ be a single-valued function defined on a curve $C = \{(\xi, \eta) : \eta = \phi(\xi)\}$ inside a region T . The curve has merely to be the image of a real interval under a continuous function ϕ . He then defined the path integral as follows:

$$\int_C P(\xi, \phi(\xi)) d\xi = \int_{(x_0, y_0)}^{(x, y)} P(\xi, \eta) d\xi = \int_{x_0}^x P(\xi, \phi(\xi)) d\xi \quad (8.14)$$

He then showed how to establish the Cauchy integral theorem for regions bounded by curves which are step-shaped (made up, piece-wise, of curves that are parallel to the coordinate axes). He then extended the proof to deal with continuous curves, C , that are the limits of step-shaped curves, using an analogue of the mean value theorem.

He also considered when an integral of the form $\int_{(x_0, y_0)}^{(x, y)} P(\xi, \eta) d\xi + Q(\xi, \eta) d\eta$ is independent of the path in a simply or multiply connected region and found that the condition $\frac{\partial P}{\partial \eta} = \frac{\partial Q}{\partial \xi}$ was necessary. In a second paper later the same year, (1895c), he cleared up the relationship between the derivative and the differential quotient, by giving a simple proof that continuity of $f'(z)$ implied that the differential quotient converged uniformly to $f'(z)$. So, he said, Goursat’s achievement was only a simpler proof of a major result, not a weakening of its conditions.

Three years later Pringsheim (1898 and 1899a) returned to the question and sought to investigate what happens when the function $f(z)$ fails to be analytic at a number of points, indeed, even on a 2-dimensional set of points. He found that there were cases when the Cauchy integral theorem still held. He therefore concluded that it might be the case that the mere existence of the derivative, without any assumption of continuity, was enough to establish the Cauchy integral theorem. Therefore, as long as this possibility was not excluded by counter-examples, the question, he said, must remain open.

Pringsheim's papers provoked a flurry of comments in America. Bôcher had already taken up the issue in his (1896). Now Goursat, at Osgood's request, repeated his proof in the first issue of the *Transactions of the American Mathematical Society* (Goursat 1900). In fact, the proof was more sophisticated this time around. He now said that the closed contour C satisfied condition α with respect to a number $\varepsilon > 0$, if there is a fixed z' inside or on C such that

$$|f(z) - f(z') - (z - z')f'(z')| < |z - z'| \varepsilon$$

as z describes the contour C . He then established what came to be called Goursat's lemma: given any $\varepsilon > 0$, any region T bounded by a simple closed contour can be divided into portions satisfying condition α with respect to the number ε . For, if not, successive subdivisions of T can be made yielding a sequence of subregions that never contain a region appropriately bounded. But any sequence so obtained converges to a limit point at which, however, the function $f(z)$ was differentiable. This implied a contradiction. The proof of the Cauchy integral theorem followed immediately. The given region T is divided up into congruent squares so small that condition α applies to them for an arbitrary but fixed $\varepsilon > 0$. Condition α allows the integral to be estimated by estimating values of $\int_C dz$ and $\int_C z dz$, which were bounded

by the perimeter and area of the squares. But for them the theorem is trivially true, and so, allowing ε to tend to zero, the full theorem is established. In the same issue E.H. Moore gave his own proof. He defined a continuous curve as the image in the complex plane of an interval under a continuous function, and $\int_C f(z) dz$ as the

limit as $\delta_k \rightarrow 0$ and $n \rightarrow \infty$ of the sum $\sum_{k=0}^n f(\zeta_k)(z_{k+1} - z_k)$, where $z_{n+1} = z_0$, ζ_k is any point on the arc (z_k, z_{k+1}) and $\delta_k = t_{k+1} - t_k$. He observed that the path integral exists if the curve is rectifiable, quoting a theorem in Jordan's *Cours d'analyse* to that effect (Jordan 1893, §§193–196). In a footnote he said he had done this because Pringsheim (1895a) had proved what were special cases of this result, and “seems to be unfortunately out of touch with the current notion of the general rectifiable curve”, as treated by Scheeffer, Ascoli, and Study.

Moore now proved a theorem about any single-valued function f which is continuous and has a single-valued derivative everywhere inside and on a region R bounded by a closed, continuous, rectifiable curve C , subject to the following conditions:

1. The curve C meets curves parallel to the x - and y -axes in only finitely many points, and, to simplify the proof.
2. If a sequence of squares whose sides are parallel to the x - and y -axes converges to a point A on C , then the ratio of the total lengths of the arcs of C inside the squares to the perimeter of the squares is ultimately less than some constant ρ_ζ , which may vary as ζ traverses C (for the usual curves considered, $\rho_\zeta = 1$ for all points ζ).

He then showed (Moore 1900) that under these conditions, $\int_C f(z) dz = 0$. The proof was the usual proof by contradiction, the various hypotheses being introduced to guarantee the existence of suitable estimates. The observation that for each $z \in R$, $f(z) = f(\zeta) + (z - \zeta)(f'(\zeta) + \Delta(z))$, where $|\Delta(z)| < \varepsilon|z - \zeta|$ whenever z is within a suitably small distance of ζ , reduced the evaluation of the integral to estimating sums of integrals of the form around suitable contours. Conditions (1) and (2) control the lengths of the parts of the curve C to be considered and the behaviour of ρ_ζ . A compactness argument is at work here under the surface. The Cauchy integral theorem followed immediately from Moore's theorem, and as he observed, requiring the boundary curve to be rectifiable allowed him to avoid Goursat's lemma.

In May 1901 Pringsheim presented his reply to the American Mathematical Society at its meeting in Ithaca; it was published in the second volume of the *Transactions*. He certainly did not agree that he was "out of touch". As a friend of the late Ludwig Scheeffer he could claim, he said, to be as well acquainted with the new ideas as any one, and he referred any doubting reader to his recent article in the *Encyclopädie der mathematischen Wissenschaften* (Pringsheim 1899b, 41). He now objected to Goursat's proof on the grounds that it was incautiously expressed: there was not only no need to use congruent squares, but if one were so restricted then only a restricted class of boundary curves could be admitted. It would be necessary to allow those that were only piece-wise monotonic (so their coordinate functions have only finitely many extrema). Moore's condition (1) is insufficient, as the example of $y = x^2 \sin(1/x)$ shows, to ensure that small enough squares meet the boundary curve C in at most two points. If that condition is not met, there may be curves that go back and forth through some of the squares. Pringsheim therefore proposed to subdivide only those squares for which Goursat's condition did not already hold, thus adapting the subdivision to the curve at hand and to exclude curves for which there was no suitable assembly of squares. This gave him a proof of the Cauchy integral theorem for rectifiable curves based on his (new) proof of Goursat's lemma.

Pringsheim returned to the question in 1903, when he gave the proof his obituarist Perron was to regard as definitive (see Perron 1952). He began by noting that Heffter (1902) had recently established that the integral $\int_C P(x,y)dx + Q(x,y)dy$ vanishes when taken along a closed curve, provided that $P(x,y)dx + Q(x,y)dy$ is an exact differential and satisfies the condition $\frac{\partial P}{\partial y} = \frac{\partial Q}{\partial x}$. This result contains the Cauchy integral theorem as a special case. But Pringsheim now wished to avoid his earlier use of step-shaped functions and to give a proof immediately applicable to contours

bounded by straight lines, such as triangles. To describe what he did, we need to explain his notation. He wrote f_1 for $\frac{\partial f}{\partial x}$, f_2 for $\frac{\partial f}{\partial y}$, and defined

$$f(x, y|x_0, y_0) = f(x, y) - f(x_0, y_0) - f_1(x_0, y_0) \cdot (x - x_0) - f_2(x_0, y_0) \cdot (y - y_0). \quad (8.15)$$

He said that a function $f(x, y)$ was (totally) differentiable at a point (x_0, y_0) if and only if $f_1(x_0, y_0)$ and $f_2(x_0, y_0)$ have values there and for all $\varepsilon > 0$ there is a $\delta > 0$ such that $|x - x_0| < \delta$ and $|y - y_0| < \delta$ implies

$$|f(x, y|x_0, y_0)| < \varepsilon(|x - x_0| + |y - y_0|).$$

He observed that uniform differentiability was a stronger condition than this. He could now state and prove the following result, now known as Pringsheim's Theorem: Let $P(x, y)$ and $Q(x, y)$ be differentiable in the interior and on the boundary of a triangle Δ , and suppose that $P_2(x, y) = Q_1(x, y)$, then $\int_{\Delta} P(x, y)dx + Q(x, y)dy = 0$.

To prove it, Pringsheim first showed that, if Δ is a triangle lying entirely inside T , and the integrals (taken in the positive direction) $\int_{\Delta} f(x, y)dx$ and $\int_{\Delta} f(x, y)dy$ are defined by restriction, then :

$$\int_{\Delta} f(x, y)dx = \int_{\Delta} f(x, y|x_0, y_0)dx + f_2(x_0, y_0) \int_{\Delta} ydx.$$

Similarly,

$$\int_{\Delta} f(x, y)dy = \int_{\Delta} f(x, y|x_0, y_0)dy + f_1(x_0, y_0) \int_{\Delta} xdy.$$

Then, to prove the theorem, Pringsheim argued that one could subdivide Δ into four congruent similar triangles, pick one for which the integral $\int_{\Delta} f(x, y)dx$ is largest (call it Δ_1) then

$$\left| \int_{\Delta_1} (P(x, y)dx + Q(x, y)dy) \right| \geq \frac{1}{4} \left| \int_{\Delta} (P(x, y)dx + Q(x, y)dy) \right|.$$

Proceed successively in this manner. One finds

$$\left| \int_{\Delta_n} P(x, y)dx + Q(x, y)dy \right| \leq 4^{-n} \left| \int_{\Delta} P(x, y)dx + Q(x, y)dy \right|.$$

The n th triangle Δ_n has perimeter s_n , say, and the perimeters halve at each stage, so if Δ has perimeter s then $s_n = \frac{s}{2^n}$. The triangles converge to a point (x_0, y_0) inside or on Δ . Because P and Q are differentiable, for any $\varepsilon > 0$, there is an n such that $P(x, y|x_0, y_0)$ and $Q(x, y|x_0, y_0)$ are each less than $\varepsilon(|x - x_0| + |y - y_0|)$ for all

$(x, y) \in \Delta_n$. Pringsheim applied the above observation to P and Q , used the facts that $P_2(x, y) = Q_1(x, y)$ and $\int_{\Delta_n} y dx + \int_{\Delta_n} x dy = \int_{\Delta_n} d(xy) = 0$, to obtain

$$\left| \int_{\Delta_n} P(x, y) dx + Q(x, y) dy \right| < \varepsilon \int_{\Delta_n} (|x - x_0| + |y - y_0|) |dx + dy|.$$

Finally, he argued that when n is taken large enough so that $|x - x_0|$ and $|y - y_0|$ are both less than $\frac{s_n}{2}$, then

$$\left| \int_{\Delta_n} (P(x, y) dx + Q(x, y) dy) \right| < \varepsilon \frac{s_n}{2} \int_{\Delta_n} |dx + dy| < \varepsilon \frac{s^2}{4^n}.$$

Whence, from the inequality relating integrals around Δ_n and Δ , he deduced

$$\left| \int_{\Delta} (P(x, y) dx + Q(x, y) dy) \right| < \varepsilon s^2.$$

But since ε can be arbitrarily small, the sought-for result follows. The Cauchy integral theorem follows on letting P and Q be the real and imaginary parts of a complex function $f(z)$; the integrability condition is one of the Cauchy–Riemann equations; Pringsheim also pointed out the proof can be easily adapted directly to the complex case.

The route to Goursat's proof is surprisingly intricate and closely related to what might be called the discovery of continuity: the realisation that once a curve is not smooth but merely continuous many expected properties may lapse, or at least be hard to establish. Familiar examples from the period include the Jordan curve theorem and Peano's space-filling curve. The acuity with which Pringsheim pounced on what seemed like a triviality to Goursat is a good example of what has to be done. In fact, the question of how to admit general, continuous boundary curves so that an integral taken around such a boundary curve makes sense is another story, barely begun in 1903.

Problems with the Cauchy integral theorem flared up again after 1929, when Pringsheim returned to rebut a charge levelled at him by Mittag-Leffler. The point at issue was a published remark of Mittag-Leffler's in (Mittag-Leffler 1923c) that quoted Pringsheim out of context and seemingly in error. Pringsheim wrote to Mittag-Leffler, who agreed he had made a mistake and offered to correct it at the first opportunity. That was in May 1925, but when Mittag-Leffler died in July 1927 restitution had not been made, so Pringsheim took up the issue himself.⁷⁸

⁷⁸See Pringsheim (1929).

Mittag-Leffler's mistake had been to confuse Pringsheim's remarks about the proof of the Cauchy integral theorem with the statement of the theorem itself. As Pringsheim saw it, the so-called Riemannian proof of the theorem, by a Green's theorem argument, was due to Cauchy before Riemann, and Riemann should be credited with introducing the theorem itself into Germany. But Mittag-Leffler had gone on to remind readers that he and others had priority over Goursat. The first was the Swedish mathematician C.J. Malmsten (in [Malmsten \(1865\)](#), which we have not seen) then Mittag-Leffler himself (in [Mittag-Leffler \(1873\)](#) and [Mittag-Leffler \(1875\)](#)) and independently Briot and Bouquet. These contributions seem to have been forgotten, and so Mittag-Leffler took the occasion of the appearance of yet another proof (this one in [Borel \(1917\)](#)) to remind readers of the earlier work. His own early proof of the Cauchy integral theorem in his (1873) being in Swedish, he recapitulated the proof in German 2 years later, in the *Göttinger Nachrichten*. In his (1895a) Pringsheim had criticised it for tacitly assuming that the quantity $\left| \frac{f(z+h)-f(z)}{h} - f'(z) \right|$ converged uniformly to zero as $h \rightarrow 0$. In the letter of 1925, Mittag-Leffler implied that the fault lay in the German translation, and referred Pringsheim to a new, more accurate French version (of which he enclosed a copy). Never one to be fobbed off, Pringsheim recruited a Swedish mathematician who spoke good German to make a new translation of the Swedish original. He found that the German edition amounted to the first half of the Swedish version, but where they overlapped they had only inessential differences. Both texts agreed in assuming:

1. That the function $f(x)$ was (in addition to being finite and continuous) such that it had a single-valued and finite derivative $f'(x)$.
2. In making no mention at all of the uniform convergence of the quantity

$$\left| \frac{f(x+h)-f(x)}{h} - f'(x) \right|.$$

However, in the new French version, and its German translation, matters were the other way round. Now the above assumption (1) was missing, but assumption (2) now appeared, in the form of an assumption that

$$\left| \frac{f(\rho_1 e^{\theta_1 i}) - f(\rho e^{\theta_1 i})}{(\rho_1 - \rho) e^{\theta_1 i}} - \frac{f(\rho e^{\theta_1 i}) - f(\rho e^{\theta i})}{\rho (e^{\theta_1 i} - e^{\theta i})} \right| < \varepsilon$$

held uniformly for $|\rho - \rho_1| < \delta$, $|\theta - \theta_1| < \delta$, and for all $z = \rho e^{i\theta}$ in the annular domain $R_0 \leq \rho \leq R$, $0 \leq \theta < 2\pi$. This assumption crucially makes no reference to the existence and equality of the two differential quotients, but only to the uniform vanishing of their difference (from which the Cauchy integral theorem can be derived).

Now, said Pringsheim, a quick look at the proof of 1873 shows that the assumptions about $f'(x)$ are used only to establish the equality of this difference in the limit, which means that it would have been enough to assume precisely

such a limiting equality. A more precise argument then shows that it is sufficient to establish the result that the limiting property holds uniformly. What to make of this muddle? Pringsheim took the shrewd view that in 1873 the idea of uniform convergence and the awareness of its indispensability was not yet in the shared opinion of mathematicians. Even Weierstrass, who had led the way in emphasising the importance of the concept, had seen fit to explain the uniform convergence of a sequence of rational functions carefully in a footnote to a paper of 1880 (see [Weierstrass 1880c](#), 202), and in 1873 Mittag-Leffler had yet to make his trip to Germany and hear Weierstrass lecture for the first time. Thereafter he took the Weierstrassian approach to analysis so firmly to heart that he perhaps read into his earlier work arguments that were not in fact there. So Pringsheim was inclined to credit Mittag-Leffler with being the first to have the idea that the Cauchy integral theorem could be proved without assuming the function to be continuously differentiable and for being the first to have some success in that direction. But priority could not be claimed for the proof of 1923, for a rigorous proof of that kind had been given by Lichtenstein in 1910.

In that paper, Pringsheim explained, Lichtenstein had shown how to push through a Green's theorem approach to the Cauchy integral theorem first with and then—surprisingly—without assumptions of uniformity. Pringsheim argued that Lichtenstein's proof finally showed clearly what lay behind Goursat's proof. Lichtenstein had considered the (in Pringsheim's view inappropriately named) Green's formula:

$$\int \int_T \left(\frac{\partial Q}{\partial x} - \frac{\partial P}{\partial y} \right) dx dy = \int_{\partial T} P dx + Q dy,$$

where P and Q are functions of real variables x and y , continuous in the region T and on its boundary, and the partial derivatives are taken to be continuous and single-valued. The boundary ∂T of the region T is taken to be a rectifiable Jordan curve and the right-hand integral is taken along it in the positive sense. He then defined

$$\Delta_x Q = Q(x + \delta, y) - Q(x, y) \text{ and } \Delta_x P = P(x, y + \delta) - P(x, y),$$

and observed that Green's formula was equivalent to the claim that

$$\int \int_T \lim_{\delta \rightarrow 0} \frac{1}{\delta} (\Delta_x Q - \Delta_y P) dx dy = \int_{\partial T} P dx + Q dy$$

provided the two limits $\lim_{\delta \rightarrow 0} \frac{1}{\delta} (\Delta_x Q)$ and $\lim_{\delta \rightarrow 0} \frac{1}{\delta} (\Delta_x P)$ exist and are continuous in T , which he showed by vindicating the exchange of the limits and integration. This showed that Green's formula was equivalent to the claim that

$$\lim_{\delta \rightarrow 0} \int \int_T \frac{1}{\delta} (\Delta_x Q - \Delta_y P) dx dy = \int_{\partial T} P dx + Q dy$$

Lichtenstein's crucial insight was that this argument could be reversed, and Green's formula deduced without requiring that the two limits $\lim_{\delta \rightarrow 0} \frac{1}{\delta} (\Delta_x Q)$ and $\lim_{\delta \rightarrow 0} \frac{1}{\delta} (\Delta_x P)$ exist and are continuous in T . Instead it was enough to show the weaker requirement that $\lim_{\delta \rightarrow 0} \frac{1}{\delta} \iint (\Delta_x Q - \Delta_y P) dx dy$ was a continuous function of x and y in T . Lichtenstein proved the theorem by reducing it to the special case where the boundary of the region is a triangle.

Pringsheim noted that the Cauchy integral theorem now followed on setting $x + iy = z$ and letting P and Q become complex functions:

$$P(x, y) = f(z), \quad Q(x, y) = if(z).$$

Green's formula then says that $\int_{\partial T} f(z) dz = 0$ if $\lim_{\delta \rightarrow 0} \frac{1}{\delta} (i\Delta_x f(z) - \Delta_y f(z)) = 0$. This is the Cauchy integral theorem without any assumption about the differentiability of $f(z)$, almost exactly as Mittag-Leffler had proclaimed it.

Pringsheim's paper seems to have re-opened the question. In his paper (1932) Kamke astutely asked what it was that the Cauchy integral theorem actually said. Which of the following was it?

1. If a function $f(z)$ is regular in a simply-connected domain bounded by a closed continuous, rectifiable curve C , then $\int_C f(z) dz = 0$;
2. If a function $f(z)$ is regular in a domain bounded by a closed, rectifiable Jordan curve C , and it is regular on C , then $\int_C f(z) dz = 0$;
3. If a function $f(z)$ is regular in a domain bounded by a closed, rectifiable Jordan curve C , and it is continuous inside and on C , then $\int_C f(z) dz = 0$.

He observed that proofs of the first version could be found in these books: (Bieberbach 1930, 118)⁷⁹ and (Knopp 1930, 1, 56–61), and of the second version also by (Knopp 1930, 1, 62–63),⁸⁰ but he knew no proof of the third, although it was stated in that form in (Osgood 1929, 369) and (Hurwitz and Courant 1929, 283). However, Knopp's proof of (2) seemed to need some more care.

Knopp had reduced (2) to (1) by the Heine–Borel theorem, arguing that C and its interior can be covered by finitely many circles inside each of which $f(z)$ is regular, thus giving a larger region G containing C and for which the first result was true. Accordingly version (2) followed. But Kamke felt this was a little glib. So he first showed that the function f extends to a function g which is regular on G . To do this he covered the boundary C by discs, took a finite subcover of boundary, and then argued carefully that the analytic continuation of the individual function elements yielded a single-valued function. This still left version (3) without what Kamke

⁷⁹This is the 3rd edn. of Bieberbach (1921b).

⁸⁰Kamke quoted from the 4th edn. of Knopp (1913), which reproduced the proof given in the first 1913 edition at the same pages.

presumably regarded as a satisfactory proof, although he did not specify what he found wrong with the published attempts.

His paper stimulated Denjoy (see his (1933)) to prove (3) in the form: if a function $f(z)$ is defined in a domain bounded by a closed, rectifiable Jordan curve C , has a finite derivative inside C , and it is continuous inside and on C , then $\int_C f(z) dz = 0$. To prove this result Denjoy took an arbitrary plane set E and considered the squares (closed, containing their boundaries, of side δ) which have an interior or boundary point in common with E (which he called these a polygonal approximation to E). He then showed that, if G is a simple, rectifiable Jordan curve of length L , any polygonal approximation to G having more than 8 sides with a perimeter less than $16L$. (Denjoy assumed that δ is less than L .) He argued that one could work round the boundary of the curve G and its polygonal approximation picking vertices N common to two squares and points M which are (among the) closest to N in the corresponding squares so that the points N and M occur in the same order.

It follows that the length of the polygonal curve defined by the M s is at most L . Denjoy then argued by contradiction that as a result if $N_1, \dots, N_8$ are 8 consecutive vertices then at least 2 of the corresponding points M are at least δ apart. Consequently there are at least two points M which are less than δ apart. This in turn implies the claim about the sides of the polygonal approximation.

This done, Denjoy let took a region R bounded by a Jordan curve C of length L (in fact, Denjoy considered finitely many Jordan curves, but that makes for no extra difficulty). He let $f(z)$ be a holomorphic function defined inside R which is continuous inside and on C . Then, if A is a point of R and δ is sufficiently small, the boundary, σ , of the domain formed by the squares in the polygonal approximation to C that also contain the point A is a polygonal approximation to the curve C having at least 10 sides and therefore a perimeter of length less than $16L$. Now, Goursat's theorem applies to the domain σ . Consider the curve g defined by the line segment NM , the arc MM' of C , the segment $M'N'$ (where N' is the next vertex of the polygonal approximation after N) and the side NN' of the polygonal approximation.

Let $\omega(g)$ be the oscillation of f on a curve g and $\omega(\delta)$ the maximum value of $\omega(g)$ (over all the curves g). Then $|\int_C f(z) dz| = |\int_C f(z) dz - \int_\sigma f(z) dz| = \left| \sum \int_g f(z) dz \right| < \omega(\delta) \cdot (\sum \text{length}(g)) < \omega(\delta) \cdot (16 + 16\sqrt{2} + 1)L$. But as δ tends to zero so does the largest dimension of each g and so $\omega(\delta)$ tends to zero, and Goursat's theorem is proved in the form stated.

What may be the last word on the matter was then given by the Harvard mathematician J.L. Walsh, in his one-page paper (1933). His proof was, as he said, "much more immediate than that of Denjoy, although not so elementary". Walsh began by observing that the Cauchy integral theorem (in the third of Kamke's forms) was true for a polynomial, because it was then possible to replace the contour C by a suitably chosen polygon. However, the given function can be represented in the closed interior of the contour as the limit of a uniformly convergent sequence of polynomials, because the function is analytic inside C and continuous inside and on

C. This sequence can be integrated term by term, and so the result is established. The theorem can be extended to regions bounded by finitely many non-intersecting rectifiable Jordan curves by replacing the polynomial approximations with rational functions whose poles lie outside the regions considered.

8.6 Series of Analytic Functions

We have earlier characterised Weierstrass's theory of analytic functions as power series algebra with uniform convergence. But a general theory of the convergence of a series of functions to an analytic function was lacking, and it is surprising to note that the study of sequences and series of analytic functions was delayed until the end of the century.

Almost by definition, every power series is the limit of a sequence of polynomials, but while polynomials are always well behaved, power series exhibit many different properties with respect to their circle of convergence. If polynomials are replaced by rational functions, more strange things can happen, as we saw when we discussed Weierstrass's example of the series $\sum_{n=1}^{\infty} (z^n + \frac{1}{z^n})$ (see Sect. 6.9.1). Runge's theorem (Runge 1885a) showing that any single-valued (or "monogene") analytic function can be represented by a series of rational functions valid everywhere in the natural domain of definition of the function, marks the extent of what can happen in this direction.

Carl Runge studied mathematics at Berlin, where he was influenced by Weierstrass and Kronecker.⁸¹ In September 1884 he visited Mittag-Leffler in Stockholm and quickly produced some papers on complex function theory for *Acta mathematica* before turning to a successful career in applied mathematics. In his (1885a) Runge showed that any path-connected domain in $\mathbb{C}$ can be the domain of definition of a single-valued meromorphic function—thus solving the domain of holomorphy problem for functions of a single variable. This result had already been established in (Mittag-Leffler 1884a) but Runge's work was done independently and made an elegant use of Cauchy's integral theorem, as Mittag-Leffler was gracious enough to note in a footnote he added to Runge's paper. Interestingly enough, it seems that Runge first proved his theorem without resorting to Cauchy's theorem but then turned it because "This path satisfies me much more than my original one", as he put it in a letter to Hurwitz on October 31, 1882 (quoted in Richenhagen (1985), 62).⁸² (Hurwitz, 1897, 464) commented that Runge's proof was more simple and

⁸¹For Runge's biography, see Richenhagen (1985).

⁸²Runge may have already been departing from Weierstrassian orthodoxy. To Sonya Kovalevskaya Runge confided that "The subject does not interest me enough any more. These developments are too general. What general developments, when the function is above all only defined in any way whatever, and indeed it is defined by the Cauchy integral in a very lovely way. And whether there are functions with arbitrary natural boundaries is also a matter of some indifference to me". Runge to Kovalevskaya, 24 January 1884, quoted in (Richenhagen 1985, 62).

elementary than Mittag-Leffler's. In fact it did more: it showed how to approximate a single-valued meromorphic function on a domain by rational functions.

Runge first proved that given a bounded domain B composed of a finite number of disjoint path-connected domains, f a meromorphic function defined on B , and C a domain disjoint from B there is a rational function $R(x)$ which is arbitrarily close to f on B and to the zero function on C . He argued that B can be contained in a larger domain B' that is also disjoint from C and that the integral $f(x) = \frac{1}{2\pi i} \int \frac{f(z)dz}{z-x}$, taken round the boundary of B' defines a meromorphic function that vanishes when x lies outside B' , and in particular when $x \in C$ but can be arbitrarily well approximated by a rational function (determined by f) when $x \in B$. He then deduced that if all the poles of a rational function $R(x)$ lie in a number of disjoint domains then there is a rational function which becomes infinite at an arbitrary point in each of these domains and only at those points and outside those domains agrees arbitrarily exactly with $R(x)$.⁸³

In his (1885b) Runge then showed that a series of rational function that converges non-uniformly can still represent a single-valued ("monogene") analytic function, contrary to an impression that might have been gained from reading (Weierstrass 1880c) where the convergence was in fact uniform. He also showed that if a series converges uniformly on the boundary of a simply connected domain, then it converges uniformly in the interior of that domain.⁸⁴

The first result of Runge's recalls a letter Jules Tannery sent to Weierstrass (who communicated it to the Berlin Akademie in 1881, see Weierstrass *Werke* II, 231–233) concerning the series

$$\frac{a + bc z^{m_0}}{a + bz^{m_0}} + \sum_{j=1}^{\infty} \left(\frac{a + bc z^{m_j}}{a + bz^{m_j}} - \frac{a + bc z^{m_{j-1}}}{a + bz^{m_{j-1}}} \right),$$

where the sequence (m_j) tends to ∞ with j . This converges uniformly to the value a in any disc of radius less than 1, and it converges uniformly to the value c outside any disc of radius greater than 1, but it does not converge at all on the circle $|z| = 1$. The simplest case occurs when $b = -1$ and $m_j = 2^j$, when the function reduces to

$$\frac{a - cz}{1 - z} + (a - c) \sum_{j=0}^{\infty} \left(\frac{1}{z^{2^j} + z^{-2^j}} \right).$$

Using this function, it is possible to construct a function which equals an arbitrary function $F(z)$ inside the unit circle and another arbitrary function $G(z)$ outside the

⁸³This ability to vary the location of the poles of a rational function (within certain domains) is called "pole pushing" in the modern literature. The modern form of this result isolates the poles of the rational function $R(x)$ and considers a holomorphic function on a closed bounded but not necessarily simply connected domain B ; the claim is that the given function can be approximated uniformly by a sequence of rational functions having poles only in the components of the complement of B in $\mathbb{C}$.

⁸⁴Montel observed in his (1912b) that uniform convergence on the boundary is a necessary condition for term-by-term differentiation to be valid.

unit circle. For we can take Tannery's series $\theta(z)$ with $a = 1$ and $c = 0$, which is 1 inside the unit circle and 0 outside it, and then the function $G(z) + (F(z) - G(z))\theta(z)$ has the required property. More generally, as Weierstrass showed, if a function given in the form $\sum_j f_j(z)$ is continuous in a set of disjoint domains U_j the series "represents a single function in each domain but does not necessarily represent the same function in different parts", to quote (Forsyth 1918/1965, 164).

Runge's work probed the question of what functions can be represented by a series of analytic functions, and when the sum of a convergent series of analytic functions is itself analytic. Mittag-Leffler's theorem on functions defined on a star-shaped domain (Mittag-Leffler 1884a) had shown that if they have only isolated singular points then they can be represented by a unique series that converges at every point where the function is defined. But the set of singular points can be much more complicated than an isolated set, and in 1898 Painlevé showed that every holomorphic function can be represented by a series of rational functions that converges absolutely and uniformly everywhere that the function is defined. The series is not unique, the poles of the rational functions lie among the singular points of the given function, and if the singular points of the function form a perfect set then the poles of the rational functions are all simple.⁸⁵

As Stäckel, echoing Borel, noted in his review of (Borel 1901a) in *Fortschritte* 31.0411.01, a refinement of this paper by Painlevé seemed to diminish the interest in Runge's theorem that every single-valued holomorphic function can be represented by a series of rational functions, for Painlevé showed that sums of rational functions can represent arbitrary single-valued holomorphic functions defined on arbitrary disjoint domains. This does not detract from the topologically interesting result that Runge had established in (Runge 1885a) that any connected domain can be the domain of a holomorphic function whose only singular points are poles. His proof invoked the idea of successive approximations to a given holomorphic function by means of rational functions, and it is this theorem had been put in the shade by Painlevé's generalisation.

What Borel focussed on in this paper and at greater length in the first edition of his *Leçons*, 1900, was the fact that the poles of the rational functions in the series that represents a given function $F(z)$ need have nothing to do with the singular points of $F(z)$ itself. Mittag-Leffler's theorem was much better in this respect, and Borel now showed that analogous results can be obtained for quite simple series of rational functions representing a given function with an arbitrary distribution of poles (see also Borel (1901b)).

Osgood took up similar questions in his short paper (Osgood 1902b). He began by observing that if the series $f_1(x) + f_2(x) + \dots$ of continuous real functions converges everywhere in an interval to a function $F(x)$ it need not converge to a continuous function anywhere in the interval. Nor need it do so even if the functions $f_n(x)$ are real analytic in the interval. Nor, necessarily, was a pointwise convergent series of complex analytic functions on a domain any better. The series will converge

⁸⁵See (Painlevé 1898). Similar results had been obtained in (Hilbert 1897a).

uniformly on subsets of the domain to an analytic function (but, as Montel showed with an example in his (1907), the convergence may be to different functions on infinitely many different domains). So the question was: what extra assumptions must be made before the function $F(z)$ defined by $F(z) = f_1(z) + f_2(z) + \cdots$ on a domain T converges to an analytic function on the domain? The answer Osgood found was couched in the language of the newly emerging theory of sets of points and singled out a simple boundedness criterion. He showed that if convergence is assured for an everywhere dense subset S of T and there exists a constant C such that $|f_1(z) + f_2(z) + \cdots + f_n(z)| < C$ for all $z \in S$ and all n , then the series converges for all $z \in T$ to a function analytic throughout T . This result was improved by M.B. Porter (1904), who showed that it was enough to insist on convergence on a set of points everywhere dense on a rectifiable curve C lying entirely inside the domain T .⁸⁶ The requirements were therefore, informally, that the convergence be uniform and the limiting function be continuous.

Osgood's theorem was proved by passing to the real and imaginary parts of the f_n 's and invoking Harnack's theorem on the limit of a sequence of harmonic functions. Porter showed that the convergence was uniform on the contour and then, by applying the Cauchy integral theorem, he proved that the series converges uniformly to an analytic function inside the contour. The second part of the argument, Porter wrote, is one that "though doubtless well known, I can give no reference (to) in the literature". In (1902–1903) the Italian mathematician Arzelà simplified the proof by applying the idea of equicontinuity introduced by Ascoli in his (1884) and by himself in (1895–1896) (see (7), commenting on the theorem that its "generality and importance (was) obvious". Following this, Vitali (1903) showed that if a series of analytic functions $\sum f_n(z)$ in a domain T converges on a point set with an accumulation point in T , and the sequence of its partial sums is uniformly bounded in T , then $\sum f_n(z)$ converges for all points of the domain T , and $F(z) = \sum f_n(z)$ is analytic in T .

Then in 1907 the French mathematician Paul Montel published what became the definitive point of reference for future work in this area. Paul Montel had graduated from the École Normale in 1897. His sociable manner and liking for the quiet life meant that the customary position of a professor in a provincial lycée was quite congenial to him, and his friends had to urge him not to waste his talents (Dieudonné 1990, 649). He therefore took his doctorate only in 1907, at the age of 31, with a thesis on infinite sequences of functions.⁸⁷ Among his results concerning analytic function were new proofs of the theorems of Osgood and Porter, and, by passing to the harmonic functions associated with a complex function, of Harnack's

⁸⁶Milton Brockett Porter was a student of Halsted's at the University of Texas. He took his PhD at Harvard in 1897 and returned to Texas as Halsted's successor in 1903, where he remained for the next 42 years. See Parker (2005).

⁸⁷As was customary, his thesis, or a modified version of it, was then published in the *Annales* of the École Normale (1907). In later life he was Dean of Faculty of Science during the Second World War; Dieudonné (1990, 649) wrote that "he was able to uphold the honour of the French university" in those difficult times.

theorem, thus inverting Osgood's route and contributing to the emancipation of complex function theory from the techniques of harmonic function theory. Other results concerned sequences of holomorphic functions of a single variable which are bounded on some domain and converge pointwise but not necessarily uniformly. He was able to show that in a wide class of cases the sequence converges to a holomorphic function.

An important shift in Montel's approach came with his decision to study families of functions enjoying a certain property. Interest in sets (families, spaces) of functions existed in many parts of mathematics by 1900 and was a growing topic. The calculus of variations is one example, the emerging field of the functional analysis in its various forms was another.⁸⁸ Among Montel's peers, Fréchet shared the feeling that the study of such large spaces by topological methods introduced for the purpose would be productive, and his thesis (Fréchet 1906) is one of the most important occasions in the gradual recognition of the importance of the concept of compactness in mathematics. It is, however, Montel to whom Dieudonné attributed the merit of having shown the fundamental place of compactness in analysis (Dieudonné 1966, 86).

After writing his thesis Montel proceeded to sharpen his ideas and isolated the concept of what he called a normal family of functions. He defined a normal family to be a set, F , of holomorphic or meromorphic functions defined on a domain T with the property that every sequence of functions in F has a subsequence which either converges uniformly or tends uniformly to ∞ on every compact set in T . He showed that Arzelà's theorem of 1895 established that a family is normal if it is equicontinuous and uniformly bounded, so normal families are a natural collection to consider. (In fact, a family is equicontinuous if and only if it is normal.)

Montel's most remarkable and profound result about normality was his discovery that a family of functions holomorphic on a fixed domain is normal if there are two finite values that are never taken by any function in the family. Similarly a meromorphic family is normal if it omits three distinct values. This result recalls Picard's theorems, and indeed one of the motivations for Montel's work was to obtain a proof of those results without invoking the elliptic modular function. He began a series of investigations of this theme in the paper with which he returned the compliment paid him by Carathéodory and Landau.⁸⁹ Here he showed, with an explicit use of the elliptic modular function, that the family of all analytic functions omitting the two values 0 and 1 is normal (the functions we called "well-behaved" earlier). From this it follows that the subfamily of such functions bounded at $z = 0$ is uniformly bounded in the interior of the unit disc, which is equivalent to Schottky's theorem. The proof is based on Picard's big theorem, for which Montel provided a simple proof in his (1912b, 512–514).

Montel's next paper, his (1916), gave a different proof of the normality of Schottky families, based on Borel's proof of Picard's theorem. This gave a self-

⁸⁸See (Siegmond-Schultze 1998 and 2003) and (Fraser 2003).

⁸⁹See (Montel 1912a), expanded as (Montel 1912b).

contained exposition, and Montel could claim that whereas Borel had proved the little Picard theorem by considering a single function, he had been able to establish Schottky's theorem by considering a family.⁹⁰

Montel also proved Picard's big theorem by observing that if $f(z)$ is a function with an isolated singular point at $z = 0$ and if it omits two values in some neighbourhood of $z = 0$, then the family of functions $f(2^n z)$ is a normal family on an annulus centre $z = 0$. But this contradicts the hypothesis that $z = 0$ is an essential singular point.

Montel also obtained important consequences for the convergence of sequences of analytic functions from his theory of normal families. In his (1912, 531) he showed that any sequence of holomorphic functions drawn from a normal family defined on a domain D that converges on an infinite subset of D converges everywhere in D and that if the sequence converges D it does so uniformly in the interior of D . The first of these results was, as Montel noted, a theorem due to Carathéodory and Landau in their (1911) (see above Sect. 8.5.2) but now it displayed the elegance and apparent simplicity of the idea of normal families.

In his (1966) Dieudonné, a former student of Montel's, observed that Montel's introduction of normal families [brought] substantial simplifications in the proofs of many classical results of function theory, such as the mapping theorem of Riemann and Hadamard's characterisation of entire functions of finite order. The fecundity of Montel's ideas are typical of a number of breakthroughs that occurred in mathematics at this time with the study of families of functions rather than individuals. This process was not ended by the First World War, but it would take us too far afield to trace all these important ramifications.

8.7 Fatou, Julia, and the Iteration of Rational Functions

Another, rather more unexpected, application of the theory of sequences of analytic functions was to the study of the iteration of rational functions.⁹¹ To pick up this story in the middle, in his (1890b) Poincaré had taken up Weierstrass's interest in functions $f(z)$ which, like the elliptic and Abelian functions, satisfy a multiplication law, and he was led to ask when the limit of successive iterates of such a function, $f(z)$, $f^2(z) = f(f(z))$, $f^n(z) = f(f(\dots f(z)))$, was a constant. A solution of the equation $f(z) = z$ is called a fixed point of f . When $f(z)$ is itself rational with

⁹⁰The Schottky inequality principle re-appeared in the French literature in 1918 in the third edition of Goursat's *Cours d'analyse*, vol. 2, Appendix, pp. 651–663, which followed the lines of (Schottky 1906).

⁹¹See the book (Alexander et al. 2011) for a thorough history documenting the subject's origins in work on functional equations and its continuation to the celebrated prize competition and beyond, and also (Alexander 1994) and (Alexander 1996).

distinct fixed points α such that $|f'(\alpha)| > 1$ he showed there is a meromorphic function $\theta(z)$ such that $\theta(f'(\alpha)z) = f(\theta(z))$.⁹²

In his short paper (1918) Lattès called this function $\theta(z)$ the Poincaré function relative to α .⁹³ It is a meromorphic function. Lattès wrote S for the multiplier $f'(\alpha)$ and considered the sequence $w_n = \theta(S^n z)$ which has α as a limit point as $n \rightarrow -\infty$. It follows that among the antecedents of an arbitrary point z one can choose a sequence of successive antecedents z_n tending, as $n \rightarrow -\infty$ to any of the fixed points α with multiplier S such that $S > 1$. The problem he then set himself was to determine the derived set E' of the set of consequents $z, z_1 = \theta(Sz), z_2 = \theta(S^2 z), \dots, z_n = \theta(S^n z), \dots$ of an arbitrary point z . As he observed, the set E' is the natural object to study because it contains the consequents of each of its points. However, he contented himself with giving just one example. From the equation for the Weierstrass function $\wp(2z) = \frac{(\wp^2(z)+1)^2}{4\wp(z)(\wp^2(z)-1)}$, which applies when $g_2 = 4$ and $g_3 = 0$, he defined $z_1 = \frac{(z^2+1)^2}{4z(z^2-1)}$. In this case the Poincaré function is $\wp$ and the multiplier is $S = 2$, so $z_n = \wp(2^n z)$. Although he did not say so clearly, this is an example where E' is the whole Riemann sphere.

The stage was then set for two long papers, one by Fatou and one by Julia, which took the theory of the iteration of rational functions about as far as it was able to go until the advent of high powered computers. Their papers overlap strikingly in content. We first discuss Julia's memoir and then Fatou's, noting where each contributed something different. We have found it helpful to follow Julia's plan and describe the results he found and then indicate the methods he used to establish them, without entering into detail.

8.7.1 Julia

Julia's paper was awarded the Grand Prix of the Académie des sciences in Paris the year the war ended. Julia himself had been grievously wounded on 25 January 1915, but through a considerable effort of will, and with the support of the mathematicians Humbert and Picard, and his future wife, the career that had seemed to beckon when he had placed first for both the École Polytechnique and the École Normale in 1911 was resumed. He took a doctorate from the University of Paris in 1917 and won the Prix Bordin of the Académie des sciences in the same year for a study of non-quadratic binary forms (Julia 1917).

Julia began his *Mémoire sur l'itération des fonctions rationnelles* (Julia 1918) by noting that the global study of the problem had hitherto been studied only by Fatou

⁹²Fatou (1917a, b) showed there is always such a fixed point.

⁹³As mentioned by Audin, Lattès died of typhoid in the summer of 1918 at the age of 45. His work is discussed in (Alexander et al. 2011, Chap. 7).

in two short notes that made great use of the particular features of his examples; Julia proposed to sketch a general theory. He singled out the question of describing the derived set e' , of a set $e = \{z_n | z_n = \phi^n(z)\}$ consisting of all the images of a point z under iteration of a rational function ϕ . He asked such questions as (1918, 122): given a point ζ in e' , what is the domain in which z could vary so that ζ is an analytic function of z ? This is to be understood much as one understands claims about multi-valued functions. Well-known examples showed that ζ could be one constant as z varied in one part of the plane and another constant while z varied in another part, so it was necessary to find what he called the separators, that is set of points, z , at which ζ was not an analytic function. Fatou, Julia noted, had given examples in (Fatou 1906) that showed that this set could be perfect and discontinuous or else a curve. In either case it contained all the points which are fixed points of some iterate of ϕ (i.e. where $z = \phi^p(z)$) for which $|\phi^{p'}(z)| > 1$. In modern terms the separators form the Julia set, and the observation is that it contains the repelling periodic points.⁹⁴

Julia therefore decided to study the set of periodic repelling points, a set he denoted E , with a view to showing that its derived set is exactly the set of separators. He surveyed the results he had been able to obtain in these terms. The set E is countable. Montel's theory of normal families applied to the functions $\phi^n(z)$ (with n sufficiently large) on any disc centred on any point $P \in E$ showed that these functions take all but at most two values in the entire plane, and the cases where there are exactly one or exactly two omitted values could be analysed completely. This suggested by analogy with Picard's theorem that the set E and its derived set E' was the set of separators. Julia could show that E' contained E and that it was perfect.⁹⁵ Moreover, every point of E' had the same property that points of E did. So he set himself the task of establishing the structure of the set E' for any rational function ϕ . He could find examples where it was discontinuous or a curve, and if the set E' contained a two-dimensional region he could show that it must be the entire plane. The comparison with the limit sets of Fuchsian and Kleinian groups, he said, had suggested to him that indeed the set E' could be the whole plane, but he had been unable to adduce an example: (Lattés paper had not been published when Julia finished the paper, but for the footnotes, at the end of 1917). However, he could show that, as he put it, the structure of E' was the same in each of its parts. That is, it could not be a union of a discrete set and a set of curves.

The case where $E' = \mathbb{C}$ aside, he could show that in any domain D that contained no point of E' the functions $\phi^n(z)$ formed a normal family and so every limit point ζ of a point $z \in D$ depended analytically on D . So indeed E' was the sought-for set of separators. This is the result in modern terms that the Julia set is the closure of the set of repelling periodic points.

⁹⁴A fixed point z of a function f is said to be repelling if $|f'(z)| > 1$. A point of period p is repelling if $|(f^p)'(z)| > 1$. Similarly, fixed and periodic points for which the absolute value of the derivative is less than 1 are called attractive, and neutral if that absolute value = 1.

⁹⁵A set is perfect if it coincides with its derived set.

Julia noted with regret that his solution was, as he put it, “theoretical” and not fully satisfactory. As he put it: “Poincaré observed very shrewdly ‘There are not problems which are solved and others that are not; there are only problems more or less solved, etc.’”. However, it was impossible to obtain in any easy way a geometrical description of the set E' , as even to locate one point on it required solving infinitely many algebraic equations.

He had, however, been able to find results about the scope of convergence to the attractive fixed points—to give the modern term. These are the points where $z = \phi(z)$ and $|\phi'(z)| < 1$. He could also handle the situation where $z = \phi^p(z)$ and $|\phi^{p'}(z)| < 1$ and the points

$$\{z, \phi(z), \phi^2(z), \dots, \phi^p(z) = z \mid |\phi^{p'}(z)| < 1\}$$

—in modern language, the attracting p -cycles. The points exist in some cases but not in all, said Julia. All the points of such a p -cycle live in the same region, R , which was bounded by the set E' , and every point of that region was attracted to the p -cycle. He called the region R the immediate domain of convergence for the p -cycle. It was, he said, of capital importance for the theory that every immediate domain of convergence contained a critical point of the inverse function f^{-1} , that is to say a point which is a consequent where $f'(z) = 0$. But, he pointed out, the set of all points attracted to an attracting p -cycle could be much greater; it might consist of infinitely many regions, separated by E' and whose boundaries belonged to E' .

The immediate domain of convergence was, he claimed, either simply connected or infinitely connected. That being so, if there were more than two attracting cycles at most one of the attracting cycles has a total domain of convergence that coincides with its immediate domain of convergence, while all the others have total domains of convergence that are in infinitely many pieces.

Julia gave several illustrative examples. Examples obtained from applying Newton’s rule displayed an infinitely connected total domain of convergence. So did the iteration

$$z_1 = \frac{-z^3 + 3z}{2}$$

for which the set E' was a continuous closed curve

$$z_1 = A \left(\frac{z^5}{5} - 2a \frac{z^4}{4} + a^2 \frac{z^3}{3} \right)$$

for a sufficiently large value of A is one where the domain of points attracted to ∞ is infinitely connected and bounded by an infinity of distinct closed curves exterior to one another. It could be that E' formed a Jordan curve. Fatou’s example of 1906,

$$z_1 = \frac{z + z^2}{2}$$

was one where it was known that the set E' was a non-analytic curve; Julia showed that it was a simple closed Jordan curve having an everywhere dense set of points where it had no tangent. It could also be that E' was made of a curve with a dense set of double points made up as the union of infinitely many Jordan curves. Such a curve was obtained from the function

$$\phi(z) = \frac{-z^3 + 3z}{2}.$$

The iteration $z_1 = z^2 - 2$, however, was one where the set E' was a non-closed curve, in this case the segment $(-2, 2)$ of the real axis.

Julia concluded his list of examples by indicating where his research was inconclusive. He had some results about the convergence to points $\zeta = f(\zeta)$ where $f'(\zeta) = e^{i\theta}$ or, more generally, to points $\zeta = f^p(\zeta)$ where $f^{p'}(\zeta) = e^{i\theta}$, where p was real and a rational multiple of p , but he said he was less happy when θ was incommensurable.⁹⁶

Julia then surveyed the main tools he had used to get so far. They were:

1. Elementary results, which he attributed to Koenigs, about fixed points $\zeta = f(\zeta)$ for which $|f'(\zeta)| < 1$, which showed that they deserve the name attracting fixed points, and the obvious generalisation to attracting p -cycles (Koenigs 1884).
2. Results in the theory of point sets, for which he referred the (French) reader to Zoretti's (1912) article in the *Encyclopédie des sciences mathématiques*, the French edition of the *Encyclopädie der mathematischen Wissenschaften*. Here Julia cited the Jordan curve theorem, and the fundamental definitions of interior and exterior points and of a closed set.
3. More interestingly in the present context, he had made considerable use of Montel's theory of normal families, and in particular the result that a family of meromorphic functions which omits three distinct values is itself normal. He also used the interior Riemann mapping theorem, which he connected to Poincaré's proof in 1907 of the uniformisation theorem and to Carathéodory's proof of both the interior and the extended Riemann mapping theorem. He also cited Lindelöf's simplification of this result (Lindelöf 1915).
4. There were also some more anonymous results in the theory of functions. These were useful in showing how the study of the iteration of a function ϕ defined on a domain D could be transformed by an invertible analytic map into the study of another iteration on the disc. Since the original domain D may well be mapped by ϕ onto a multiply connected region, Julia explained how careful consideration of the Riemann surfaces involved could disentangle the matter.
5. Schwarz's lemma, that if f is an analytic map of the open unit disc to itself with $f(0) = 0$, then $|f(z)| < |z|$ unless $f(z)$ is the linear function $f(z) = ze^{i\theta}$, in which case $|f(z)| = |z|$. Julia quickly summarised the proof and observed that,

⁹⁶This problem became known as the centre problem and required techniques from number theory for its further treatment.

the linear case aside, the lemma implied that $|f'(0)| < 1$ and so $z = 0$ was a fixed point of the map f , and indeed it was the only one in the unit disc. Then he showed how the theorem could be generalised; in view of the previous remarks Schwarz's lemma applies to any simply connected domain. We add that Julia used Carathéodory's work on the extended Riemann mapping theorem to show that in some cases the Julia set was a simple Jordan curve.

6. Finally, facts about algebraic functions that enabled one to define analytic branches of the inverse function, ϕ^{-1} .

8.7.2 Fatou

Fatou did not send in an entry to the prize competition, although the note by him in 1906 was among the most important papers written on the subject before the prize competition was announced in 1915. This surprising fact is studied in detail in Michèle Audin's book (Audin 2009) and we shall not discuss it here. Instead, we proceed to Fatou's account, spread over three papers in the *Bulletin de la Société mathématique de France* for 1919 and 1920 (Fatou 1919). When he wrote these papers, he said, Julia's prize-winning memoir was not published and he knew only those results that had been published in some brief notes in 1917.

Chapter I of his paper, said Fatou, was devoted to an algebraic study of the fixed points of the iteration of a function ϕ , those that is, for which $z = \phi(z)$.⁹⁷ This led him to consider the possible values of ϕ' at a fixed point, the multipliers as he called them. Fatou showed that there was always at least one fixed point where the absolute value of a multiplier was greater than or equal to 1. He was pleased to note that he had given an elementary proof that did not use the theory of normal families. Chapter II studied the case where the multipliers were either 1 or a p th root of unity. Chapter III gave an elementary exposition of iterations that map a circle to itself. By a simple inversion this is the same as studying rational maps that map the real axis to itself.

Chapter IV of the account, which begins the second part of the paper, brings in Montel's theory of normal families. The family of iterates of a rational function may give rise to normal families on being restricted to certain domains. This happens, for example, when there is a fixed point (of ϕ or of some iterate of ϕ with a multiplier less than 1 in absolute value or a multiplier of the form $e^{2k\pi i/m}$). The set of points on which the functions ϕ_n form a normal family is completely invariant (meaning that it is mapped to itself under forward and backward iteration of ϕ) and can be composed of finitely or even infinitely many disjoint domains. This set became known as the Fatou set; Beardon suggests this happened as late as 1984.⁹⁸ Fatou

⁹⁷Fatou denoted the function R .

⁹⁸See (Beardon 1991, 50), the reference is to (Blanchard 1984).

then drew attention to the set of points at which the family of iterates is not normal, which he denoted $\mathbb{F}$ (the Julia set, as it is called today).

As Julia had done, Fatou showed that $\mathbb{F}$ is perfect and that if it contains its interior points it must be the whole complex plane. He showed that Lattès' example gave rise to a function for which $\mathbb{F}$ was indeed the whole complex plane. This extreme case aside, the set $\mathbb{F}$ was either everywhere discontinuous or else a one-dimensional continuum, which could be a Jordan curve or even a line segment. Fatou investigated further and showed that $\mathbb{F}$ is the limit of the periodic points, it divides the complex plane into either 1, 2, or infinitely many regions. Finally he concluded that $\mathbb{F}$ is the closure of the periodic repelling points. Chapter V was devoted to the domains of convergence of attractive or indifferent fixed points. Such domains, he showed, are either simply or infinitely connected, but if the domain for one such point is infinitely connected, then the domains for all such points are also infinitely connected.

Fatou was able to illustrate his results with a number of examples. In each case the analysis began by locating the fixed points, determining if they are attractive, indifferent, or repelling, and then considering the behaviour of ϕ_{-1} . His first was $\phi(z) = az + z^2$, with a real and between 1 and 2. In this case $\mathbb{F}$ is a single continuum with infinitely many double points where Jordan curves come together. His second example was $\phi(z) = \frac{z}{2+z+z^3}$, where $\mathbb{F}$ was everywhere disconnected. The third example, $\phi(z) = \frac{z^4+1}{4\sqrt{2}} + z$, shows a set $\mathbb{F}$ with infinitely many distinct continua. We pass over other examples to come to the sixth, obtained by applying Newton's method to the equation $z^3 - 1 = 0$, that is, to the function $\phi(z) = \frac{2z^3+1}{3z^2}$. Fatou did the best he could to describe the resulting picture, but later generations of computer images are much more eloquent. In the seventh example, $\phi(z) = Kz - \frac{1}{z}$, different things happen according as K is real and positive, real and negative, or imaginary. In the third case, one possibility is that the set $\mathbb{F}$ is a Jordan curve having a dense set of points (the antecedents of $z = \infty$) where there is no tangent.

Chapter VI used the theories of normal families and conformal maps to elucidate the nature of $\mathbb{F}$ when it is a non-analytic curve. He showed that the nature of the curve is affected by the behaviour of the attractive fixed points and that the curve is either differentiable nowhere or at most a countable infinity of points. It follows, he observed, by a theorem of Lebesgue that the curve is not rectifiable, and he noted that his curves had many analogies with those constructed by von Koch. However, said Fatou, von Koch had constructed his curves to have certain properties specified in advance whereas here, on the contrary, "it was necessary, in order to analyse the mechanism by which these curves are constructed, to carry out long and patient research chiefly involving a profound analytic study of certain functional equations" (Fatou 1920, 242).

Chapter VII returned to the topic that was the source of the investigations by Fatou and Julia, the functional equations of Schröder and Abel, and discussed their solutions.

8.7.3 A Third Man

The judging commission of the Grand Prix, although it considered only the work of Lattès' and Julia, reported that three memoirs had been submitted. Who was the third applicant? The silence of the report is intriguing, all the more so because the third man was Pincherle, who had recently devoted about a quarter of his long paper on functional equations in the German *Encyklopädie* to the iteration of complex functions (see [Pincherle 1906](#)).⁹⁹ The paper had also appeared in the French edition of the *Encyklopädie* in 1912. Indeed, in the early 1890s Pincherle had begun to elaborate a wide research programme on the general theory of functional, linear (distributive, in his terminology) operations that he expounded in the memoir *Sur le calcul fonctionnel distributif* that appeared in 1897 in the *Mathematische Annalen* ([Pincherle 1897](#)).

Under the term “functional calculus” he included “the chapters of analysis where the variable element is not a number any more but the function as such”. Pincherle’s aim was the study of distributive operations that can be applied to analytic functions (and in particular that transform power series into power series). He considered the set of all power series as a functional space of (denumerably) infinitely many dimensions, where any series can be considered as a point whose coordinates are the coefficients of the series. In this geometrical vein he introduced the concept of an n -dimensional, linear vector space generated by the linear combinations of n linearly independent functions $\alpha_1, \alpha_2, \dots, \alpha_n$. Having established the main properties of distributive operations, he defined the concept of functional derivation, established an analogy to Taylor expansions for functional operations, and in the concluding part applied this to the integration of linear differential equations. He gave an expository presentation of his results in the book (1901), where he introduced linear spaces in axiomatic terms. This pioneering work made Pincherle one of the founders of functional analysis, and explains why he was best suited to contribute the German *Encyklopädie* with a paper on functional operations. As usual with the *Encyklopädie*’s articles, this paper provides a survey of the topic rich of historical references. As for iteration, ([Pincherle 1906](#)) began with Babbage’s equations and followed the historical developments of the subject up to Schröder’s and Koenigs’ most recent results.

Pincherle’s original research on iteration dated back to 1914, and during the First World War he presented several papers to the Accademia delle scienze in Bologna on the iteration of $\alpha(x) = x^2 - a$ (with x complex, and $a > 0$ a real parameter). This was the subject of the memoir entitled “Sur l’itération de la substitution $x^2 - a$ ” that he submitted for the *Grand Prix*. As [Audin \(2009, 78\)](#) has aptly remarked, this is the quadratic polynomial that later on in Mandelbrot’s hands will produce his eponymous set. Pincherle was able to determine topological and geometrical properties of the set Ω of points x that are divergent under the iteration of $\alpha(x)$ and

⁹⁹As [Alexander \(1994, 114\)](#) has also noted.

also of the boundary of this set. More precisely, he presented a detailed study of the corresponding Julia set (of course without naming it this way) according to whether $a < 2$, $a = 2$ or $a > 2$. Once the prize competition was settled, and Julia had been awarded the *Grand Prix*, Pincherle presented his results in the papers (Pincherle 1918, 1920a) and, in an extended version, (Pincherle 1920b). We agree with Audin (2009, 79) that it would be unfair to state that Pincherle's work consisted only some examples, but he himself recognised that he had studied only a particular case, and indeed Pincherle did not list his work on iteration among his major achievements in (Pincherle 1925, 61). In fact, compared with Julia's and Fatou's, Pincherle's results seem to be of limited interest, and they were likely judged as such by the members of the commission of the *Grand Prix*.

8.7.4 The Role of Complex Function Theory

Both Fatou and Julia made considerable use of Montel's theory of normal families. They used it to show that the Julia set could not be the domain of a normal family of iterates of a given function ϕ , and they used it to study the behaviour of a normal family of iterates restricted to part or all of the Fatou set.

That the Julia set is a perfect set was also crucial to the theory, because it allowed Fatou and Julia to prove that every point of the set is a limit-point of its own antecedents. Julia gave two proofs. The first was a mixture of Montel-type arguments and point-set topology, the second relied on Picard's theorem "with an improvement due to Landau" (Julia 1918, 169). This is the result, in (Landau 1906), that if f is a function of the form $f(z) = \alpha + \beta z + \dots$ where α and $\beta \neq 0$ are complex numbers then there is a number R depending only on α and β such that in the disc of radius R the function takes either the value 0 or 1. We do not wish to discuss how the theorem was used, but to note that Julia commented that "This second proof is not essentially different from the first, especially when one realises that M. Montel has deduced the Picard–Landau theorem from notions which he introduced for normal families of functions". Fatou also made a similar observation before settling on Montel's methods exclusively (Fatou 1919, 163).

Because the Julia set is formed from the repelling fixed points, it plays the role of a source in the dynamics of the flow defined by the iteration, so it was natural for both men to investigate the backward iterates of any given point z , that is, the set $\Phi_{-1}(z) = \{\phi_{-n}(z) : n = 1, 2, \dots\}$. The key insight here was Montel's theorem that a family is normal if it omits more than two values, so a family that is not normal can omit at most two values. Whence the theorem that the Julia set is contained the closure of $\Phi_{-1}(z)$ for all but at most two values of z . This result is the key to modern-day computerised depictions of Julia sets, and it was also the key for Julia and Fatou's accounts.

Fatou and Julia also drew attention to the fact that the Julia set has the property of self-similarity, as mathematicians say today. Specifically, Julia wrote that "One can therefore say that *starting from any arbitrarily small part E'' of E' one can generate*

all of E' by means of a finite number of iterations of E'' , this will be useful in discussing the structure of E' because one can say, as a consequence of this remark: *The structure of all of E' is the same as that of any part E'' of E' formed from the points of E' inside an arbitrarily small area in the plane that contains an arbitrary point of E' .*” (Julia 1968, 173, emphasis in original). Fatou wrote that “The set $\mathbb{F}$ has the same structure in all its parts”.¹⁰⁰

Iteration restricted to the Fatou set concerns the forward iterates of points. Both men now used Montel’s theory to show that if there is an attractive fixed point x in the Fatou set then all points in the connected component of x are attracted to x . They also showed that the number of attractive and indifferent points is finite. This does not imply that the number of connected components of a Fatou domain is finite, and both men showed that it could be infinite. Indeed, the number of these domains is 1, 2, or ∞ . Fatou then considered the behaviour of these domains under iteration, and found, as Julia had, that the most obscure part of the problem concerned fixed points whose multiplier was of the form $e^{i\alpha}$ where α was an irrational multiple of π . It was clear that nearby points were not attracted to the fixed point under the iteration, and he asked: “Can there exist domains whose consequents tend towards the fixed point? At the present time we can neither give an example nor prove that it is impossible” (Fatou 1919, 221). This is the origin of the question if there could be a component that was never mapped back to itself. In symbols, could there be a component Ω such that the sequence $\phi_n(\Omega)$, $n = 1, 2, \dots$ is a sequence of disjoint sets for all n . Fatou was unable to answer this question, which remained open until 1985 when it was solved in the negative by Sullivan: there are no wandering domains for a rational function. His answer made essential use of the idea of quasi-conformal transformations, a development that lies outside the period covered by this book.

These papers represent a tour de force of contemporary complex function theory, but their reception poses a puzzle for the historian. For as is well known and documented in every good account of iteration theory, there was essentially no response until the 1980s, when the increasing availability of computers made it possible to bring geometrical intuition to bear on the subject. The remarkable pictures that can be generated with a little skill suggested new theorems which the combined efforts of several leading mathematicians have steadily been able to prove.¹⁰¹ The fact remains that even Fatou and Julia themselves turned to other subjects. For their generation at least what they wrote was the last word.

By the start of the twentieth century some general trends had become noticeable in the distinctive and largely French and Scandinavian viewpoint on complex function theory. There was a vigorous debate about the nature of analytic continuation. A successful theory of entire functions was created, based on the concept of order, which established characteristic properties of complex functions and provoked a much less successful search for a theory of meromorphic functions that would match the theory of entire functions. Proofs and generalisations of Picard’s theorems were

¹⁰⁰Fatou (1920, 40), quoted in (Alexander 1994, 132).

¹⁰¹See Beardon (1991) for a good starting point and overview.

found in various overlapping fields: elementary ones due to Borel, Schottky, and, finally, Bloch; and by using Montel's simple but fertile idea of normal families. On the other hand, conformal mappings and ideas of Riemann surfaces were not much in evidence, especially among the French.¹⁰² This French influence was strong enough, despite the contributions of Schottky, Landau, Bieberbach and Ostrowski, to influence even the choice of language in which later papers tended to be written.

¹⁰²Geometry could not be kept away indefinitely, as Ahlfors would demonstrate in the 1930s.

Chapter 9

Several Complex Variables

When I was a student, Abelian functions—as a consequence of the Jacobian tradition—stood as the undisputed summit of mathematics, and every one of us had the self-evident ambition to go further in that direction themselves. And now? The younger generation scarcely know Abelian functions any more.

Klein

9.1 Introduction: The Challenge Posed by Several Variables

Throughout the nineteenth century and well into the twentieth the study of complex functions of several variables posed a challenge to the experts in the function theory of a single variable. As we have seen in Chap. 6, the prospect of creating a theory of Abelian functions was one that Weierstrass continually had in mind; it was the ultimate goal of all his work. And yet a marked distinction between the theories of one and several variables persists to the present day. Almost all universities offer a mainstream course in single variable complex function theory; few, if any, present the theory of several variables as other than a specialist option. We shall see that this distinction is in the nature of the functions studied. Because this dichotomy survives in the modern syllabus, we have divided this chapter into three sections. The first is a survey of the claim that the theories of one and several variables diverge markedly. We give an indication of what was discovered about the complex function theory of several variables, but generally slight the proofs so that the section can be read by a broad audience. The second section looks at the history of the principal results about Abelian functions and theta functions which, for a long time, were the only examples known of complex functions of several variables. In the final section we reconsider the general theory, look at some of the techniques used, and seek sharpen the discussion of the opening section. The latter sections naturally place greater demands on the reader.

9.2 The General Theory

At every point the theory of several variables offers a sharp contrast to the theory of just one.¹ Throughout the nineteenth century, was there a store of useful accessible examples, other than polynomial and rational functions, to guide the development of a general theory? For one variable, yes—the theory of elliptic functions. For several variables, there was only the much less penetrable theory of Abelian functions. In the case of one variable, the Cauchy–Riemann equations establish an intimate connection between analytic functions and harmonic ones. In the case of many variables the corresponding equations do exist and were to prove to be a very powerful tool, but throughout the period considered in this chapter their meaning was much less clear. Is there a simple theory of the local behaviour of a function? For a single variable the zeros form an isolated set; it was easy to see by example that the naive generalisation of this result to several variables was false, but the correct generalisation was harder to find. For a single variable the concept of a singular point is powerful, and the distinction between finite poles and essential singularities profound. Even as late as 1900 much less could be said in the case of several variables. For a single variable the Riemann mapping theorem completely solves the question of what sort of simply connected domain can be the domain of a function; no such result was to be found for several variables. For a single variable there is the remarkable theorem of Cauchy concerning integrals and residues; for several variables there is a formal analogue known in its essentials to Cauchy himself, but, for example, in the two-variable case² the formula for a region of the form $D_1 \times D_2$, where D_1 and D_2 are discs in $\mathbb{C}$ is

$$F(z_1, z_2) = \frac{1}{(2\pi i)^2} \int_{\delta_1} \frac{dt_1}{t_1 - z_1} \int_{\delta_2} \frac{F(t_1, t_2) dt_2}{t_2 - z_2},$$

where δ_1 and δ_2 are the positively oriented boundaries of D_1 and D_2 , respectively. But this is a double integral over a surface, not the three-dimensional boundary of $D_1 \times D_2$ (and the situation is worse in higher dimensions).³ Poincaré and Picard in the 1880s did find results about the integral over the three-dimensional real boundary of a real four-dimensional region, but they avoided the fundamental obstacle that, as

¹For a clear introduction to the differences, see [Krantz \(1987\)](#) or [Krantz \(1990\)](#) and the opening chapter of [Krantz \(1982, 2nd ed. 1992\)](#).

²We have taken this example from ([Osgood 1914](#)) which has generally been a very helpful guide to the material in this chapter.

³There were investigations of multiple complex integrals by Maximilien Marie in the 1850s, but these seem to have been rather formal, superficial, and occasionally wrong as shown in [Poincaré \(1887a, 440–441\)](#). In the end they were without much influence, perhaps because, to quote ([Coolidge 1924, 77](#)) Marie “possessed the knack of quarrelling with his contemporaries almost to the point of genius”.

we shall see, derives from geometric problems to do with the polar set of a complex function and makes it difficult to produce a theory of residues.

Only the theory of power series and their convergence seemed as amenable in one variable as in several, but to any but the most algebraic of temperaments it was a half-blind creature when all geometric insight was lacking. Nonetheless, the merits of the power series approach commended it to Weierstrass, who, as we have seen, often treated the theory of power series in one or more variables on a par. His first great success having been with the theory of hyperelliptic integrals, Weierstrass chose the power series approach because it offered him the best way to create a theory of Abelian functions, which always marked the climax (although not the end) of his 2-year lecture cycle. The availability of the method of power series when all others failed surely also contributed to the growing emphasis Weierstrass placed on algebra.⁴

It might seem that the theory of several variables would develop as the single variable theory had, with the theory of Abelian functions advancing, on the one hand, and the general theory emerging, on the other, until the two converged. Such a picture would not be entirely false, but it is too naive in various ways. The single-variable theory always existed to divert research into better-dug channels, and the subject of several variables itself was not to permit a Cauchy. Instead there was to be a series of piecemeal developments, crudely divisible into two phases. The first phase around the 1880s sees the achievements of Weierstrass and Poincaré. A second phase began around the turn of the century and is associated with the names of Cousin, Hartogs, Levi, and once again Poincaré.

9.2.1 *Definitions of An Analytic Function*

To begin with, even the definition of an analytic function of several variables was problematic. For most French mathematicians, a function $F(z_1, z_2, \dots, z_n)$ of n complex variables was analytic at a point if and only if it was analytic in each variable separately at that point, and it was analytic on a domain if it was analytic at each point of the domain. This was the approach first proposed by Cauchy in his Turin memoir of 1831 and again, in the more precise form just quoted, by Jordan in 1893. On the other hand, Weierstrass preferred to define a function to be analytic if it can be given a uniformly convergent Taylor series development in some neighbourhood of any ordinary point, and Poincaré used this definition in his doctoral thesis (1879). The question arises as to the equivalence of these definitions. The Weierstrass condition implies the Cauchy condition by standard results about power series. In 1899 Osgood showed that a function of two complex

⁴So it is particularly interesting to note Krantz's comment (Krantz 1982, 16) that, by comparison with their comparatively modest role in the single-variable theory, when it comes to constructing functions in several complex variables the Cauchy–Riemann equations are central.

variables, defined on a domain $D_1 \times D_2$, which is bounded and analytic in each variable separately is analytic in the Weierstrassian sense throughout the domain. Then in 1905 Hartogs removed the condition that the function be bounded, thus establishing that a function of several complex variables which is analytic in the sense of Cauchy and Jordan is analytic in the sense of Weierstrass and with that the equivalence of the two definitions. Bieberbach made the interesting comment (1921a, 518) that Hartogs' result remained unknown to several people working on this question, including Levi.

It follows from either definition that there are analogues of the Cauchy–Riemann equations, or, rather, their second order implications. If $z_r = x_r + iy_r$, and $F = u + iv$, then these equations hold for the real part u (and similar ones for the imaginary part v):

$$\frac{\partial^2 u}{\partial x_r^2} + \frac{\partial^2 u}{\partial y_r^2} = 0, \quad r = 1, 2, \dots, n, \quad (9.1)$$

$$\frac{\partial^2 u}{\partial x_r \partial x_s} + \frac{\partial^2 u}{\partial y_r \partial y_s} = 0, \quad (9.2)$$

$$\frac{\partial^2 u}{\partial x_r \partial y_s} = \frac{\partial^2 u}{\partial y_r \partial x_s}. \quad (9.3)$$

These equations seem to have been written down for the first time by Poincaré in his (1883e) for the case of two variables. One might have expected Weierstrass to have done so, but as we saw in Chap. 6, he was unwilling to base even the theory of a single variable on such foundations. These equations make it plain that there can be no simple relationship between harmonic functions and holomorphic functions of several complex variables, for there will be a unique harmonic function of two or more variables that satisfies some given boundary conditions that is not the real or imaginary part of a holomorphic function because it does not satisfy all the above equations. This is just one mathematical reason why generalising from one variable was to prove difficult: Poincaré (1883f, 102) said that this provided the true explanation of the differences between complex functions of one complex variable and functions of two, and in particular why one could not construct an analytic function of two variables having four arbitrary periods.

From a historical standpoint, it is worth observing that not much of an elementary theory could have been built up from a theory of real functions of several variables, since for most of the nineteenth century there simply was no such theory; it too was in the process of being uncovered. Consider the claim that an equation $F(u, z_1, z_2, \dots, z_n) = 0$ defines a function $u = G(z_1, z_2, \dots, z_n)$, at least on some domain. Conditions that permit one to define a function G are described today in the implicit function theorem. They were known to Cauchy, but rigorous proofs seem to be due to Poincaré and Neumann independently. The late date for the proofs, and the comparative neglect of Cauchy's work for some years after his death, means that there was no impact from any theory of real functions on the development of a theory of complex functions of several variables.

9.2.2 The Zero Set

Weierstrass did, however, prove an important theorem about any function $F(u; z_1, z_2, \dots, z_n)$ that vanishes at the point $(0; 0, \dots, 0)$ of its domain, but for which $F(u; 0, 0, \dots, 0)$ is not identically zero. This result was the first significant breakthrough in the theory of several complex variables, and it gave a valuable insight into the nature of their zero sets. He claimed to presented it in lectures from 1860, and it was circulated in lithographed notes from 1879 and in printed form as (Weierstrass 1886). The theorem states that there is a neighbourhood N of the origin, given by the inequalities $|u| < h$, $|z_r| < h'$, $r = 1, 2, \dots, n$, within which the following factorisation theorem holds:

$$F(u; z_1, z_2, \dots, z_n) = (u^m + A_1 u^{m-1} + \dots + A_m) G(u; z_1, z_2, \dots, z_n), \quad (9.4)$$

where the functions A_r are analytic in $z_1, z_2, \dots, z_n$ throughout the region given by $|z_r| < h'$, $r = 1, 2, \dots, n$, and all vanish at $(0, 0, \dots, 0)$, and the function G , which is uniquely determined by F , is analytic in N and never vanishes there. So the zero set can be described by a polynomial in u with coefficients that are analytic functions in the z 's. Moreover, any function that does not vanish identically in a neighbourhood of the origin can be reduced to one of the above form by a linear change of the u - and z -variables. For this reason the theorem is sometimes called the Weierstrass preparation theorem, which was Weierstrass's name for it, as well as the Weierstrass factorisation theorem (see Sect. 6.8.3).

This theorem generalises the result that an analytic function $f(z)$ of one variable can always be written uniquely in the form $f(z) = (z - z_0)^n g(z)$, $g(z_0) \neq 0$, which shows that the zeros of the function f form a discrete set. In the case of two variables, u and z , the theorem asserts that $F(u, z) = p(u, z) g(u, z)$, where $p(u, z)$ is a polynomial in u with coefficients that are analytic functions in z that vanish at $z = 0$, and $g(u, z)$ is non-zero in some neighbourhood of the origin.⁵

Geometrically interpreted, the Weierstrass preparation theorem says that the zeros of a function of several variables form continuously varying branches described by the zeros of $p(u, z)$ for each fixed value of z in N . Weierstrass, however, preferred to offer an analogy with the theory of the factorisation of polynomials. It is no longer true that the roots are isolated, but the factor of the function F that does vanish at the origin has roots which are single-valued functions except where the factor has multiple roots. Weierstrass also gave a parametric representation of the roots of an analytic function $F(z_1, z_2, \dots, z_n)$ that vanishes (but not identically) at the point $(0, \dots, 0)$. Then there are, he claimed, n functions $z_i = \varphi_i(t_1, t_2, \dots, t_{n-1})$, $i = 1, 2, \dots, n$, analytic and vanishing at the t -origin, such that on a neighbourhood of the t -origin, the points $(z_1, z_2, \dots, z_n)$ are roots of F . Because of the restriction

⁵The function $p(u, z)$ was called an algebroid function, by Poincaré in his then-unpublished thesis (1879, lii).

to a neighbourhood, the theorem is often referred to as a representation *im Kleinen*, meaning “in the small”, or, as we would say, locally. Weierstrass did not give a proof of this claim, which was only established later.

9.2.3 The Singular Set

These theorems of Weierstrass gave the first indications of how the zeros of an analytic function of several variables could behave. The geometrical interpretation of the behaviour of the zero set is clearly tricky to elucidate. However, the corresponding question about the set of singular points of an analytic function of several variables is much harder, and even Weierstrass could do little with it.⁶ The finite or non-essential singularities of a rational function are of two types, of which only the first resembles the single variable case. It might be that the function is of the form $F(\mathbf{z}) = \frac{f(\mathbf{z})}{g(\mathbf{z})}$, where $\mathbf{z} = (z_1, \dots, z_n)$, f and g have no common factors and $g(\mathbf{z})$ vanishes at $\mathbf{z}_0$ while $f(\mathbf{z})$ does not; Weierstrass called this a non-essential singular point of the first kind. It resembles the case of a pole of a function of a single variable, and Osgood, following Weierstrass in his account of the preparation theorem, called such singularities poles (Osgood 1914, 51). The poles of the function $F(\mathbf{z})$ are not isolated, however, but lie on the real analytic variety of dimension $2n - 2$ defined by the equation $g(\mathbf{z}) = 0$.

The second kind of singularity occurs when both $f(\mathbf{z})$ and $g(\mathbf{z})$ vanish at $\mathbf{z}_0$, as is the case, for example, with the function $\frac{x}{y}$ of the two variables x and y at the point $(0, 0)$. A function with singular points of the second kind, written in the form $F(\mathbf{z}) = \frac{f(\mathbf{z})}{g(\mathbf{z})}$, will take any pre-assigned value on a $2n - 2$ dimensional variety in the neighbourhood of the singular point: to take the value C it is enough to solve the equation $f(\mathbf{z}) = Cg(\mathbf{z})$ in a region where $g(\mathbf{z}) \neq 0$. So these singular points have more in common with the essential singular points of functions of a single variable. Such a function may be written, perhaps after a suitable linear change of variable⁷ in the form $F(\mathbf{z}) = \frac{p(z_n)}{q(z_n)}w(\mathbf{z})$, where p and q are relatively prime monic polynomials in z_n whose coefficients are analytic functions in $(z_1, z_2, \dots, z_{n-1})$ vanishing at $(0, \dots, 0)$ and where $w(\mathbf{z})$ is an analytic function that does not vanish at the origin. So these singular points lie on a variety of real dimension $2n - 4$ through the singular point.

Finally, a function whose only singularities in a given region, or in a neighbourhood of a given point are poles or non-essential singularities of the second kind was said to be meromorphic in that region or neighbourhood. So a meromorphic function is locally a rational function, in the sense that it can be written in a neighbourhood

⁶This account follows (Osgood 1914, 50–53).

⁷This takes care of the examples such as $\frac{x}{y}$ above.

of each of its points as a quotient of two analytic functions. This raised the question of whether it can be written everywhere as a quotient, which will be discussed below.

9.2.3.1 Essential Singularities

It was the nature of the essential singularities that caused the deepest problems. A function of a single variable may either have an isolated singular point, meaning that it is analytic at every point of a neighbourhood of the essential singular point (except, of course, for the point itself); or else the singular point is an accumulation point of a set of singular points and the function possess infinitely many poles or essential singularities in any neighbourhood of the singular point. The latter case can occur along the natural boundary of the function. In a rare lapse, [Weierstrass \(1880a\)](#) seems to have implied that functions of several variables may also have isolated essential singular points. More precisely (1880a, 129), he claimed that there are functions, defined on some domain in complex n -space, that are meromorphic at every point of the domain but at no point on the boundary of the domain, and “Therefore”, he continued, “the essential singular points of a single-valued function of r variables are not necessarily isolated, but rather any possible figure (*Gebilde*) in the domain (*Gebiete*) of r complex variables can be the locus of such points”.

This statement turned out to be erroneous in two ways. First, the claim that an essential singular point of a meromorphic function can be isolated was shown to be incorrect by [Osgood \(1901, § 42\)](#) by a simple argument using the Cauchy integral theorem. More importantly, in 1910 the Italian mathematician E.E. Levi⁸ showed that some bounded domains are not domains of holomorphy, and under certain modest extra conditions on the boundary a meromorphic function on a bounded domain can be continued analytically to points on the boundary. That Weierstrass’s error persisted from its promulgation in 1880 to its correction by [Levi in 1910](#) shows how difficult this branch of the subject was from the start. Even the simple observation that an analytic function of several variables cannot have an ordinary singularity at an isolated point, which follows from a consideration of its generalised Laurent series expansion, seems to have been made for the first time by [Hurwitz](#) in his (1897, 105).⁹ So the first phase of the complex function theory of several variables proceeded without an adequate grasp of the possible singularities.

⁸One of the most gifted Italian mathematicians of his generation, in his short life—he died in 1917 at the front during WWI—in addition to function theory Levi gave substantial contributions to various fields of mathematics, including the theory of partial differential equations and the calculus of variations. See Picone’s introduction in (Levi, *Opere* 1, V–XV).

⁹The generalised Laurent theorem is explained in (Osgood 1924, 68–70).

9.2.4 Quotients

It was of increasing importance in the later nineteenth century to exploit the recognition of the fact that an analytic function of a single variable defined everywhere and having no essential singularities (except perhaps at infinity) was what was called a rational function (i.e. a quotient of two entire functions). A further claim that [Weierstrass](#) made in his (1879) was that a function of several variables that is meromorphic everywhere including infinity is also rational. It is striking that the cautious Weierstrass would go into print with unproven claims. One explanation may have been a desire to reach beyond Berlin to find any one capable of taking up his ideas. It is noticeable that none of the many students that embody the Berlin school seem to have responded: Frobenius, Fuchs, Kovalevskaya, Mittag-Leffler, Schwarz all preferred other tasks. This further emphasises the real difficulties that were felt to obstruct the creation of the otherwise much-needed theory.

This result, however, that a function meromorphic everywhere is a quotient, is not elementary. It does not follow from the Weierstrass preparation theorem, which indeed gives such a decomposition locally. For, as Hartogs pointed out in his (1906), the decomposition afforded by that theorem was far from being unique, so one could not easily patch the locally defined expressions together to get a globally defined function. Nonetheless, it was speedily proved by Hurwitz and Poincaré independently. Hurwitz's argument ([Hurwitz 1883](#)) used induction on the number of variables.

[Poincaré](#), in the paper (1883f) that consolidates his first involvement with the complex function theory of several variables, exploited what analogy he could find with the theory of harmonic functions. He began by writing down the partial differential equations for the real part of an analytic function of two complex variables [Eqs. (9.1)–(9.3) above]. He then covered the plane $\mathbb{C}^2$ by “hyperspheres” (open balls of 4 real dimensions) inside any one of which the given meromorphic function, F , was representable as a quotient of two analytic functions $\frac{N_j}{D_j}$. For each ball he found, by a Poisson integral argument, a function that tended to $\log|D_j|$ on the boundary of the hypersphere and was harmonic inside it, and another that was harmonic outside the ball, tended to $-\log|D_j|$ on the boundary of the hypersphere, and tended to zero at infinity. These functions did not define a function continuous on the hypersphere, but by looking at them more carefully and taking into account the limiting values of $\frac{d|D_j|}{dr}$ as $r \rightarrow 1$, he found he was able to define a harmonic function J_j^0 such that, if at an arbitrary point $F = \frac{N}{D}$ then $\Phi - \log|D|$ was real analytic. This function J_j^0 had these properties: outside the ball, it tends to zero at infinity; inside the ball at an arbitrary point, where $F = \frac{N_j}{D_j}$, the difference $J_j^0 - \log|D_j|$ is analytic. This required a separate argument and an appeal to a theorem of [Kovalevskaya](#)'s (1875) on the uniqueness of solutions to partial differential equations.

Next he gave a separate argument to show how these functions behaved on the overlap of two hyperspheres. He called the resulting functions J_j^1 . Then he

constructed a harmonic function Φ out of the various J_j^1 on the whole of $\mathbb{C}^2$ with the property that the function $\Phi - \log |D_j|$ was real analytic, by following Weierstrass's proof of Mittag-Leffler's theorem. Although the function Φ was not a complex analytic function, he claimed that one could always find an entire harmonic function G satisfying Eqs. (9.1)–(9.3) and such that the difference $\Phi - G$ was the real part of an analytic function Ψ of two variables. Accordingly, the functions G_1 and G_2 defined by the equations

$$e^\Psi = G_1 \text{ and } Fe^\Psi = G_2$$

were entire, and the function F was their quotient everywhere, $F = \frac{G_2}{G_1}$.

It seems that his work did not satisfy Weierstrass, who, without mentioning Poincaré by name, noted in his (1886, 137) that the question was unresolved and some considerable difficulties seemed to lie in the path of a solution.

9.3 The Theory of Abelian Functions

In the first half of the nineteenth century, the theory of elliptic functions had proved an important source of ideas and theorems for mathematicians seeking to create a theory of complex functions. However, insights into the theory of Abelian functions were harder to find, and their theory did not shed much light on the general theory of complex functions of several variables. Rather, the reverse was the case: the first proofs of significant results in the theory of Abelian functions were actually based on more general theorems.

Their investigations into the theory of Abelian functions in the second half of the nineteenth century had led mathematicians to consider p -tuples of complex functions of p complex variables with certain periodicity properties. Given a function f of p variables, the p -tuple of numbers $(a_1, \dots, a_p)$ is called a period of the function if $f(u_1 + a_1, \dots, u_p + a_p) = f(u_1, \dots, u_p)$. The periods form a $2p$ -dimensional lattice of periods, a basis for which may be simplified by taking suitable linear combinations to form a $p \times 2p$ matrix of the form

$$\begin{pmatrix} \pi i & 0 & \dots & 0 & a_{11} & a_{12} & \dots & a_{1p} \\ 0 & \pi i & \dots & 0 & a_{21} & a_{22} & \dots & a_{2p} \\ \dots & \dots & \dots & \dots & \dots & \dots & \dots & \dots \\ 0 & 0 & \dots & \pi i & a_{p1} & a_{p2} & \dots & a_{pp} \end{pmatrix}.$$

This argument suggests that the periods depend on p^2 complex constants, the a_{ij} . However, Riemann claimed that every $2p$ -fold periodic function can be expressed as a quotient of theta functions of p arguments. These depend, by the definition of a theta function of p variables, on $\frac{1}{2}p(p+1)$ complex constants, so it follows that the periods cannot be written down arbitrarily when $p > 1$. Riemann visited Paris in 1860, where he told Hermite of this result, and Hermite included it as part of the

additional material he was writing in [Hermite \(1862, 153\)](#). However, Riemann never published a proof of his claim.

The fundamental theorems, beliefs, or conjectures in this area that go back to Riemann and Weierstrass were expressed by Krazer and Wirtinger in ([1920, 824–827](#)) as follows:

1. Given an analytic function f of p variables that has $2p$ periods and no essential singularity in its fundamental domain, and that cannot be written as an analytic function in fewer variables, there is at least one algebraic curve of genus $q \geq p$ on which there are p linearly independent integrals of the first kind whose periods are linear functions of the given periods with integer coefficients.
2. Furthermore: the periods themselves must satisfy two conditions, which can be expressed in slightly more modern language this way: (a) for any $p \times 2p$ matrix W whose columns generate the periods there is a $2p \times 2p$ matrix non-singular skew-symmetric matrix C with integer entries such that $WCW^T = 0$ (where W^T denotes the transpose of W) and (b) if w is a row of W then moreover $\text{Rew}C \text{Im}w > 0$.
3. Given p algebraically independent $2p$ -periodic functions of this kind,

$$f_1(u_1, \dots, u_p), \dots, f_p(u_1, \dots, u_p),$$

and a set of values $c_1, \dots, c_p$, chosen to avoid the singular points of the functions $f_1, \dots, f_p$ and also a certain $2p - 2$ -dimensional submanifold of the domain, the equations

$$f_1(u_1, \dots, u_p) = c_1, \dots, f_p(u_1, \dots, u_p) = c_p$$

have only a finite number, m , of solutions modulo the periods and this number does not depend on the values $c_1, \dots, c_p$.

4. If, moreover, $f_{p+1}(u_1, \dots, u_p)$ is another function of the above kind, then there is an irreducible equation between

$$f_1(u_1, \dots, u_p), \dots, f_p(u_1, \dots, u_p)$$

and $f_{p+1}(u_1, \dots, u_p)$ of degree with respect to $f_{p+1}(u_1, \dots, u_p)$ that is either m or a divisor of m .

5. If the above degree is exactly m , then the function f itself in (1) above can be written as a rational function of

$$f_1(u_1, \dots, u_p), \dots, f_p(u_1, \dots, u_p)$$

and $f_{p+1}(u_1, \dots, u_p)$.

6. And finally: The functions $f_1(u_1, \dots, u_p), \dots, f_p(u_1, \dots, u_p)$ satisfy an algebraic addition theorem.

From the second of these results a further, and complicated, argument will show that every $2p$ -fold periodic function of p variables can be expressed as a quotient of theta functions of p arguments.

It will be helpful to call the first two of these six theorems the period matrix theorem, their important consequence the representation theorem, and the rest the algebraic dependency theorem. Weierstrass's starting point was the algebraic dependency theorem, but he did not get close to a proof. He did, however, state the claims in a rare published paper (Weierstrass 1880a).

As we shall see, despite their evident importance, these theorems were to hold out for a long time, and so their proofs were not available to suggest how general theorems about several variables might be proved. Indeed, the reverse is the case: their first proofs were actually based on more general theorems.

An overview of the publication of claims about these results will establish how important they were taken to be, before we look in more detail at various attempts at providing proofs. It is clear, for example, that neither Riemann nor Weierstrass had proofs of any of these theorems; Hermite's account in his (1862) gives no indication of how to prove the period matrix theorem. Also in 1862 Weierstrass published a statement, but not a proof, of the part of the dependency theorem: any $p + 1$ Abelian functions (with respect to the same $2p$ -dimensional lattice) are algebraically dependent. The importance of establishing that every Abelian function is a quotient of theta functions was mentioned in a widely quoted paper by Kronecker (1869), but he did not attempt a proof. In the same year Weierstrass claimed more of the dependency theorem: under the above hypotheses there are $p + 1$ Abelian functions with respect to which every other can be expressed rationally. But only in 1879 did Weierstrass claim that every Abelian function is a quotient of theta functions; a detailed but still not complete treatment was only to be published posthumously in 1903. As a result, the first proof of any of these theorems was only to appear in 1883. As Igusa justly remarks (1982, 163): "Therefore although Riemann's paper of 1857 certainly was a classic in 1883, the theory of general abelian functions was still up in the air at that time".

9.3.1 Opening Proofs

In a short paper of 1883, Poincaré and Picard outlined a proof that the algebraic dependency theorem implies the period matrix theorem which in turn implies the representation theorem. They also remarked that the most basic of these had been claimed by Weierstrass although he had never published a proof. Poincaré and Picard started from Weierstrass's theorem (1862) that, given a system of $n + 1$ functions of n variables that are $2n$ -fold periodic there is an algebraic relation $f(u_1, u_2, \dots, u_n, u_{n+1}) = 0$ between these functions, and any other function of this kind is rational in these $n + 1$ functions. They let $u_1, u_2, \dots, u_{n+1}$ be such a system, and noted that the functions $u_1, u_2, \dots, u_n$ satisfy a system of total differential equations

$$dx_j = P_{j1}du_1 + P_{j2}du_2 + \dots + P_{jn}du_n,$$

where the P_{jk} are rational functions of $u_1, u_2, \dots, u_n$.

They studied the corresponding functions $x_j, j = 1, \dots, n$ by regarding each u_j as a function of the same single variable, t . The x 's are now Abelian integrals of the first kind corresponding to the algebraic equation between t and u_{n+1} obtained from $f(u_1, \dots, u_n, u_{n+1}) = 0$. The rest of the argument was what would now be called linear algebra. The n Abelian integrals are linearly independent. Their periods are therefore a non-degenerate linear combination of the given system of $2n$ periods, and they satisfy Riemann's conditions.

In the same year, Poincaré sketched his proof that a function of two variables which is a meromorphic function whenever the variables are finite can be represented as the quotient of two convergent power series (Poincaré 1883f). These matters rested until in 1890 Appell gave a proof¹⁰ of the period matrix theorem in the special case when $n = 2$. He used Poincaré's result that a meromorphic function on $\mathbb{C}^2$ is a quotient of two entire functions, and avoided Weierstrass's unproved claims. Burkhardt, in his review of this paper (*Fortschritte* 23.0430.01), recalled Weierstrass's criticisms of Poincaré's account, and observed that a new proof of this result would be very valuable.

Appell argued that if $f(x, y)$ is a rational function in the finite part of $\mathbb{C}^2$ then, by Poincaré's result, it can be written as a quotient of two entire functions, $\varphi(x, y)$ and $\psi(x, y)$, that only vanish simultaneously at points where $f(x, y)$ is undetermined. But, he said, if this can be done in two ways, so

$$f(x, y) = \frac{\varphi(x, y)}{\psi(x, y)} = \frac{\varphi_1(x, y)}{\psi_1(x, y)},$$

then by the Weierstrass preparation theorem,

$$\varphi_1(x, y) = \varphi(x, y)G(x, y); \quad \psi_1(x, y) = \psi(x, y)G(x, y),$$

for some entire function $G(x, y)$. So, if the function $f(x, y)$ has two groups of periods a and b , one can take for $\varphi_1(x, y)$ the function $\varphi(x + a, y + b)$ and for the function $\psi_1(x, y)$ one can take $\psi(x + a, y + b)$. Simple algebra now shows that the function $G(x, y)$ has an inverse that is entire, and so $G(x, y) = e^{g(x, y)}$. It follows (after a lengthy argument here omitted) that a function of two variables with four pairs of periods and no essential singularities at a finite distance can be written as a quotient of theta functions. In his thesis of 1895, published in *Acta mathematica*, Cousin generalised Poincaré's result to any dimension and so Appell's theorem became valid for all g .

Then in his (1897) Poincaré announced detailed proofs of the algebraic dependency theorem and the claim that every Abelian function can be written as a quotient of theta functions, remarking that Weierstrass had never published a proof of this latter result. Details of the proof of the second of these appeared in his (1898a) but of the first only in his (1902a). To prove the second one, Poincaré went back to his *Acta* article (1883f), in which he showed that a meromorphic function in 2 complex

¹⁰See Appell (1890) and Appell (1891).

variables is a quotient, and modified it to establish that the entire functions forming the quotient can be taken to be theta functions. The delay may well have been caused by the greater difficulty inherent in proving the dependency theorem.

The paper of 1902 was written in answer to a request from Mittag-Leffler. In it, Poincaré proved two theorems. Theorem A, as he called it, was the algebraic dependency theorem; theorem B that every $2n$ -fold periodic function can be expressed by means of theta functions. Poincaré commented that it might seem that Riemann had shown that because Abelian functions depend on $3p - 3$ complex parameters and the corresponding theta functions depend on $\frac{1}{2}p(p + 1)$ parameters, there must be relations between the theta functions once $p > 3$. This might suggest, he went on, that Theorem B could not be true without restrictions, however, he would show that indeed every $2n$ -fold periodic function can be expressed by means of theta functions. The reason is that not every $2n$ -fold periodic function is an Abelian function. Poincaré also noted that most demonstrations of theorem B derived it from theorem A, but he was going to give an independent proof. Moreover, he was also going to give a new proof of theorem A (one that did not invoke Cousin's theorem). Instead, Poincaré gave a geometrical demonstration that a modern commentator (Igusa 1982, 163) observes is interesting but not quite satisfactory.

Theorem B had, of course, been claimed long ago by Weierstrass, but no proof had ever been forthcoming. Poincaré had seemingly grown tired of this, however, and commented tartly at the start of part 3 of his paper that although he believed that Weierstrass had given the principles of his proof in lectures, "be that as it may, the proof has never been made public and his pupils, if they knew it, have communicated it to no-one". So he and Picard had given a proof in 1883, entirely ignorant of Weierstrass's, and it only turned out much later, when Weierstrass's proof of this result appeared in 1903 in the third volume of his *Mathematische Werke* that the proofs were essentially identical. Appell, and later Picard, had by then given other proofs. This new one by Poincaré occupied a middle position between his first proof and the methods of Cousin.

These papers were picked up by Baker in Cambridge, who responded with two of his own. In his (1900) he gave a simplified version of Poincaré's proof of the theorem that a function that is meromorphic everywhere is rational. Poincaré had constructed the real part of the numerator and denominator of the rational function using the potential of the subvariety of $2n - 2$ real dimensions formed by the zeros of the given meromorphic function; Baker's proof worked simultaneously with the real and imaginary parts, and Baker expressed the answer as a $2n - 1$ -fold integral. In his (1903) Baker first extended these ideas to give an alternative derivation of the main result in Poincaré's (1902a). Then he attempted "to put a point of view which appears to open a whole series of important questions" (1903, 14). This was an exploration of the shape of domains of convergence of power series in several variables. Baker observed (1903, 30), a year before Hartogs published his much more thorough work on the associated radii of convergence to be described below, that a power series of the form $\sum a_{m,n} x^m y^n$ is understood to converge for a disc in x for fixed each value of y but the radius of this disc will generally vary with y

and may even be infinite. A consideration of the intricacies involved even when the polar sets of a function are given by linear equations led Baker to hope (1903, 36) that they threw light on the question “How far do there exist Mittag-Leffler series of simpler functions for multiperiodic functions without finite essential singularities analogous to the well known series for the elliptic function $\wp(u)$ ”. However, Baker never returned to the question.

9.3.2 *The Schottky Problem*

One problem raised by Jacobi and more forcefully by Riemann that concerns the theory of theta functions in several variables continued to tantalise mathematicians throughout the decades and is worth a brief discussion: the Schottky problem.

Riemann had shown in his paper on Abelian functions that an algebraic curve of genus $p > 1$ gives rise, via the period relations, to a $p \times p$ symmetric matrix that determines a lattice in p -dimensional complex space $\mathbb{C}^p$. The quotient of this space by the lattice is a p -dimensional complex torus today called its Jacobian, and because the original curve has a complex structure determined by its $3p - 3$ moduli, the space of all possible Jacobians arising from algebraic curves in this fashion has dimension at most $3p - 3$. On the other hand, the space of all $p \times p$ symmetric matrices is clearly of dimension $\frac{1}{2}p(p + 1)$, and so the space of p -dimensional complex tori also has that dimension. The number $3p - 3$ is less than $\frac{1}{2}p(p + 1)$ except when $p = 2$ and $p = 3$, from which it follows that the period matrices that come from algebraic curves are somehow special, and the problem raised by Schottky in 1888 was to characterise the theta functions that arise in that way.

Schottky devoted the first half of (Schottky 1888) to the Weierstrassian theory of theta functions in genus p . There are, he reminded his readers, 4^p different theta functions associated with a given lattice, which are distinguished by their characteristics, and suitable quotients of them form Abelian functions. Following Riemann, Frobenius had made a detailed study of the way the theta functions with different characteristics can be regarded as even or odd and meaningfully formed into families, and Schottky conducted his readers through the key ideas. He then turned, in the second half of his paper, to the problem that, as he put it, the number of equations of degree 4 which lead to Abelian functions in four variables depends on 9 parameters, while the dimension of the space of moduli of periodicity is 10—that is, the parameters determining a (complex) 4-dimensional period lattice are 10 in number. Since these moduli of periodicity are determined from the curve by the Riemann period relations they depend on the 9 parameters, there must be an equation between the moduli. On the other hand, the dimension of the space of Abelian functions obtained from arbitrary theta functions is indeed 10, so one can look for an equation between the moduli which characterises the lattices that arise from curves. A long and ingenious argument that we cannot summarise here led him to answer his question by showing that when $p = 4$ the null values of the even

theta functions must satisfy an explicit equation of degree 16 that the general theta functions do not satisfy.

The complexity of the problem grows rapidly with p , and little more was done with it until Schottky returned to it in 1909 with his student H.W.E. Jung in (Schottky and Jung 1909) and found a family of equations that are satisfied by the theta functions that come from curves, but not enough to provide a characterisation, and there the matter seems to have rested, aside from a paper by Wirtinger in 1938, until the 1950s and the work of Weil and Andreotti.

In 1893/94 Hilbert lectured at Göttingen on a generalisation of the theory of modular functions to modular functions of several complex variables. His motivation lay in the theory of numbers, and his discoveries connected his newly defined functions with the transformation theory of theta functions of several variables. In due course Hilbert's ideas attracted a student, his first, Otto Blumenthal, who wrote a series of papers outlining and extending them. It would take us too far afield to discuss the theory of Hilbert modular functions, but Blumenthal did indicate that he would show how the new functions enabled one to prove two theorems due to Weierstrass about multiply periodic functions (the dependency theorems). Illness, he said, had prevented Weierstrass from publishing proofs of these results.

9.4 The General Theory of Complex Functions of Several Variables

9.4.1 *Blumenthal*

Blumenthal¹¹ introduced the first of his papers (1903, 509) on modular functions with some interesting historical remarks:

If up till now the theory of functions of several variables has lagged behind the widely extended and highly developed theory of functions of a single complex variable, this can essentially be attributed to the lack of interesting and appropriate examples with which a general theory could connect. In fact, for a long time the theory of Abelian functions has stood somewhat on its own, although in recent years ... Cousin has taken up the general theory of single-valued functions of several variables for the first time since Weierstrass.

The point about the lack of interesting examples, although naturally meant to quicken interest in the new modular functions, is nonetheless apposite. There had

¹¹Otto Blumenthal was the first of Hilbert's students. He taught at the University of Aachen for many years, and became an editor of *Mathematische Annalen*, but when the Nazis came to power he was forced to resign because one of his grandparents was a Jew, although he himself was a practising Protestant. In 1939 he obtained a teaching position in Delft, but in 1943 he was captured, and he died in Theresienstadt in November 1944, age 68.

indeed been little progress on the theory of complex functions of several variables until the paper by Cousin.¹²

9.4.2 Cousin

Cousin's point of departure in his thesis (Cousin 1895) was Poincaré's theorem on meromorphic functions, and his aim was indeed to establish a theory of functions of several variables having the greatest possible analogy with the single-variable theory.¹³ In particular, he sought to generalise the theorem of Mittag-Leffler on the existence of a function with prescribed principal parts and what he considered as its consequences, two theorems of Weierstrass (see Sect. 6.7.1). One asserted the existence of a function with prescribed zeros and the other the representation of a function with prescribed zeros and prescribed poles as a quotient of two entire functions. He found that he could succeed in establishing suitable generalisations, but that the fact that the zeros of a function of several variables were no longer isolated points forced him to pose the second theorem in a different way. Moreover, the generalisations were not entirely straightforward, as will be shown below. On the other hand, however, he did succeed in giving entirely function-theoretic proofs of his theorems, thus avoiding Poincaré's reliance on ideas from potential theory.

Cousin's generalisation of Mittag-Leffler's theorem was stated first for certain functions of two variables, the generalisation to n variables being quite straightforward. (Although it is worth observing that even in 1921 Bieberbach could remark early on in his survey of complex function theory in several variables that he would restrict himself to examples of functions of two variables: "This must suffice for a theory that is so much in flux as the one presented here".) Let us state Cousin's theorem too simply first, and then correct it below. He considered a region $S_1 \times S_2$ of $\mathbb{C}^2$, where S_1 and S_2 are finite and simply connected regions of $\mathbb{C}$ containing two (not necessarily connected) regions s_1 and s_2 , respectively. He assumed that, for each interior point (a, b) of $S_1 \times S_2$ there was a function f_{ab} defined on a neighbourhood N_{ab} of (a, b) such that if (a', b') was in N_{ab} then $f_{ab} - f_{a'b'}$ was regular. Then, he showed there was a function F defined on the interior of $s_1 \times s_2$ and which differed from f_{ab} (where $(a, b) \in s_1 \times s_2$) by a regular function. In this

¹²For an analysis of Cousin's work and the subsequent developments in sheaf theory, see [Chorlay \(2010\)](#).

¹³See Maurey, B. and J.-P. Tacchi, La genèse du théorème de recouvrement de Borel, <http://www.math.jussieu.fr/~maurey/articles/GenRev.pdf>, who cite "le recueil de l'association des anciens élèves de l'École Normale Supérieure" for 1934. It seems that Cousin was born in Paris on 18 March 1867 and studied mathematics at the École Normale where he wrote his doctorate in 1886. He then taught at the Lycée in Caen until 1894, when he became a maître de conférences at Grenoble, and then in 1902 he became a professor at Bordeaux. His last publication was published in *Acta mathematica* in 1910, he was Head of the Faculté des sciences at Bordeaux from 1924 to 1930, and he died in 1933.

version of the theorem the given functions were assumed to be single-valued and “without lacunary spaces”, meaning that there were regular points arbitrarily close to any singular points of the function.

The proof depended chiefly on what Cousin called the fundamental theorem. This applied to functions of the above type, which he ingeniously considered as functions of $n + 1$ complex variables $(\mathbf{x}, y)$, where each component of $\mathbf{x} = (x_1, \dots, x_n)$ is constrained to lie in a disc-like region of a copy of $\mathbb{C}$. For the fundamental theorem y was supposed to lie in a region S made up of several disc-like regions R_k overlapping at most along curves or at points, and on the interiors of which were prescribed functions $f_k(\mathbf{x}, y)$. The theorem asserted that there was a function $F(\mathbf{x}, y)$ (also of the kind described above) defined for all $y \in S$ and differing from $f_k(\mathbf{x}, y)$ in R_k by a regular function. The proof of the theorem was a careful and ultimately straightforward exercise in analytic continuation. The Cauchy integral formula was used to define a function regular everywhere except on the overlaps of two regions. The restriction, called ϕ_k , of this function to the interior of the region R_k was then taken and shown to continue analytically to an analytic function on all of the interior of S . Had a different region R_m been chosen, then, Cousin showed, the following equation would hold:

$$\phi_k - \phi_m = f_m - f_k.$$

Consequently the sought-for function could be defined by the equation

$$F(\mathbf{x}, y) = \phi_k(\mathbf{x}, y) + f_k(\mathbf{x}, y).$$

Cousin also had an alternative version of the theorem which applied to functions which were not single-valued but were the logarithms of regular functions, and which yielded a function on S defined up to a multiple of 2π .¹⁴

Cousin also proved that given functions $v_{ab}(\mathbf{x}, y)$ in the same sort of way but for which the quotients $\frac{v_{ab}(\mathbf{x}, y)}{v_{a'b'}(\mathbf{x}, y)}$ were regular and non-zero when defined, there was a regular function $V \times S(\mathbf{x}, y)$ defined in $s_1 \times \dots \times s_n$ and which was such that the quotient $\frac{V(\mathbf{x}, y)}{v_{ab}(\mathbf{x}, y)}$ was regular and non-zero at the point $(\mathbf{a}, b)$. From this he deduced, using Weierstrass's factorisation theorem, that a function of any number of variables that had only inessential singularities in a region of the form $s_1 \times \dots \times s_n$ was representable there as the irreducible quotient of two regular functions. He went on to show that indeed the function was the irreducible quotient of two convergent series. In particular, he observed, if the region $s_1 \times \dots \times s_n$ was all of $\mathbb{C}^n$, then this last result generalised Poincaré's theorem about meromorphic functions from two to any number of variables.

This summary of Cousin's arguments was deliberately imprecise. As [Gronwall \(1917\)](#) showed in his important criticism of Cousin's paper, Cousin had established

¹⁴[Gronwall](#), in his (1917), observed that [Osgood](#) had noted in (1914) that Cousin's argument is sketchy when it comes to specifying what branches are involved and gave Osgood's modifications to make the matter precise.

a generalisation of Mittag-Leffler's theorem to several variables, but the purported generalisation of Weierstrass's theorem was flawed. Specifically, all, or all but one of the neighbourhoods N_{ab} of (a, b) must necessarily be simply connected and what came to be called polydiscs or polycylinders: products of the form $D_1 \times D_2 \times \dots \times D_{n+1}$, where each D_k is a disc in $\mathbb{C}$ (for a polydisc) or perhaps $\mathbb{C} \cup \infty$ (in the case of a polycylinder). This was to ensure the existence of a polynomial (and not just a regular function) arbitrarily close to f_{ab} on N_{ab} . This resulted in the otherwise obscure restriction of the sought-for function to regions of the form $s_1 \times \dots \times s_n$ inside $S_1 \times \dots \times S_n$. Cousin himself had been aware of the issue and had devoted the final third of his paper to trying to avoid this restriction. He was, however, only able to show that the function F existed on all of $S_1 \times \dots \times S_n$ provided that all or all but one of the regions S_k were disc-like. What Gronwall showed in the final third of his paper was that it is possible to specify a quotient, $f(z_1, z_2)$, of two functions which is a non-constant, meromorphic, and quadruply periodic single-valued function on the domain $S_1 \times S_2$, where each S_i is the complex z_i plane with the origin removed, such that the quotient cannot be written as the quotient of two relatively prime holomorphic functions on $S_1 \times S_2$. When one follows the steps of Cousin's construction, the result is the representation of f as a quotient G_1/G_0 but the functions $G_0(z_1, z_2)$ and $G_1(z_1, z_2)$ have a common zero set that cannot be removed if the G 's are to remain single-valued.

So the restrictions are in the nature of the problem and what Cousin seems to have suspected and Gronwall later showed to be the case is that there is a genuine difference between complex function theory in one and several variables: the Mittag-Leffler theorem for one variable does, and the Weierstrass theorem for one variable does not, generalise to several variables without a restriction on the domains. Perhaps for that reason the Cousin theorems are today called the Cousin problems in the literature; the term was apparently introduced in (Cartan 1934).

Cousin's first problem asks for a meromorphic function with given poles, and Cousin's second problem asks for a holomorphic function with given zeros. The first problem is solvable on any domain of holomorphy (Oka 1937) and the second problem is solvable if in addition the second cohomology group of the domain with integer coefficients vanishes (Oka 1939; Serre 1953). So, for example, the second problem is solvable on $\mathbb{C}^n$, and so every meromorphic function on $\mathbb{C}^n$ is a quotient of entire functions, see Vitushkin (1990, 13).¹⁵

The reaction to Cousin's work in 1895 is harder to determine. Hurwitz described it in *Fortschritte* (26.0456.02) as an essential advance in the general theory of analytic functions of several variables, and gave it a lengthy review. Even so, the paper did not immediately open up the field of complex function theory in several variables, and indeed Cousin seems to have left it himself a few years later. It was, however, discussed at length by Osgood in his two-volume work of 1924 and by other German writers in the 1920s.

¹⁵Modern treatments find the Cousin problems are cohomological in nature, see Krantz (1982, Chap. 6).

9.4.3 Hartogs

Matters began to change in 1904, when Hartogs wrote his Habilitationsschrift at Munich, where he had been a student of Pringsheim's.¹⁶ On the strength of this thesis he was invited to lecture to the DMV in Stuttgart in September 1906 (see [Hartogs 1907](#)). He began the lecture in the by-then customary way by observing that although the study of functions of several complex variables was almost as old as the theory of a single variable, there was almost no general theory of several variables. He attributed this to the fact that the one and several variables cases lay in no way parallel. For example, the zeros and the singularities of functions of several variables were no longer isolated. Indeed, as the Weierstrass preparation theorem showed, the zero set of an analytic function was analytic in character. His own contribution was to point up another disparity between the two cases, for he showed that there were certain domains which had the property that any analytic function defined on such a domain could always be extended to a function analytic on a larger domain. It is a consequence of the Riemann mapping theorem in the single variable theory that every simple closed Jordan curve is a natural boundary for some function analytic in its interior; Hartogs' result shows that this fact does not generalise to functions of several variables. To introduce a later phrase, a domain which is the largest domain of an analytic function is called a domain of holomorphy. Hartogs' results established that the determination of domains of holomorphy in the case of two or more complex variables was a non-trivial task. For example, he showed that if there was a function $f(x, y)$ which was defined on a polydisc $D_1 \times D_2$, analytic on $\partial D_1 \times D_2$ and also on $D_1 \times \{0\}$, then the function was analytic everywhere in the polydisc.

Hartogs based his work on an analysis of power series in two variables. Such a series may be written as $\sum_{m,n} a_{mn} z^m w^n$. Suppose that the series converges absolutely for $|z| < R$ and $|w| < S$. For each fixed value of one variable (say z) the double series may be considered as a series in the other variable (in this case, w). Choose r in the range $0 < r < R$, and for each fixed value of z in the interval $0 < |z| < r$ determine the radius of convergence (call it s_z) of the double series regarded as a series in w . Let s be the least upper bound (assumed finite) of the values of s_z . Then s is a function of r , denoted $s = \varphi(r)$, and called the associated radius of convergence. For example, for the double series

$$\sum_{m=0}^{\infty} z^m w^m = \frac{1}{1 - zw},$$

the associated radius of convergence is $s = \frac{1}{r}$.

¹⁶Friedrich Hartogs became a Privatdozent in 1909, and an extraordinary professor at Munich in 1912, but he had to wait until 1927 to be appointed full professor there. After the Nazi party came to power in 1933 Hartogs, who was a Jew, suffered increasingly difficult times until he was forced to retire in 1935. After the Kristallnacht of 9–10 November 1938 he was imprisoned at Dachau for several weeks. Further humiliations followed his release, including a enforced divorce from his Aryan wife to protect her, and in the end he committed suicide in 1943. For biographical information on Pringsheim, Hartogs and other Jewish mathematicians at Munich University, see [Bauer \(1997\)](#).

There are several conditions that a function $s = \varphi(r)$ must satisfy if it is to be a radius of convergence function. The necessary and sufficient condition which Hartogs singled out as the most basic is this: if $r_1 < r_2 < r_3$ are three points in the interval of definition of φ , then¹⁷

$$\left| \begin{array}{l} 1 \log r_1 \log \varphi(r_1) \\ 1 \log r_2 \log \varphi(r_2) \\ 1 \log r_3 \log \varphi(r_3) \end{array} \right| \leq 0.$$

The idea of the associated radius of convergence enabled Hartogs to use the Cauchy integral theorem in several complex variables to refine Hurwitz's result that an analytic function of two variables cannot have an isolated singular point. Hartogs considered a single-valued function F of z_1 and z_2 , defined for z_1 in a domain B_1 and z_2 in a domain B_2 and regular on the boundary of the domain for z_1 and z_2 . By a domain he meant a closed point-set in $\mathbb{C}^n$, $n \geq 1$, whose interior is path-connected. He denoted the boundaries of B_1 and B_2 by C_1 and C_2 , respectively. He also required that there be an analytic function mapping B_1 onto the interior of B_2 . Under these conditions Hartogs showed that the function $F(z_1, z_2)$ has no singular point in the entire domain for z_1 and z_2 .

His proof rested on an insight into the Cauchy integral formula, which says that

$$F(z_1, z_2) = \left(\frac{1}{2\pi i} \right)^2 \int_{C_1} \int_{C_2} \frac{F(\xi, \eta)}{(\xi - z_1)(\eta - z_2)} d\xi d\eta.$$

Hartogs noted and exploited the fact that for integral theorem to make sense it is not necessary that the function F be defined on all of $B_1 \times B_2$, but only on $C_1 \times B_2$ and on $\{(z_1, g(z_1))\} \subset B_1 \times B_2$. His proof also required quite stringent conditions on C_2 the boundary of B_2 , namely that it meet straight lines in the z_2 plane either in straight line segments or in at most two points.

Hartogs published a detailed account of his theory in *Acta mathematica* in 1908, where he went on to show that the set of singular points of an analytic function of several complex variables, if continuous (the word of the day for what we would call connected), must be composed of the zero sets of analytic functions. He showed that if $f(x, y)$ is a (single-valued branch of an) analytic function, singular at $(x, y) = (0, 0)$, and if in the polydisc $|x| < \rho$, $|y| < \rho'$ the singular points (ξ, η) are such that to every ξ there is exactly one singular point (ξ, η) , and η depends continuously on ξ , then η is an analytic function of ξ which is regular at $(0, 0)$. In other words: if the singular points of a function f form a continuous (i.e. connected) set, then they form an analytic one.

¹⁷Osgood commented (1914, 38) that "this theorem in its present form was first given by Fabry C.R. 134 (1902) pp. 1190, and rediscovered by Hartogs." He also noted that a certain A. Meyer had already given a geometric interpretation of such a function in 1883: the graph of $\log s$ as a function of $\log r$ is continuous and concave downwards.

Hartogs also showed that if $f(z_1, z_2)$ is analytic at the points $(0, z_2)$, $0 < |z_2| < h$, and if f is singular at $(0, 0)$ then for each z'_1 in a certain region $B = |z_1| < \rho$ there is at least one z'_2 in $|z_2| < h$, $0 < |z'_2| < |z_2|$, such that (z'_1, z'_2) is a singular point of the function f . He used this result to give conditions which imply that, given two domains B and D in $\mathbb{C}$, and a neighbourhood K of a point in D , a function analytic on $B \times K$ extends to an analytic function on all of $B \times D$.

9.4.4 E.E. Levi

What E.E. Levi showed (1910, 66) was that if $(0, 0)$ is an essential singular point of a complex function $f(z_1, z_2)$ which is meromorphic at all the points $(0, z_2)$, $0 < |z_2| \leq k$, then for every ε there is a δ such that in every plane $z_1 = a_1$, $|a_1| \leq \delta$ there is at least one essential singular point of the function f for which $|z_2| \leq \varepsilon$. It follows that the set of essential singular points of an analytic function of two complex variables is perfect, because it is clearly closed and by the result just given it cannot contain any isolated points.

Levi then considered a perfect set of points E and an arbitrary point O , and, denoting by $r(O, P)$ the distance between the points O and P , he claimed that if there is a point P' in E such that the function $r(O, P)$ has a local maximum at the point P' , then there is no single-valued analytic function which has the set E as the set of its essential singular points. From this theorem it followed that “the set of singular points of an analytic function $f(x, y)$ cannot contain a perfect, isolated subset which lies entirely in the finite domain” (Levi 1910, 207)—which we take to mean in more modern language that the set of singularities of the function cannot form a compact set. Levi also reformulated the theorem in a way that extended Hartogs’ earlier results: if a function is meromorphic at every point of a closed hypersurface (one that is the boundary of a four-dimensional region), then it is meromorphic in some neighbourhood of the hypersurface, and in particular it is meromorphic inside the hypersurface. Whence it followed that a single-valued meromorphic function of two variables cannot have the boundary of its domain of definition bound a finite region, or, in other words, its lacunary space cannot lie entirely in the finite domain. This showed that Weierstrass’s claim was erroneous.

In Sect. III of his paper Levi set himself the task of deciding when a given real hypersurface S defined by an equation of the form $\varphi(x_1, x_2, y_1, y_2) = 0$ is the natural boundary of a domain of definition of a single-valued meromorphic function, or when, as he put it, there is a meromorphic function in a region with S as its boundary and for which every point of S is an essential singular point. He argued that for S to be a natural boundary it was necessary that at every point P of S there should be a characteristic surface Σ which touches the surface at P and otherwise meets a neighbourhood of the point only at P . Levi, following Levi-Civita, defined a characteristic surface to be one defined by an analytic function. By looking at the Taylor expansion of φ , he showed that the necessary condition became the condition

that Ω_φ always has the same sign on the hypersurface S , where Ω_φ is an expression that can be written as

$$\begin{vmatrix} 0 & \varphi_w & \varphi_{\bar{z}} \\ \varphi_{\bar{w}} & \varphi_{w\bar{w}} & \varphi_{z\bar{w}} \\ \varphi_{\bar{z}} & \varphi_{w\bar{z}} & \varphi_{z\bar{z}} \end{vmatrix}.$$

It followed that for a real hypersurface S to be the common boundary of two functions it must be a solution of the differential equation $\Omega_\varphi = 0$ (Levi 1910, 207).¹⁸

Levi now asked if the necessary condition was also sufficient. He found himself unable to answer this question exactly, but he could show that given a hypersurface defined by the differential equation $\Omega_\varphi = 0$ there always were meromorphic functions in the regions $\varphi > 0$ and $\varphi < 0$ which have at least a part of the hypersurface as their common boundary.

In the paper (1912) that he presented to the Weber *Festschrift* Blumenthal gave a somewhat more intuitive geometric interpretation of real hypersurfaces S that are natural boundaries, and so satisfy Levi's differential equation. At each point of the hypersurface he took $X_1 + iX_2$, where $X_1 + X_2$ is a linear expressions in the complex variables x and y such that $X_1 = 0$ defines the tangent hyperplane at the point and the direction $X_1 > 0$ always lies on the same side of S . If $Y_1 + iY_2$ is an arbitrary vector on S , then the expression $\frac{\partial^2 X_1}{\partial Y_1^2} + \frac{\partial^2 X_1}{\partial Y_2^2}$ never changes its sign on S . Blumenthal now claimed that at anywhere this expression is nonzero, there is a function defined only on the side of S where the expression is positive and that has S as its singular variety. He also gave simple examples of hypersurfaces that could not be the singular variety of a complex function.

9.4.5 Poincaré and Conformal Maps in Two Variables

In his paper (1907b), Poincaré raised the question of the complex analytic nature of maps between domains in two complex variables. He considered his work to be incomplete, although it contained enough material to establish conclusively that the boundaries of some domains are such that there can be no analytic map between the interiors of these domains. It follows that there is no possibility of an analogue of the Riemann mapping theorem in two complex dimensions, but Poincaré stopped short of giving specific examples, and the first of these are due to Reinhardt in 1921.

Poincaré began his paper by observing that in the complex function theory of a single complex variable, there are two distinct ways of asking a question about the existence of an analytic map. One, which he called the local problem, takes as given two copies of $\mathbb{C}$ the first of which contains a curve ℓ upon which there is a point

¹⁸This marks the first appearance of the Levi form. See Range (2012) for an introduction to the convexity issues that arise.

m and the second of which contains a curve $\mathcal{L}$ upon which there is a point M , and asks if there is an analytic function regular in a neighbourhood of m that maps m to M and ℓ to $\mathcal{L}$. The second problem, which he called the extended problem, takes as given two copies of $\mathbb{C}$, the first of which contains a closed curve ℓ bounding a domain d and the second of which contains a closed curve $\mathcal{L}$ bounding a domain D , and asks if there is an analytic function that maps ℓ to $\mathcal{L}$ and d to D . The former problem is always solvable and in infinitely many ways; the second problem has a unique solution via the Dirichlet principle.

The analogous problems for analytic functions of two complex variables behave very differently, however, as Poincaré proceeded to show. The local problem takes as given two copies of $\mathbb{C}^2$, the first of which contains a three-dimensional “surface” s upon which there is a point m and the second of which contains a three-dimensional hypersurface S upon which there is a point M , and asks if there is an analytic function regular in a neighbourhood of m that maps m to M and s to S . The extended problem takes as given two copies of $\mathbb{C}^2$, the first of which contains a closed hypersurface s bounding a domain d and the second of which contains a closed hypersurface S bounding a domain D , and asks if there is a regular function that maps s to S and d to D .

Poincaré showed at once that the local problem will not always have a solution. It is over-determined because it asks for three functions that are the solutions of four differential equations. So Poincaré turned the local question into one about types of surfaces, classified according to their groups of analytic automorphisms. He observed that if a surface s admits only the identity analytic automorphism, then the local problem has at most one solution, else the automorphism can be used to generate a second solution. Similarly, if two surfaces correspond under an analytic automorphism, their groups are necessarily conjugate, and so the surfaces belong to the same class. Poincaré now invoked Lie’s theory of transformation groups to obtain all the relevant groups, citing Lie *Theorie der Transformationsgruppen*, vol III, and Campbell’s *Introductory treatise* (1903) to establish that there are 27 possible groups and showed explicitly that for most groups there is a hypersurface having that group as its analytic automorphism group, but some groups correspond to two-dimensional surfaces. It follows that there are hypersurfaces that not analytically equivalent. Unfortunately, Poincaré’s account was very unspecific. The hypersurface (hypersphere) with equation $z\bar{z} + z'\bar{z}' = 1$ was discussed, and its group described explicitly (in Sect. 7), but otherwise the nearest Poincaré got to describing a hypersurface with a different group was to indicate how its equation could be found by means of Lie’s theory.

In Sect. 8 of the paper Poincaré turned to the extended problem in two complex dimensions. He supposed given an analytic map between the hypersurfaces s and S and asked if it necessarily extended to an analytic map between the interiors, d and D . He found that Hartogs’ theorem said directly that the answer was “Yes”, and sketched his own proof of that result. The paper then ended with some investigations of the hypersphere and hypersurfaces “infinitely close” to it, which we shall not discuss.

Credit for the first explicit examples of domains with inequivalent boundaries therefore goes to K. Reinhardt, who published his account in 1921 in the paper that introduced what are often called Reinhardt domains. As Reinhardt noted, his paper marks an advance upon Poincaré's because it drops the requirement that the map be regular on the boundary hypersurfaces. It also used quite different methods, being an ingenious blend of elementary four-dimensional geometry and the use of two complex variables.

Reinhardt took the unit sphere (as he called it) with equation $|z_1|^2 + |z_2|^2 = 1$ and the unit cylinder $|z_1| \leq 1$ and $|z_2| \leq 1$. What he called a circle domain (*Kreisbereich*) is a domain that is cut by every plane parallel to the z_1 -plane in a circle with its centre in the z_2 -plane, and by every plane parallel to the z_2 -plane in a circle with its centre in the z_1 -plane. A lengthy but elementary analysis, in the course of which he showed that the analogues of Schwarz's lemma and the reflection principle hold in two complex dimensions, enabled Reinhardt to characterise completely the automorphisms of various types of circle domain. In particular, he showed that the automorphism groups of the unit sphere and the unit cylinder are different, and therefore the domains cannot be holomorphically equivalent.

9.5 Conclusion

This chapter has looked at a number of issues in the complex function theory of several variables that occupied mathematicians in the years up to 1914. It looks at the problems that arose, and at a number of breakthroughs, and shows that some progress was made in understanding the similarities and the differences with the function theory of a single variable. Nonetheless, truly decisive progress only came with each subsequent generation, and while that work cannot be treated here in any detail a few brief comments are in order.

There have been several considerable shifts of emphasis. Contrary to Weierstrass's point of view, the Cauchy–Riemann equations have assumed a major role, and other partial differential equations have also entered the subject. This does not mean that power series methods were marginalised—Henri Cartan made great use of them—but a somewhat exclusive preference for them has been blended with other techniques. Chief among the new techniques is the method of sheaves, introduced by Jean Leray, which have been considered from a historical point of view in (Chorlay 2010). This approach has greatly illuminated the study of singularities and meromorphic functions, and also the Cousin problems, which are usually split into two types: the additive and the multiplicative. The additive problem generalises the Mittag-Leffler theorem: it specifies meromorphic functions f_j on domains U_j with the property that $f_j - f_k$ is holomorphic on $U_j \cap U_k$, and asks for a meromorphic function f such that $f - f_j$ is holomorphic on U_j . The multiplicative problem generalises Weierstrass's theorem: it specifies holomorphic functions f_j on domains U_j with the property that the quotient f_j/f_k is holomorphic on $U_j \cap U_k$, and asks

for a meromorphic function f such that f/f_j is holomorphic and non-vanishing on U_j . These problems can be asked for a variety of domains and so in many other settings than the original one, and after Oka's breakthroughs in the late 1930s led to a considerable amount of research. Following the work of Cartan, they are nowadays discussed in terms of the cohomology of the domain.

After the work of Hartogs, Poincaré, and Levi, a great deal of attention was focussed on the topic of domains of holomorphy and the Levi condition on the boundary, the geometry of which was only understood after the work of Oka, Narasimhan, Bremerman, Norguet, Grauert, and others. The definitive result was that a domain is a domain of holomorphy if and only if it is strictly pseudoconvex. A number of generalisations of the problem to different setting and of the accompanying concept of convexity were developed, and the topic became one of the dominant branches in the complex function theory of several variables.¹⁹

On the other hand, the topic of Abelian functions, which so animated analysts in the nineteenth century, seems not to have drawn much attention from analysts in the twentieth century. Rather, it has become a flourishing part of algebraic geometry.

¹⁹See, for example, [Krantz \(1982\)](#) and [Siu \(1978\)](#).

Chapter 10

The Textbook Tradition

What is a text-book? It should, I think, be a complete, comprehensible, and unified presentation of a scientific domain.

Bieberbach

10.1 Introduction: The Modern Consensus

It is a significant event in the life of a mathematical topic when it enters the student degree syllabus, and another when it does so in a generally agreed way in many different universities. Today a remarkable degree of consensus exists about what constitutes elementary complex function theory. It is the purpose of this chapter to investigate how this consensus came about and in what way it embodies a vision of complex function theory central to the modern mathematical syllabus.

The consensus is formed around the topics Serge Lang described as “essentially what cannot be left out of, say, a one-semester course” (Lang 1985, v) which were complex numbers and functions, power series, the Cauchy integral theorem, Cauchy’s formula with applications to power series expansions, Laurent series, poles and essential singularities, the calculus of residues, conformal mappings, and harmonic functions. The same topics, in a different order, are the heart of Ahlfors’ famous textbook (1953b); he added a chapter on many-valued functions. Hille’s textbook (1962) omitted the topic of harmonic functions. The agreement extends further back, to Titchmarsh’s book (1932) and includes Priestley’s book (1985), in which she added a chapter on Fourier series. Kodaira (1977) is yet another book of this kind.

Several of these books of course include further chapters, even second volumes, where more advanced topics are dealt with. Naturally there is less consensus here, as the authors take the opportunity to work round to their own areas of expertise. But prominent among them are such topics as: elliptic functions, analytic continuation, the theory of entire and meromorphic functions, and the Riemann mapping theorem (all of them in Lang’s book); algebraic functions and compact Riemann surfaces,

and normal families (in Hille, vol. 2 and Ahlfors). One could continue cataloguing in this vein, looking for the Weierstrass and Mittag-Leffler representation theorems, noting the occasional chapter on applications, but perhaps the point is made.

Nor is this agreement restricted to books written in, or translated into, English. It can be found in, for example, Behnke and Sommer's book (1955, 2nd ed. 1962), in the one chapter of Dieudonné's *Foundations of modern analysis* devoted to complex function theory, and, with one important modification, in Remmert's *Theory of complex functions*. The modification, now to be found in a number of recent textbooks, is the systematic use of topology, replacing the rather ad hoc use of real analysis in earlier textbooks. This in turn derives from the growing realisation that some fundamental topological concepts are at work in analytic function theory.

Even when this consensus is deliberate, as it is when Lang writes "In some sense, I think the classical German pre-war texts were the best (Hurwitz–Courant, Knopp, Bieberbach, etc.) and I would recommend anyone to look through them" it is nonetheless interesting. It conveys an image of an agreed subject matter that is enough for many purposes and also adapted to specialist study. There is even good agreement about what the natural second stage topics are, as well as about their more advanced status. Within this consensus there is room for a little disagreement. Some of this is pedagogic: should one introduce Möbius transformations early or late? Where to study power series methods? Some of it is more substantial: the question of the winding number is one, and the level of generality of the Cauchy integral theorem is another. But the agreement extends to important matters of detail, too. The very definition of a complex function is standard, as is the use of the Goursat proof of the Cauchy integral theorem.

Moreover, this route through the subject (taken from Ahlfors' text) is more or less canonical: A holomorphic function is one that, by definition, has a derivative (not necessarily continuous). A holomorphic function therefore satisfies the Cauchy–Riemann equations (but one must discuss the matter of its domain here). The first main result is the Cauchy integral theorem (for what class of closed paths? in what simply connected domain?) from which the Cauchy integral formula follows. The integral theorem is fundamental, it is the formula that unlocks the most results. Using it, one shows that a holomorphic function is infinitely differentiable and has a Taylor series expansion. The maximum modulus principle plays a role here in guaranteeing convergence. A theory of poles and their finite orders enables one to pass to the Cauchy residue theorem, Laurent series expansions, and usually to the enumeration of zeros and poles inside a region. Essential singularities will be described, and the Casorati–Weierstrass theorem proved. A few more topics lie close at hand, Morera's theorem and Riemann's removable singularities theorem are cases in point.

With this image of the subject before us, we have looked at as many textbooks as we could find written before 1940. This produced a list of more than 60 books. It is interesting, but not surprising, to observe that the most prolific nation was Germany, followed by France. English and American textbooks are about as numerous as Italian ones, Spanish, Russian (somewhat to our surprise) Polish and Danish ones follow along behind. Several authors give the impression that German textbooks

were for a long time divided into Riemannian and Weierstrassian camps, until this became a sore point with several authors and attempts were made to unite the two approaches; we shall examine this claim below. The French, on the other hand, were slow to follow the lead of Briot and Bouquet, and waited quite some time for authors who could assimilate what Riemann had tried to say. They then produced two famous works of a much more advanced level: the treatises by Picard–Simart and Appell–Goursat (which are not considered in this survey).¹ Only the Italians among the other nations could from the first boast of prominent researchers in complex function theory. This gives the texts by Casorati, Bianchi, and Pincherle a special significance, but Casorati’s aside they are strangely late (1899–1901). English and American writers had instead to work out where they stood as, so to speak, eager pupils. Forsyth took the view that he should present, in a unified way, the three main theories (due to Cauchy, Riemann, and Weierstrass). Harkness and Morley, on the other hand, sided heavily with the Weierstrassian approach. The English, but also the Germans, made translations as well, which are worth discussing. The situation in Denmark, Spain, Russia and other countries will be described below.

Each author had to decide what the subject was. In the early period, their views were often determined by where they were trying to get to. One might say that complex function theory was still subservient to its applications. For Briot and Bouquet, and later Koenigsberger, the point was to give a good account of doubly periodic functions. For Durège in 1864 the point was to get to Riemann’s theory of functions on a Riemann surface. For Fricke, who had the deliberate aim of stimulating applied mathematicians, some of the famous problems first studied by Legendre were a focus. For other authors, such as Pierpont (1914) to give a late example, it was the theory of special functions. Osgood is unusual in having sought to present a rich theory of complex functions in both one and several variables. Gradually this changes, and books begin to be written on what one might call elementary complex function theory. This is a subject at once basic (in the sense of constituting a set of elements) and easy (elementary, suitable for non-specialists). There are many moments when this transition is visible, as when Ahlfors decided not to include a chapter on elliptic function theory, saying (1953b, vii) “One of the main reasons is that it is hardly possible to improve on the beautiful treatment in E.T. Copson’s book (1935)”.² What one can see is authors allowing material that can, after all, be got through in about 100 pages, to expand until it fills a book and the many vital connections it has with other topics are marked by the passage to another book, another course, another year. The material differentiates, a palm appears and fingers bud outwards.

It is unlikely that a graphical or tabular presentation of the content of 60 books would communicate much. In any case, with each book it is the route as much as the

¹Similarly, we omit a discussion of Weber’s *Lehrbuch der Algebra* although volume II concludes with a discussion of algebraic curves and the Riemann–Roch theorem.

²Ahlfors later changed his mind, and the third edition of his book does have a chapter on elliptic functions.

highlights that naturally occupies one's attention. But nor would a summary of each book be very revealing. Instead, we have opted to give an analysis aimed at seeing how the boundaries and internal structure of the subject shifted in various countries over the years. We have examined each book from four points of view: what is its content? how is it structured? what is its aim? what is on the boundary, and what is left out? This approach allows us to look at key points in more detail, such as the level of rigour employed, but more importantly the way it contributed to what was a long-running debate about the extent to which the topic of complex functions could be said to form an independent theory. We can then draw in the reviews these books received and begin to form an impression of how they fared and what influence they had. Were the books translated, or copied in some way?

There were some surprises for us in this survey of the entire literature. It became clear that whole books could be written on Riemann surfaces and algebraic curves with no more than a nod in the direction of complex function theory. This was Klein's approach in his lecture course of WS 1880/81, for example. Whether the Cauchy theory was assumed known, or common-sense was taken to prevail is not clear. The trick is to stop short of theta functions and to concentrate either on the topology (by no means clear in the nineteenth century) or on the close connection to algebraic geometry. We have set such books aside as inappropriate. (We hinted at them briefly earlier, in Sect. 5.11.6, when we discussed responses from the algebraic geometers to Riemann's work. The work of a Portuguese authority on Riemann surfaces (H.M. de Figueiredo) belongs in this tradition just described (see [Gray and Ortiz 1999](#)). On the other hand, Kronecker, whose name is usually left out of these matters, included a rather attractive, straightforward account of much of this material in his lectures ([Kronecker 1894](#)), and they were published shortly after his death in a faithful edition produced by Netto. We shall look at it in more detail below.

One final general observation should be made. These books were written before the modern Definition–Theorem–Proof style of book, with problems at the end of each chapter, came into vogue. It is true that many an author today fails to communicate any sense of excitement in this format, and that an endless string of little results can be boring and uninformative—but they are easy to navigate. By contrast, most of the older books lack an index, a list of symbols, and, unless they are English, problems. Even if a student diligently reads every page, it is hard to find specific results. Unless the structure is entirely linear, and usually it cannot be, it is not always clear which results depend on which. Theorems, sometimes numbered, sometimes not, seldom named, are placed in italics in the text, but definitions often just emerge. Cross-referencing is difficult, and seldom attempted. The only guide is the table of contents, which, fortunately, is usually thorough. Since the few reviews we have are by other experts, we can only wonder at the efficacy of such works, and speculate on the quality of the lectures, seminars, and discussion groups that might have in fact led the students along.

We have also one anomaly to get out of the way: the Harvard professor Benjamin Peirce's *An elementary treatise on curves, functions, and forces*, published in Boston in 1846. In Book III Peirce broached the calculus of imaginary quantities, as he

called it. He presented them geometrically, discussed the modulus and argument of a complex number, genuine and imaginary infinitesimals, as he called them, the imaginary roots of equations, and the exponential, log, and trigonometric functions as complex functions of a complex variable. In Book IV he presented what he called the Residual calculus, replete with the symbol $\mathcal{E}$ on p. 49—but never a mention of Cauchy by name.

10.2 The First Four Textbooks

Because of their priority, the first four books to do with complex function theory seem particularly worthy of note: the *Théorie des fonctions doublement périodiques et, en particulier, des fonctions elliptiques*, by Briot and Bouquet, published in Paris in 1859; the *Elemente der Theorie der Functionen einer complexen veränderlichen Grösse mit besonderer Berücksichtigung der Schöpfungen Riemanns* by H. Durège, published in Leipzig in 1864; Carl Neumann's *Vorlesungen über Riemann's Theorie der Abel'schen Integrale* published in Leipzig in 1865; and Oskar Schlömilch's *Vorlesungen über einzelne Theile der Höheren Analysis gehalten an der K.S. Polytechnischen Schule zu Dresden* of 1866. Cryptic references were also made by Serret and Hermite in the sixth edition of Lacroix's *Traité* (1862).

As the full title of their book makes clear, Briot and Bouquet saw Cauchy's complex function theory as the way to explain the theory of elliptic functions. Without it, they said in their preface, the theory remained highly obscure and very complicated, for the functions could not be defined rigorously. They devoted the first section of their book to his theory before moving on to weightier topics. With Cauchy, they said a complex valued function u of a complex variable z is monodrome if the value of u at each point was independent of the path the independent variable z had taken reaching that point. A function was monogène if it had a derivative at every point and the direction of the derivative was independent of direction. They pointed out that a necessary condition for this was that the Cauchy–Riemann equations hold for the real and imaginary parts of the function u . They attributed to Cauchy the idea that a monogène function is conformal, but gave no precise reference. A function which is finite and continuous, monodrome and monogène everywhere in a portion of the plane they called synectique (in that part of the plane). Examples of synectique functions were provided by power series in z within their circles of convergence.

In Chap. III they proved the Cauchy integral theorem for a function synectique on a given region, expressing it as a comparison (indeed, an equality) between the integral taken along two paths with the same end points. The proof echoed Cauchy's proof using the calculus of variations to vary the path from one to the other. The Cauchy formula followed. To avoid problems with the expression $\frac{f(z)-f(t)}{z-t}$ when $z = t$, they assumed that the value of $f'(z)$ at $z = t$ was finite. Expressions for all the successive derivatives of the function f followed, and the Taylor series for

a synectique function was written down (the argument for its convergence was distinctly superficial). The Laurent series for a function synectique in an annulus was then obtained.

General properties of synectique functions followed: the constancy theorem; not all of its derivatives can vanish at a point, unless the function is constant. When $f(a) = 0$, $f(z)$ is divisible by $(z-a)^n$ for some positive integer n . If $f(a)$ is infinite, whatever route z takes to reach a , then $f(z) = \frac{A_0}{(z-a)^n} + \frac{A_1}{(z-a)^{n-1}} + \dots + \frac{A_{n-1}}{(z-a)} + g(z)$, where $g(z)$ is a monodrome monogène function of z that is finite at $z = a$. They pointed out that the condition on the path taken was to rule out such functions as $e^{1/z}$ which, as they put it, becomes zero or infinite or indeterminate according to the path taken to $z = 0$. They followed this result with one even more worrying to modern eyes: a monodrome monogène function in the entire plane necessarily become infinite for some finite or infinite value of the variable. The point presumably to note is that the function $e^{1/z}$ is not monodrome at $z = 0$. Therefore a monodrome monogène function everywhere in the plane takes every possible value, and two such functions with the same zeros and infinities are a constant multiple of each other. A monodrome monogène function which is infinite only at $z = \infty$ and is not indeterminate there is a polynomial. Such a function with only finitely many points of infinity is a rational function. A monogène but not monodrome function taking m values for each z and only finitely many infinities is the root of an irreducible algebraic equation. The root of an algebraic equation of degree m takes m values for each value of z .

As one might expect, the backbone of this approach is that provided by Cauchy, with some of the master's obscurities. Given that Liouville privately denounced them as plagiarists (quoted above p. 231) it is amusing to see that nowhere is Liouville's theorem clearly stated, either in general in these chapters or for elliptic functions when they are discussed. The muddled talk about functions defined everywhere, even at infinity, and possessing certain properties is a reflection of Liouville's theorem, however. A function on such a domain which is not infinite is bounded, hence constant. Indeed, the distinction between a finite pole and an essential singularity is at best obscurely present and consequently Briot and Bouquet could not suspect the Casorati–Weierstrass theorem. The characterisation of rational functions in terms of their zeros and poles is given, so there is a little surprise in noticing that the fundamental theorem of algebra is not stated. It would have been an easy corollary to draw, albeit one of little relevance in a book aimed at the theory of doubly periodic functions.

Hermann Fischer published a German translation quite swiftly, in 1862, which shows how great the need for such a book had become. He replaced the preface by Briot and Bouquet with a much better historical preface running quickly but informatively through the discovery of elliptic integrals and elliptic functions in the work of Legendre, Gauss, Abel, Jacobi, and others—Briot and Bouquet had managed to mention only Legendre, Abel, Cauchy and Puiseux by name. Thereafter Fischer added numerous footnotes restoring not just German mathematicians to their proper places (for example, Gauss's (1825) is explicitly noted as the reference

for the study of conformal maps) but adding numerous references to the literature, including some to Cauchy. At the muddled claim that a function is characterised by its zeros and infinities, Fischer noted, without giving a full reference, that the idea goes back to Liouville. But he did not alter the presentation of the mathematics itself.

One book must be interjected at this point, the sixth edition of Lacroix's *Traité* with its additional Notes by Serret and Hermite. Serret gave a quick exposition of what it is for a function of a complex variable to be differentiable, deduced that the Cauchy–Riemann equations are then satisfied (they were not named) and that under suitable conditions $f(z)dz$ is an exact differential. From this the Cauchy integral theorem followed, and the existence of a power series expansion of an analytic function. He concluded this discussion with Lagrange's theorem on the reversion of series, illustrated with the example of the Kepler problem, $z = x + tz^m$. Hermite's notes then dealt with the theory of elliptic functions, on the assumption that Cauchy's theory of integrals along curves was known to the reader. From a reading of Briot and Bouquet, one must presume.

The first person to write a textbook exclusively on the theory of complex functions was the German mathematician Jacob Heinrich Karl Durège, who was born in Danzig in 1821. He had taught with Dedekind in Zurich before becoming professor in Prague. His first book, on elliptic functions, was published in 1861. In it he took a deliberately old-fashioned approach, using the techniques of complex function theory only towards the end, when he acknowledged the influence of Puiseux, Cauchy, and Riemann. Then in 1864 he published his book on the theory of functions. It was a successful work of some 225 pages, and it ran to five editions, the fourth appearing in 1893, the year Durège died. An endorsement by Fuchs, who used it his teaching at Heidelberg, appears in the end papers of several mathematics books published at the time.

After an introduction designed to show that imaginary quantities are possible and are created to solve particular problems, such as root extraction, just as negative quantities are introduced to solve problems in subtraction, Durège described their geometrical representation. Like Briot and Bouquet, whom he does not mention, but unlike Cauchy, Durège had no qualms about complex quantities. Then he defined functions of a complex variable. Quoting from Riemann's *Grundlagen*, he said that what defines a function w of a complex variable z is that its derivative $\frac{dw}{dz}$ is independent of dz . Such functions are, he observed, conformal provided that $\frac{dw}{dz}$ is neither zero nor infinite; he postponed a detailed discussion of what can happen at branch points to much later in the book (Section VII). In Section III he described multi-valued functions and their branch points, and after discussing several examples he showed that every such function can always be described by a Riemann surface. To deal with the case when z is infinite Durège introduced the Riemann spherical surface, as he called it and showed how to extend the Riemann surface of a function appropriately. His method was to replace the variable with the variable $u = 1/z$.

In Section IV he began the study of integrals of a complex function. He established the Cauchy integral theorem as Riemann had done in his lectures by

a Green's theorem argument, and then began the study of cases when the contour of integration encloses points of discontinuity of the function. He distinguished, as was necessary, between branch points and infinite values, and gave a criterion for the integral around a branch point to vanish. Section V was a brief discussion of the logarithmic and exponential functions, and then in Section VI he returned to the study of general functions and derived the Cauchy formula. From it he deduced that the derivatives of a single-valued complex function are also single-valued and continuous, the maximum modulus principle, the existence of Taylor series expansions of complex functions, and the uniqueness of analytic continuation.

Section VII was devoted to single-valued functions in general. Such a function, he showed, must have an infinite value somewhere (perhaps at $z = \infty$) unless it is a constant. He deduced (by looking at the reciprocal) that a non-constant function must take every value at least once, and a little later "established" that a single-valued function takes every value. He also showed that a single-valued function is specified (up to a constant) once one is given functions which become infinite at each of its points of discontinuity just as it does. He characterised polynomials and rational functions in terms of their zeros and poles (including their behaviour at infinity), and established the principle of the argument. He then argued that a function $\varphi(z)$ is known up to a constant once the position of its zeros and infinities are known along with the order of the infinities. He argued that if a_1 is one of these points, then the function is of the form $\varphi(z) = (z - a_1)^{n_1} \psi(z)$, where n_1 is a suitable positive or negative integer and the function $\psi(z)$ is now finite at a_1 but otherwise has the same zeros and infinities as φ . He offered the usual argument for coping with $z = \infty$ and ended up, as one would expect, with an expression for φ as a quotient of two infinite products. In the absence of any discussion of convergence or the topological nature of the point set formed by the zeros and infinities, this cannot even be called a foreshadowing of the Mittag-Leffler representation theorem, but it is eloquent about the state of function theory at the time.

In Section VIII he established the Cauchy residue theorem, before embarking in the concluding sections on a discussion of simply and multiply connected surfaces and moduli of periodicity. This was the least satisfactory part of the book, and Durège was to re-write it twice in later editions. A number of applications are given to the evaluation of real integrals (Fig. 10.1).

Such a treatment invites many responses. It is certainly a bold attempt to convey to beginners something of Riemann's theory, including the difficult notion of the Riemann surface of a many-valued function. Algebraic functions and the example of the logarithm function are stressed throughout. The main achievements of Cauchy are described: the integral and residue theorems, the maximum modulus principle, and the existence of Taylor series for complex functions. The order of presentation reflects the author's sense of what is important in the subject. Roch reviewed it for *Zeitschrift der Mathematik* in 1865, and this part was extracted and printed at the end of Hesse's *Vier Vorlesungen*: "I would like to recommend the book by Durège to all beginners. I would therefore also recommend the book as a preliminary to everyone who wishes to become familiar with the Riemann's works". For a time Teubner editions carried an endorsement of it by Fuchs. Houël, in his review of

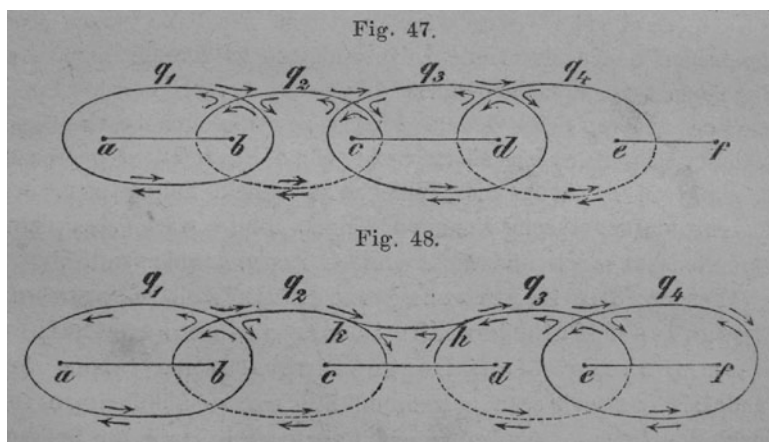


Fig. 10.1 A Riemann surface (in Durège *Elemente der Theorie der Functionen einer complexen veränderlichen Grösse* 1864, Figs. 47 and 48)

the second edition, praised the first edition for providing with clarity and for the first time “a methodical exposition of Cauchy’s doctrines, disembarrassed of certain useless complications that had been left by their inventor, and completed by the discoveries of Riemann”. The only change he noted in the second edition was an improved account of the connectivity of a Riemann surface.

And yet, something is wrong. The exponential function is never zero, but it is claimed that every single-valued function takes every value. Durège even gave this example, when he studied the behaviour of $e^{1/z}$ at $z = 0$. He distinguished between two kinds of discontinuity that a function f could have at a point $z = a$: polar ones at which the function f became infinite for every value of the approach of z to a ; and non-polar ones when the value taken depended on the approach. The former case is that where the reciprocal of the function takes the value 0 at $z = a$. The function $e^{1/z}$ has a non-polar singularity at $z = 0$, and Durège showed how any pre-assigned value can be attained for the limit as z tends to 0 along a suitably defined Archimedean spiral. The problem is not that his approach breaks down when the pre-assigned value is zero, although it does. The problem is that Durège wished to speak of a value of the function at the singular point, indeed, he wished to allow functions to take every value simultaneously at a non-polar singularity.

Such talk bedevils his proof of Liouville’s theorem (not so named by Durège) when he spoke of the function remaining finite round the circumference of the infinitely great circle. Once he has obtained the principle of the argument, it accounts for his curious statement that a function takes every value equally often. It was by no means peculiar to Durège. We have seen it earlier, in the writings of Briot and Bouquet. It was to be endorsed by Roch (1865b), in his review of Durège’s book for Schlömilch’s *Zeitschrift*. Roch quoted Weierstrass’s view, that functions that cannot be given even an infinite value at a point demonstrate the falsity of the theorem in

question. He replied that one could just as well argue that the theorem was true when more properly understood. In the case at hand, the function $e^{1/z}$ took the values zero and infinity at $z = 0$ which it took nowhere else, thus saving the theorem, once one understood the values a function took at such points of discontinuity.

Osgood, writing in 1896 (Osgood 1896), picked up another problem with Durège's book, one that was indeed less grave in the first edition than in the later ones. This concerns Riemann's theorem on removable singularities. If a complex function is given everywhere in a domain except the point $z = a$, and if it does not become infinite there, then it is either analytic at the point $z = a$, or it can be made so by assigning it the value that $f(z)$ approaches as z approaches a . Durège's error lay in assuming without proof that the theorem is true in the special case when the function takes the value zero at the point $z = a$. Osgood speculated that the source of the error was Roch's published version of Riemann's theory (Roch 1863a, 187), where the truth of Riemann's accidental singularity theorem seemed to Osgood to have been taken for granted. Durège himself seems to have been unaware of these defects, for significant changes introduced into the second edition (1873) only concern the topological study of a Riemann surface; upon which Durège also published a short note the next year (Durège 1874).

The third edition differs from the two previous ones in containing a new section devoted to explaining how difficulties with Riemann's approach can be overcome. In 1896 a posthumous American translation of the fourth edition appeared, written and published by G.E. Fisher and I.J. Schwatt of Pennsylvania.

In 1906 Ludwig Maurer brought out the fifth edition. He recognised the powerful expression Durège had given of Riemann's ideas in the first edition but noted that the small improvements made to the book even by the 4th edition left it lacking any account of modern function theory. He therefore added a chapter at the start introducing the basic processes of analysis, but decided, nonetheless, to preserve the Riemannian emphasis of the book in other ways. Maurer acknowledged the help of Brill in preparing this new edition, and the influence of Christoffel's lectures, and these may also have inclined him to keep the book as Durège had intended it. Brill's influence is surely visible in Maurer's decision to rework the account of algebraic functions, turning it towards the study of the functions and their Riemann surfaces and away from their integrals. Instead, he offered a new chapter on second order linear differential equations, on the grounds that it was interesting, led on to the study of automorphic functions, and allowed him to develop general function-theoretic principles.

A year after Durège's first account appeared, the first edition of Carl Neumann's *Vorlesungen über Riemann's Theorie der Abel'schen Integrale* appeared. Such was to be the impact of the second, enlarged and improved, edition of 1884 that one suspects the first edition did not do too well. In the preface to the second edition, Neumann tells us that the revisions strengthened the theory by insisting that all functions have continuous first derivatives. Thus he explicitly insisted that a holomorphic function $f(z)$ have a continuous derivative. He also extended the theory by dealing with all algebraic functions, not merely hyperelliptic ones. Hölder, in his

obituary of Neumann (Hölder 1926, 9), observed that although the book is called lectures

The lectures were never given as such. So much greater was the effect of the published book. The enormous value of this work lies in the fact that through it the majority of mathematicians were first enabled to understand Riemann's sphere of ideas. While with Riemann the many-valuedness of the surface representing a function is only rather broadly depicted as a many-sheeted (*Gebilde*) with branch points lying over the complex plane, Neumann started from the cut plane pieces of surface, which have clear and simple boundaries that can be put together easily and so genuinely construct the Riemann surface. The connectedness of such a many-leaved branched surface is therefore understood not merely, as in Riemann's way, by cuts, but rather also through continuous reshaping.

In both editions, Neumann defined a function $f(z)$ to be holomorphic if it satisfied the equation $\frac{\partial f}{\partial x} + i \frac{\partial f}{\partial y} = 0$, and deduced that such a function satisfies the Cauchy–Riemann equations. His term for holomorphic was *monogene*, taken over from Cauchy, and treated as a German word (compounds like “nichtmonogene” appear). He then established the Cauchy integral theorem (via Green's theorem), the Cauchy formula, and the Cauchy residue theorem. Liouville's theorem was derived from the Cauchy formula, but Liouville's name was not mentioned. By successively differentiating Cauchy's formula, all derivatives of a holomorphic function were shown to exist, and the Taylor series expansion of a function obtained. Convergence was established by a majorising argument. The Laurent series expansion was then obtained. Consideration of the value of the derivatives, regarded as the coefficients in the Taylor series expansion, then showed that a function which is locally constant is globally constant.

The convenient term “pole” was introduced by Neumann (p. 38). He defined it as a point at which the function f is not continuous but the reciprocal function $\frac{1}{f}$ is continuous. The function, he explained, may therefore be said to take the value ∞ at a pole. After discussing the concept of the order of a zero or a pole, he too established the principle of the argument. Neumann then gave the familiar characterisation of rational functions. Thereafter, he turned towards the theory of Riemann surfaces, starting with the Riemann sphere.

Oskar Schlömilch, who founded and edited the *Zeitschrift für Mathematik und Physik* in 1856, was also an energetic writer. His 538-page *Vorlesungen* of 1866 contains enough material on the subject to count as only the fourth book on complex function theory to be published, and it seems to have been very successful. Pages 35–111 cover functions of a complex variable, and further chapters look at elliptic integrals and elliptic functions.

His definition of a complex differential quotient and so of a complex function of a complex variable is Riemann's. He then turned to the Cauchy integral theorem, which he attributed to Cauchy (1825a) but established with the “more elegant and rigorous proof” that he found in Riemann's (1851). He built up to the residue theorem via numerous examples, many of which he took from Cauchy's *Mémoire sur les intégrals définies*, and then derived the MacLaurin series of an analytic function from the residue theorem on p. 86 and discussed Lagrange's series. His choice of sources is interesting as much for what it excludes as well as what it

includes—no Briot and Bouquet, no Durège—and it helped him present a theory of elliptic functions that was remarkably geometric.

He defined elliptic functions via elliptic integrals. These he first discussed, confining himself to the case where the modulus is real, for 81 pages on their elementary substitution theory, the addition theorems, and their use in rectifying ellipses, hyperbolas, and the lemniscate. When he came to the elliptic functions themselves, he took due note of the fact that the integrand is two-valued and branched at four points and so the value of the integral depends on the path and showed that the periods arise as integrals around a branch point. He depicted the double periodicity of the three Jacobian elliptic functions via lattices, and gave Jacobi's geometric construction for their addition formula via Poncelet's closure theorem (with the history sufficiently correct in a footnote). He then derived power series expansions for three functions from the system of first-order differential equations they define, attributing the result to Jacobi but noting that Jacobi had not established the convergence of the series. He then gave a derivation of the partial fraction expansion of the three functions that he attributed to Weierstrass. Other results concerning their expansions in Fourier series and as infinite products he attributed to Jacobi and Beltrami, adding that he had done this earlier himself "by a method not completely free of objections because the theory of functions of a complex variable had not attained its present state of development".

The book gives a rather attractive picture of Schlömilch as a teacher, because it is a mixture of careful exposition, examples, and general theory that moves quickly but effectively from an elementary starting point to a position of some depth. The material on elliptic functions was standard, but the clear connection to the two-valued integrands fresh and the treatment of the now-underlying complex function theory new, and it is not at all surprising that it did well. Indeed, in his review of the fourth edition, 1895, in *Fortschritte*, 26.0299.01, Lampe noted that because the book fulfilled its stated purpose it had met with great applause in some circles. Schlömilch, he said, had made a fortunate choice of topics among those necessary for technicians, and expertly adapted them to his audience, adding that the material was in fact so rich that it could be recommended for many studying mathematics, while it was also useful as a handbook.

These books point to what was to become the important divide in the appreciation of Riemann's work and the way it could be handled at textbook level. There are what might be called the elementary, the problematic, and the advanced aspects of it. The elementary aspect includes the definition of a complex differentiable function, the Cauchy–Riemann equations, the conformal character of a complex function, and the fact that the real and imaginary parts of a complex function are harmonic. This leads, of course, to the problematic part: the need to solve the Dirichlet problem. Finally, the advanced aspects include all but the simplest parts of the study of branch-points, many-valued functions, "Riemann surfaces" and complex algebraic curves, and the study of elliptic—and still more Abelian—functions. We shall see that the elementary aspects could be absorbed, the problematic aspect finessed, and the advanced aspects safely consigned to specialist, higher-level courses.

10.3 Seven Textbooks Between 1866 and 1880

The four just discussed are the only books on the subject published between the deaths of Cauchy and Riemann. There followed a period marked by the first publication of Riemann's *Werke* and the early lecture courses of Weierstrass, and which we may end a little arbitrarily in 1880 with the arrival on the scene of Picard and Poincaré. The *Werke* made Riemann's thesis of 1851 accessible for the first time, as well as many of his hitherto unpublished works. It reached France and was translated into French in 1898.

Weierstrass's lecture courses circulated among his present and former students but were never published in any form until recently. But some of his important discoveries did get published, for example the Casorati–Weierstrass theorem. Weierstrass's insistence on the importance of a sharp distinction between finite poles and essential singularities was also promulgated in the work of his students, for example in Fuchs's papers on linear ordinary differential equations. In Berlin, these ideas belonged to a theory of the natural boundary of a complex function. By 1880, with the attention of Klein moving more and more towards function theory, and with Poincaré and Picard in France ready to adopt the ideas of Riemann, the teaching of the subject entered a new phase.

There were seven significant works published between 1866 and 1880: Casorati's *Teorica delle funzioni di variabili complesse* of 1868, J.A. Serret *Cours de calcul différentiel et intégral*, also of 1868, the second volume of Bertrand's *Traité de calcul différentiel et de calcul intégral* of 1870, Thomae's *Abriss einer Theorie der Functionen einer complexen Veränderlichen und die Thetafunctionen*, also of 1870, Koenigsberger's *Vorlesungen über die Theorie der elliptischen Functionen nebst einer Einleitung in die allgemeine Functionlehre* of 1874, Hoüel's *Cours de calcul infinitesimal* of 1878–1881. In addition, in 1875 there was the second edition of the book by Briot and Bouquet, now called *Théorie des fonctions elliptiques*, which differs so completely from the first that it must count as a new book.³

Casorati was one of the most important complex analysts of his generation, and his book is not only authoritative, but it also contains a remarkable historical account of the origins of the subject to which we have several times referred. It amounts to 143 pages and provides a detailed, updated picture of the subject from its origins up to the most recent developments by Riemann and Weierstrass. By writing such a thing Casorati was seeking to strengthen the claims for complex analysis as a major new branch of mathematics. For personal as well as mathematical reasons, Casorati advanced the claims of Riemannian analysis in particular; Klein (1926–1927, 1, 274) was to hail the book as a fine exposition of complex function theory from the Riemannian point of view.

³We pass over Hattendorff's *Algebraische Analysis*, 1877, because, as its title suggests, it is about the familiar functions and their power series expansions, although with suitable attention to rigour; it does not enter into complex function *theory*.

Casorati devoted the first section of his book to the concept of number and its extension to complex numbers, including their geometrical representation. After introducing the functions of one and several real variables, and explaining that a theory of complex functions of a real variable x —the one that Cauchy had developed in his *Cours* in 1821—were nothing else than a theory of real functions of one real variable, Casorati turned to complex functions proper. He adopted Riemann's definition of a complex function in preference to Cauchy's, which he found too broad. He explained that a complex function is generally conformal and provided a classification of such functions according to their analytical representation. Accordingly, he discussed their expansion in infinite series and products and, after a lengthy discussion with many examples, worked round to establishing the Cauchy integral theorem (giving the calculus of variations proof). He proved it first for simply connected regions, then (pp. 368–384) for multiply connected regions resorting to suitable cuts and following the proof Riemann had given in his 1851 Dissertation. Then he established the Cauchy formula, and using these results he proved the principle of the argument and showed that a holomorphic function is analytic by obtaining the power series expansion of a holomorphic function and the Laurent series for a function holomorphic in an annulus. Eventually, he studied the behaviour of a function in the neighbourhood of isolated points, and established the Casorati–Weierstrass theorem (p. 434–435).

Sadly, a projected second volume which would have contained the more advanced topics of the day (applications to elliptic and Abelian functions) was never written. In his review of Casorati's book [Beltrami \(1869\)](#) emphasised the importance of the subject that was becoming “the lofty summit of the great tree of the mathematical sciences”. To support his claim he referred to Gauss' authority and gave a detailed account of Gauss's views about complex quantities. Turning to Casorati's book proper, Beltrami pointed to its even-handed appreciation of Cauchy's and Riemann's “schools” as one of its main merits. Commenting on Riemann's approach to the concept of a function, Beltrami emphasised that Casorati demonstrated its importance mainly in the historical part of his book and added that Riemann's point of view “seems to be surprisingly close to the one of modern geometry where the properties of figures are established as a consequence of few characteristic data, without appealing to analytic equations” ([Beltrami 1869](#), 37).

J.A. Serret claimed that his *Cours* (first edition 1868) reproduced the substance of the lectures he had given each year at Sorbonne, and indeed it differs little from his additions to Lacroix's *Traité*.⁴ In Volume 1, Ch XI, he discussed functions of imaginary variables, drawing examples from algebraic functions, series with imaginary terms, and especially the logarithmic, exponential and trigonometric functions. He discussed the absolute convergence of power series (which he showed implied their convergence) but ignored the theory of uniform convergence. The direct functions he defined via convergent power series, the inverses as their inverses, noting that they were not single-valued. A complex function was said to

⁴A successful book, Serret's *Cours* ran to six editions by 1911.

be differentiable if the derivative was independent of direction, so the usual rules applied.

An unusual feature was his use of Cauchy's proof (see 3.5.1) of the Cauchy integral theorem via the concept of the mean value of a function, defined as follows. Given $z = \rho e^{i\theta}$ fix ρ , and choose n equally spaced values of θ from $-\pi$ to π : $z_0, \dots, z_{n-1}$. Then the mean value of the function f corresponding to the modulus ρ is defined to be

$$M(f(z)) = \lim_{n \rightarrow \infty} \frac{f(z_0) + \dots + f(z_{n-1})}{n}.$$

For example, $M(z^\mu) = 0$, $\mu \neq 0$. Cauchy's theorem asserted that if the function f is continuous for all z with $|z| = \rho < R$, then for $\rho = R$, $M(f(z)) = 0$. The proof followed quickly from the Cauchy–Riemann equations in polar coordinates. As a corollary, if Z denotes the value of z when $\rho = R$ and $0 < |x| < R$, then $F(x) = M\left(\frac{Z}{Z-x}F(Z)\right)$, because F satisfies the terms of the theorem. Then applying the theorem to the function f defined as $\frac{F(z)-F(x)}{z-x}$, ($z \neq x$) and $F'(x)$ when $z = x$, shows that $M(Zf) = 0$. So, from the familiar power series for $\frac{Z}{Z-x}$, one deduces that $F(x) = M\left(\frac{Z}{Z-x}F(Z)\right)$. The corollary follows that

$$F^\mu(x) = 1.2 \dots \mu.M\left(\frac{Z}{(Z-x)^{\mu+1}}F(Z)\right).$$

The existence of convergent power series expansions follows. Serret then derived the Lagrange inversion formula, and applied it to $z = x + tz^m$.

In Part 2, Chap. 2, Serret gave a theory of definite integrals, the passage from the real to the imaginary, and Cauchy's formula, finally commenting that the formula for the integral round a circle does not differ at all from that of involving the mean value function, from which series expansions were deduced. The book also contained a description of the periods of elliptic integrals, the Gamma function, and differential equations, including some partial differential equations, before turning to geometry.

Bertrand, then 50, was, as the title page of his book tells us, a professor at both the École Polytechnique and the Collège de France and a member of the Institut. Four years later he was to become the permanent secretary of the Académie, a position he occupied with success for 26 years. He belonged to a distinguished mathematical dynasty that included Olinde Rodrigues, Paul Appell, Charles Hermite, and Emile Picard. Indeed, as Zerner ably described in (Zerner 1991), Bertrand's original mathematics was not as important as his influence behind the scenes, and his book was regarded (for example, by Darboux (see Gispert (1987, 103) as Schwarz (see 6.5.5) as sometimes lacking in rigour but with perfect simplicity and lucidity.

The first volume of the *Traité* treats complex function theory in Chap. XI for a mere 40 pages (pp. 291–330), as part of a discussion of its account of integration: how integrals can be differentiated, used to define functions, and evaluated approximately. So the emphasis in the chapter on the integrals of complex functions is on their use in evaluating real integrals, much as Cauchy's had been many years

before. The Cauchy integral theorem is stated and proved by a variational argument invoking the Cauchy–Riemann equations. But the Cauchy formula is not given explicitly, and only emerges from a general discussion about the evaluation of the integral of $(z-a)^k \phi(z)$ when $\phi(z)$ can be expanded in a power series. The principle of the argument is, however, proved, and used to prove the fundamental theorem of algebra.

Bertrand did confront the question of many-valued functions. He acknowledged that the ideas of Cauchy, as developed by Puiseux, were clear and satisfactory. But, he said, the approach of Riemann (“a geometer of rare merit, too soon taken from science”, p. 321) was to be preferred. Therefore he gave a brief outline of them, explaining the idea of a Riemann surface with reference to such many-valued functions as $\sqrt{(z-a)(z-b)(z-c)}$ and $\sqrt[3]{\frac{(z-a)}{(z-b)}}$. Bertrand explained carefully how to ensure that a contour drawn on a Riemann surface was indeed closed. The third volume is given over to an extensive treatment of elliptic functions which from time to time drew on these ideas again. Despite its brevity, Bertrand’s account seems to be the first one in French to present the merits of the idea of a Riemann surface. Darboux, in his review of the book in his *Bulletin des sciences mathématiques* vol 1, commented (on p. 42) that, because of Bertrand’s habitual clarity of exposition

We have therefore succeeded for the first time in properly understanding the new and very difficult system of representation due to Riemann. The rapid account that M. Bertrand has given will have the merit, if Riemann’s method is not to be abandoned, of leading several mathematicians to study the beautiful memoirs written by that *savant* whose premature death is deplored by science and who appears to have been as remarkable for the agreeable qualities of his heart as for the distinction of his mind.

Thomae’s *Abriss* (1870) is a Riemannian treatment. A complex function is defined via the Cauchy–Riemann equations and said to be conformal whenever the derivative does not vanish. The idea of a complex integral along a path is explained, the Cauchy integral theorem and formula proved, and the Taylor and Laurent series are obtained. The novelty of the book is a turn to the theory of Fourier series expansions of given functions. Thomae regarded a real periodic function as a function, $h(\phi)$, on the boundary of a circular disc, and treated it as the limit as r tended to 1 of a function $H(r \cos \phi, r \sin \phi)$. In this way he obtained results about the existence and uniqueness of convergent Fourier series representations of functions under various assumptions about the nature of their discontinuity at some points in their domain of definition.⁵ However, this material was dropped in the second edition, 1873, in favour of an extended discussion of discontinuities in functions of one or two variables.

After this excursion, Thomae returned to the complex story, characterised polynomials and rational functions by their zeros and poles, gave partial fraction and product expansions, and concluded the first half of his book with a Riemannian account of some multi-valued functions. His examples led naturally to the theory of

⁵These results are similar to some in (Heine 1870).

elliptic functions, which occupied the second half of his book. Here Thomae used the Riemann surface of a function of the form $y^2 = (1 - x^2)(1 - k^2x^2)$ (which he showed was the typical example) to derive Legendre's three types of elliptic integral, the theory of theta functions and the (Jacobian) elliptic functions.

Despite its title, slightly more than half of Koenigsberger's book is an introduction to function theory, and it is presented in a heavily Riemannian spirit. The complex numbers are defined and represented geometrically; a complex function f of a complex variable z is defined as one that has a derivative independent of the direction of dz , and the Cauchy–Riemann equations are derived (but not so called, in fact Cauchy's name is often missing from the book). The author claimed, incorrectly, that the Cauchy–Riemann equations are necessary and sufficient for a function to be a complex function. The conformality of a function, away from points where $f(z) = 0$, is proved.

At this very early stage, Koenigsberger confronted the multi-valued nature of such expressions as $\sqrt[n]{z}$. He offered a preliminary distinction between two kinds of discontinuity of a function: the first kind where the reciprocal of the function vanished, and the second kind when it took different values if approached from different directions (his example is $e^{1/z}$). This former student of Weierstrass had evidently not appreciated the introduction of the concept of an essential singularity and the meaning of the Casorati–Weierstrass theorem (as Schwarz and Weierstrass were to notice, see 6.6). He then defined a branch point of a function as a point which can be surrounded by a closed curve containing no other singular points of the function, such that analytic continuation around this curve returns a different value of the function. Koenigsberger briefly discussed how multi-valued functions can be handled by suitably cutting the Riemann sphere from each branch point to infinity, which he disliked because it obstructed the free movement of the variable. It was on these grounds, he said, that Riemann had introduced the Riemann surface of a multi-valued function. Koenigsberger showed that on the Riemann surface the original function becomes single-valued and discussed how the leaves may be interchanged at a branch point, with this example, $w = (z - a) \left(\sqrt[m]{\frac{z-b}{z}} \right) + \sqrt[n]{z-c}$. He then defined a simply connected Riemann surface and explained how to render any Riemann surface simply connected by a system of cuts and proved that the number of cuts was well defined. A surface is $n + 1$ -fold connected if n cuts can be made on it without disconnecting it, but any further cut would disconnect it. Some pages later he showed that n is even on a closed (ie, boundaryless) surface.

Only now did Koenigsberger turn to the study of integrals of complex functions. He proved a version of Green's theorem for real functions of two variables whose first partial derivatives may become infinite in mild ways at isolated points. He deduced, using the Cauchy–Riemann equations, that the integral of a complex function around a closed boundary curve containing no singular points, must vanish. This allowed him to explain when the integral between two points of a complex function in a simply connected region is independent of the path.

The Cauchy integral theorem followed, and the derivation of the Taylor and MacLaurin series for an analytic function, with an investigation of their convergence

(as far as the nearest singular point). Simultaneously, Koenigsberger obtained explicit conformal representations of certain domains on others (a simple example being of the half-plane on a disc). The series expansions are regarded as special cases of Laurent series expansions (not so called), and the Riemann mapping theorem gets mentioned in a footnote to show the range of the theory. Its proof, however, is not given because the generality is not needed. The Cauchy integral theorem is also used to show that a single-valued analytic function defined on the whole Riemann sphere must be infinite somewhere if it is not constant and therefore takes every value. Likewise n -valued functions on the whole Riemann sphere must take the value infinity somewhere and, by considering the behaviour of the symmetric functions of these n values, Koenigsberger showed that every n -valued function is algebraic. The same integral theorem also allowed him to prove the theorem on removable singularities and refine his discussion of finite poles and branch points by defining their order; he also refined his error about essential singularities, at which, he now claimed, the function takes every value. He could then state and prove the theorem on the enumeration of zeros and poles inside a simply connected region.

In keeping with his thorough-going Riemannian spirit, he then outlined the theory of integrals of multi-valued functions. He devoted a chapter to algebraic functions, stating that it was necessary and sufficient for a function to be algebraic that it takes finitely many values and is only infinite (to a finite order) at finitely many points. Indeed, if it takes n values and if it takes the value ∞ at m points, then it can be described by a polynomial function of degree n in u and m in z . The question of the power series expansion of branches of an algebraic function was then discussed at length—the Newton–Puiseux theory but without mention of either and with no polygon in sight. The example of $f(z, w) = aw^6 + bw^4z^3 + cw^3 + dz^7$ was treated completely.

Two chapters on the logarithmic and exponential functions, trigonometric and elliptic integrals, follow. The logarithm is defined via the usual integral (an integral of a rational function, Koenigsberger noted) and the exponential function is its inverse. The standard power series are derived, and the new functions are used to map various regions conformally onto others. At this stage the trigonometric and elliptic integrals are treated as special cases of hyperelliptic integrals. The introduction ends with a quick discussion of the infinite product representation of $\frac{\sin \pi z}{\pi z}$.

Hoüel is an interesting figure. He may be unique among French mathematicians in preferring a position in Bordeaux to one in Paris. He was eager to introduce the best of foreign mathematics to a French scene he found in need of serious reform, and to this end he translated the important papers of Lobachevskii, Bolyai, Riemann and Beltrami. His friend Darboux wisely placed him on the editorial board of his *Bulletin des sciences mathématiques*, and Darboux's correspondence with him makes interesting reading on many topics (see [Gispert \(1987\)](#)). His breadth of reading is apparent in his four-volume *Cours*, and if it, like Bertrand's work, lacks rigour at times it too has other merits.

In volume I, § 156, Hoüel had written rather unhelpfully that an analytic function is one that is rigorously definable by analysis! But the discussion of complex function theory in the third volume is much better: a complex function w is analytic if $\frac{\partial w}{\partial x} + i \frac{\partial w}{\partial y} = 0$ (§ 1093). It follows that a complex function satisfies the Cauchy–Riemann equations (§ 1099), and that a complex function is conformal. Hoüel described this in traditional terms as “similarity in smallest parts is preserved” (§ 1105) but did not point out that this fails when $\frac{dw}{dz}$ vanishes. He did observe that a complex function may fail to be finite and continuous in two different ways (§ 1107). If the limit of $\frac{1}{w(z)}$ existed as $z \rightarrow c$ along whatever path and was zero, then Hoüel said the function $w(z)$ had a discontinuity of the first kind and that it could be said to infinite at the point $z = c$. But if the limiting value of both the function and its reciprocal depended on the path, as was the case for $e^{1/(z-c)}$ then Hoüel said the function was discontinuous of the second kind. He did not call this an essential singularity and explicitly rejected the term “pole” for a discontinuity of the first kind, which he attributed to Neumann, because it was already in too much use.

A brisk account of the Riemann sphere and stereographic projection followed (§ 1111). Then he established the Cauchy integral theorem by a Green’s function argument (§ 1124), the Cauchy formula (§ 1133), and deduced that all derivatives of an analytic function exist. Then he considered a function defined and analytic in the entire plane, and deduced first Liouville’s theorem (§ 1137) and then that every such function must be both zero and infinite somewhere (§ 1138). It must be presumed that the entire plane (“toute l’étendu du plan”) embraced the point at infinity; Hoüel did not think to repeat his earlier example of $e^{1/(z-c)}$. Familiar theorems then followed: the Taylor series of a complex function exists and is convergent out to nearest singular point (§ 1141); whence the constancy theorem; the Laurent series expansion; and the characterisation of rational functions (§ 1160).

The fourth volume gave some of the familiar applications of this material to the evaluation of definite real integrals and then gave a lengthy treatment of the circular and elliptic functions from the standpoint of function theory.

The second edition of the book by Briot and Bouquet differs so completely from the first that even they attempted no comparison. Their first small book has grown into a sumptuous quarto by 1875 of 700 pages, marked by a new title, *Théorie des fonctions elliptiques*. A complex function is one that has a derivative independent of direction, and hence satisfies the Cauchy–Riemann equations. Such a function may be single-valued (monotrope) or multi-valued (polytrope), but if it is continuous, monotrope and has a derivative everywhere in some domain it is now said to be holomorphic there, and meromorphic if it has poles. The Riemann sphere is introduced, and stereographic projection onto the plane. The first examples are polynomial functions, and Cauchy’s proof of the fundamental theorem of algebra by studying the variation of the argument is given. Then algebraic functions are studied. Puiseux’s theory is set out, and then the Clebsch–Gordan theory of fundamental loops. Indeed, this is the one work that they acknowledge as having influenced them;

there was no direct influence from Riemann. Eight examples are treated at length, including the complex folium of Descartes.

In the second chapter emphasis switches to functions defined by power series. They are shown to be differentiable term by term within their circle of convergence, and indeed to be holomorphic. The circular and trigonometric functions and their inverses are defined as power series; theta functions are defined by double series, carefully handled. Elliptic functions are now introduced with reference to their lattice of periods. Then the integral of a complex function is defined, and Cauchy's theorem that the integral of a holomorphic function in a simply connected domain is independent of the path is proved by varying the contour, differentiating under the integral sign, and using the Cauchy–Riemann equations. Specific, one might say standard, real integrals are then evaluated: those of

$$e^{-z^2}, e^{-z}, z^{n-1}, \frac{z^{n-1}}{1+z} \text{ and } \frac{e^{azi}}{b^2+z^2}.$$

The idea of a period of a multi-valued integral is defined, and explicit examples of these are evaluated later. For example, the periods on the folium of Descartes all vanish (as they must; it is a rational curve).

In Chap. 4, on general theory, it is shown that if all the derivatives of a function defined on a connected domain vanish then the function is constant, and constancy on a curve, no matter how small, likewise implies constancy in the domain. It follows that not every derivative of a meromorphic function can vanish. It is shown that a theta function has a unique zero in its parallelogram.

More than half the book is given over to elliptic functions and elliptic integrals, their transformation theory, and the addition and multiplication, and division theory for the arguments of elliptic functions. From the theory of the modular equation they obtain Hermite's solution of the quintic equation (no mention of other authors). The book ends with a chapter on Abelian integrals and Abel's theorem. In his review of the first part of the book (Darboux 1874, 69) looked forward to a complete and classical *Traité* that would be “a veritable monument, advantageously replacing the ancient work of Legendre”.

10.4 The German Textbook Tradition

The next phase is marked by a run of German books that reflect the situation when complex function theory had become an established part of the syllabus. It is not so interesting to list their contents as to compare them and to note any particularly interesting features. We can then compare the German view (or views) with the contemporary French one. The German books concerned are: Thomae, *Elementare Theorie der analytischen Functionen einer complexen Veränderlichen*, 1880 (2nd. ed. 1898); Lipschitz *Differential- und Integralrechnung*, 1880; Harnack's *Die Elemente der Differential- und Integralrechnung. Zur Einführung in das Studium*

dargestellt of 1881, and his much revised translation of Serret's book, *Lehrbuch der Differential- und Integralrechnung*, 1884; Otto Biermann *Theorie der analytischen Functionen*, 1887; Kronecker's lecture course, *Vorlesungen über Mathematik*, edited by Netto in 1894; Stolz *Grundzüge der Differential- und Integralrechnung. Zweiter Teil: Complexe Veränderliche und Functionen*, 1896, Burkhardt's two-volume *Funktionentheoretische Vorlesungen*, 1897–1899; and finally Fricke *Kurzgefasste Vorlesungen über verschiedene Gebiete der höheren Mathematik mit Berücksichtigung der Anwendungen. Analytisch-functionentheoretischer Teil*, 1900. We also consider Hilbert's unpublished lecture course of 1896/97.

As we have seen, the first German books were firmly Riemannian in intent. The next wave is more diverse and shows the profound split that was to develop between the Riemannian and the Weierstrassian approaches. The large and active school in Berlin was successful in producing their own vision of how complex function theory should be developed, but not, one notes, by writing it themselves. The task was left, more by accident than design, to former students.

In 1880 Thomae published the first edition of his account of the elementary theory of analytic functions. Thomae had studied at both Berlin and Göttingen before becoming a professor in Jena in 1879. His book was an avowedly Weierstrassian work, not least because, as he put it in the preface, although the idea that an approach based on the theory of power series and without appeal to the infinitesimal calculus was an old idea that had many merits, not least rigour, nonetheless no-one had written a book about it. The poor student had therefore to find out about it here and there in other works written with other aims in mind.

Just how much of the foundations of the infinitesimal calculus had nonetheless to be supplied is a moot point, however. Thomae gave two constructions of the irrational numbers, one based on sequences and another on Dedekind cuts, with a nod in the direction of Frege that famously did not save him from a mauling at the hands of his colleague in the small Mathematics group in Jena. He then defined the complex numbers algebraically. In the next chapter he set out the basic theorems on the convergence of series. Chapter 3 discussed continuity of functions, Chap. 4, among other things, the binomial theorem. Only in this chapter did the presumably more familiar concepts of polynomial algebra enter and Thomae turn to the theory of log, exp, and the trigonometric functions. Then came among other things the Laurent expansion (with, in the 1898 edition, Pringsheim's 1895 proof of convergence and uniqueness that Thomae had only been able to state with restrictions in the first edition). The book ended with the Weierstrass product formula, a discussion of the reversion of series, and, but only in the first edition, a short account of theta functions and elliptic functions.

Lüroth, in his review of Thomae's book (1881, 147–148) agreed that an elementary approach of this kind was worth trying, as did Stahl in his review for *Fortschritte* (12.0306.01). Lüroth welcomed the book, which, he said, was the first extensive treatment of the Weierstrassian approach, and he judged it to be a valuable. It was clear, elementary, and could be studied in a semester without difficulty. He noted that Thomae, in the strict Weierstrassian spirit, treated the subject without reference to the idea of integration, and even differentiation is defined only formally.

For the second edition, (1898), Thomae admitted that with the appearance of Biermann's edition of the lectures of Weierstrass (1887) it was possible to give different answers to the question of what constituted elementary analysis, but he continued to think that there were good reasons not to alter his own position. He also adopted a suggestion of Lüroth's and dropped the sketchy treatment of theta functions and doubly periodic functions. This edition was reviewed by Fricke (1899, 148–149), a representative, one might say, of the Göttingen mathematicians and a long-time collaborator with Klein. He too found the book clear, but wondered if there were not more students who would come to function theory after a course in the foundations of the differential and integral calculus, for whom Cauchy's methods would not seem more natural. And perhaps there would be some for whom an introduction that began with a chapter on number theory would not be to their taste. And he admitted that Weierstrass's ideas had been given new blood in the work of Hadamard, Borel, and others.

Lipschitz had graduated from Berlin in 1853 and taught at Bonn since 1864. The second volume of his (1877–1880), entitled *Differential- und Integralrechnung*, was based on his lecture courses on real and complex analysis and has a 100-page section on complex function theory. He defined a complex function and observed that it is generally conformal. He then defined the inverse of a complex function, and showed when it is analytic, so that he could discuss the exponential and logarithmic functions. This led him to consider winding points. After this he turned to integration, obtained the Cauchy integral theorem and from it the existence of Taylor series expansions, and a discussion of their convergence. A novel feature was his proof from a complex function theoretic point of view of Abel's theorem.

Harnack had been a student of Klein's in Leipzig. Parts two and four of his *Die Elemente der Differential- und Integralrechnung. Zur Einführung in das Studium dargestellt* (1881) are on complex function theory, and they are quite like Lipschitz's text. As one might expect from his background, the mixture is a little more geometric, with some material on Riemann surfaces and algebraic curves, and a number of historical footnotes. A complex function is one that has a derivative independent of $\frac{dy}{dx}$ and so satisfies the Cauchy–Riemann equations. It is generally conformal. Cauchy's proof of the fundamental theorem of algebra, in terms of the behaviour of the logarithm of the given polynomial is used, but the Cauchy integral theorem is there, proved via a Green's function argument, along with a discussion of singular points and a derivation of the Taylor series of an analytic function. When George Cathcart of Edinburgh proposed to translate the book, Harnack was enthusiastic, but stipulated that he would want to make several changes, which he offered to supply promptly. The plan was cut short by his unexpected death, in 1888 at the age of 37, and his friend Voss wrote to Cathcart to say that no extensive list of changes could be found among his papers. Instead, on the advice of Klein and Noether he thought it better that the work be translated as it stood, which, as the English preface makes clear, is what happened. Cathcart indeed thought it unlikely that Harnack could have carried through a plan of piecemeal revision. The English edition came out in 1891, testimony to the lack of such texts which we discuss further below.

Harnack's translation of Serret's *Cours* (1884) seems to have done well. It ran to three editions: the second, 1899, was thoroughly revised with the assistance of Liebmann and Zermelo; the third edition was edited by G. Scheffers in 1906. In the first edition Harnack praised the French original for its insights into the manifold nature of real functions, of which the complex represent only a special case. It is clear, however, that the book is very far from being a translation. The second edition dealt with functions of a complex variable in volume 2, Chap. 8, after an account in Volume 1, Chap. 11, of the differentiation of functions of a complex variable.

Harnack, rather than Serret, announced three aims to this chapter: the precise determination of the idea of function of a complex variable; the study of properties of these functions, and investigation of how far the ideas of an analytic function and a function of a complex variable agree. Complex functions were defined as functions that satisfy the Cauchy–Riemann equations, it was shown that the composition of two complex functions is a complex function, and that all higher derivatives exist. Single and many-valued functions were admitted, the example of the log function was given and branch points discussed. Then the treatment turned to the integrals of complex functions and the periodicity moduli of log, arcsin, and of normal elliptic integrals of the first kind were given. Then came the development in power series, using Cauchy's fundamental theorem. The proof of that theorem was the usual one, the existence of continuous second derivatives was assumed, the treatment via the mean value idea was entirely dropped. Applications were given to the evaluation of definite integrals using Cauchy's calculus of limits

Then the idea of an inverse function was introduced via a discussion of first order differential equations. The radius of convergence was investigated, and the functions log, arctan, arcsin, as well as elliptic functions with real and imaginary periods were considered. The Lagrange reversion of a series was then explained and applied to the trinomial equation $z = z_0 + wz^n$ and to spherical functions. The chapter ended with a brief look at how these results carry over to several variables. A function of several complex variables was defined to be complex function if it is a complex function in each variable separately. It was shown that the composition of such functions is again a complex function, a residue theorem was given and Cauchy's residue calculus was stated in a naive generalisation, but no discussion of the geometric limitations of this formal extension was provided.

Next, what might be called the Biermann affair. Otto Biermann was a Privatdozent in Prague when he published his *Theorie der analytischen Functionen*. He began his Preface by observing that at a time when Weierstrass's theory of analytic functions was proving its worth there was a palpable lack of a textbook suitable for students. He set himself the task of filling this gap, claiming moreover that the book appeared "With the permission of my revered teacher Weierstrass, to whom the plan of this work is known", but it may be doubted if he succeeded. In choosing a way through the material, he drew help from Pincherle's introduction (1880, 18). The first chapter presents Weierstrass's theory of irrational and complex numbers, following (Kossak 1872). Polynomial and rational functions in one and several variables are introduced, as are power series; the emphasis on several variables is heavily Weierstrassian and unusual in a textbook. The key property of uniform

convergence is explained, and properties of uniformly convergent series derived. Only now is the concept of a monogène analytic function defined, as a family of compatible convergent power series. Such a function is shown to be differentiable, and elementary examples are given (the logarithmic and trigonometric functions). Laurent's theorem is proved, followed by Mittag-Leffler's theorem. There is a chapter on doubly periodic functions and automorphic functions, and finally a chapter on analytic functions of several variables. In fact, as in Weierstrass's own lecture course, throughout the book much of the formal or algebraic side of the subject (the algebraic theory of polynomial and rational functions, and of convergence) is developed both for one and several variables.

As always in the strictly Weierstrassian tradition, Cauchy's theory of complex integration is never mentioned. Indeed, Cauchy's and Riemann's names are barely mentioned. Thus Biermann characterised rational functions as follows. He proved the fundamental theorem of algebra by showing that a polynomial function $f(z)$ is continuous in the plane, and on any finite disc the absolute value $|f(z)|$ attains a minimum. However, an analytic function, such as $f(z)$, satisfies the property that in any neighbourhood of any point z_1 there is a point $z_1 + h$ such that $|f(z_1 + h)| < |f(z_1)|$. The fundamental theorem of algebra follows from this contradiction; Biermann promised the reader a simpler proof shortly.

Among the fundamental properties of an analytic function that Biermann described in Chap. III are that every analytic function has at least one singular point, because every power series has a singular point somewhere on its circle of convergence. At a singular point a function may be infinite or fail to be defined. It follows that an analytic function cannot be defined and finite everywhere unless it is constant. This is Liouville's theorem, not given a name by Biermann. To prove that a function having only finitely many poles, even at infinity, is a rational function, Biermann, following Weierstrass, observed (p. 178) that the stated conditions exclude the possibility of the function having an essential singularity. The function elements are therefore of the form polynomial multiplied, perhaps, by an expression of the form $(z - z_0)^{-m}$. So the given function can be multiplied by a polynomial formed from the terms $(z - z_0)^{-m}$ to become a polynomial.

A few pages later (see p. 183), Biermann gave the simpler proof of the fundamental theorem of algebra that he had promised before. It is based on the idea that the reciprocal of an entire function $g(z)$ which does not vanish at any finite point is also an entire function, and the Casorati–Weierstrass theorem (for which Biermann cited Thomae (1880), but which he did not name). It implies that in any neighbourhood of $z = 0$ there are points at which an entire function has arbitrarily large modulus. Biermann argued by contradiction. If the polynomial $g(z)$ has no zero, then its reciprocal $\frac{1}{g(z)}$ is an entire function. Let M be an arbitrary positive constant. Then there is an $r > 0$, such that $|z| > r$ implies $|g(z)| > M$, and therefore there is no z' in $|z| > r$ such that $\left| \frac{1}{g(z')} \right| > \frac{1}{M}$. Readers were (and are) left to judge the elementary character of such a proof, which is due essentially to Weierstrass.

Biermann's book was something of a scandal almost from its inception, let alone its publication. He had not been, as his introductory remarks suggested, an actual

student of Weierstrass's. He based his text solely on copies of the lecture notes of Weierstrass's courses that were in circulation. His claim that Weierstrass knew, and by implication approved, of the plan of the book rested on a conversation with Weierstrass which Weierstrass was later to describe to Schwarz in angry terms (in a letter to Schwarz, June 12th, 1888). It seems that Biermann visited Weierstrass on a day close to Weierstrass's 70th birthday to inform him of his intentions, but Weierstrass tried to discourage him, saying that "he'd really set himself too difficult a task, one that I didn't even trust myself to resolve at that time. But because he repeated the proffered question, I told him in parting "If you've learned something from my lectures, I can't forbid you to make appropriate use of it' ". Weierstrass was particularly annoyed to discover that Biermann had not even attended his lecture courses: "Generating books this way can't be abided". Whatever difficulties Weierstrass may have experienced in presenting a polished course, and one suspects he was merely trying to be polite, were unlikely to have been those that caused Biermann trouble, for the book, as Hurwitz noted in his review (*Fortschritte*, 19.0361.01), was full of obscurities and false theorems that made it unsuitable for beginners, although it had some value for the experts in providing an overview.

Hurwitz, who had actually attended Weierstrass's lectures in 1877–1879, found faults in every chapter. The opening chapter required the corrections Pringsheim had already proposed to the account of infinite products in (*Pringsheim* 1889). The continuity of polynomial functions was not adequately proved. In Chap. 3 uniform continuity was not properly defined, and an incorrect theorem about two power series in several variables was adduced. The treatment of singular points of an analytic function was "thoroughly unclear". To quote one final criticism, the proof of Mittag-Leffler's theorem in Chap. 6 fails because preliminary theorems about products of infinitely many analytic functions have not been established with sufficient care.

As we have often remarked, in Weierstrassian complex analysis the integral is avoided. It is therefore interesting to compare this philosophy with the views of Kronecker on complex analysis. These were published after his death in an edition prepared by Eugen Netto, who drew on sets of lectures notes from the five courses Kronecker had given on single and multiple integrals between 1883 and his death in 1891. Netto had studied at Berlin as a student, and had been called to the faculty there in 1888. The book he produced offers a composite portrait of the subject, but one he hoped was true to as many as possible of Kronecker's own way of presenting the matter.

Complex functions are introduced in the third lecture. Kronecker had by then raised the question of exchanging the order of integration in a double integral. He showed that $\int dF$, the integral of a complete differential, taken round a rectangle vanished, and that it therefore vanished when taken around any triangle, and indeed along any closed curve that can be approximated by a curve made up of segments alternately parallel to the x - and y -axes. For this to be true he observed that the first and second derivatives of the function F must be finite and single-valued inside the curve. A complex function was one with a derivative independent of direction, so it satisfied the Cauchy–Riemann equations (not named by him, but written down

explicitly). It followed that if $f(z)$ is a complex function, then $f(z)dz$ is always locally a complete differential. Kronecker said that a point belonged to the natural boundary of a complex function if the necessary conditions just written down failed at that point. So he stated the Cauchy integral theorem for a function taken round its natural boundary. Standard arguments already used by Kronecker allowed the theorem to apply to suitable arbitrary boundaries.

An interesting historical note then followed. Kronecker observed that the Cauchy integral theorem had been published by Cauchy in 1814, although it had been known to Gauss since 1811. But because there is a great difference between publishing a result with a proof and an appreciation of its significance and merely confiding it to a friend, the theorem was rightly known as Cauchy's. However, Riemann had been the first to apply it to the more difficult parts of analysis and to obtain the first important results. Indeed, Kronecker continued, the new advances in analysis essentially follow from Cauchy's theorem. But this is not, as is usually said, because of the use of a complex variable, rather it follows purely and simply from the extension of functions of one variable to those with two. Analysis has not a mystical application of $\sqrt{-1}$ to thank for its truly significant successes of the last century, rather the entirely natural circumstance that one is infinitely freer to move in mathematics if one allows quantities to vary in a plane rather than along a line. Functions of one variable arise as limiting cases of those with two, and exactly at these limits, these banks, there are reefs beyond which lies the open sea (Kronecker 1894, 1, 52).

In keeping with this philosophy that analysis is one subject, real and complex, in one and several variables, other topics intervened before Kronecker returned to complex function theory in Lecture 10. The Cauchy formula was used implicitly to evaluate several familiar integrals before being explicitly written down (p. 167). Kronecker said of it:

It is, with the above $\int f(z)dz = 0$, of such importance that one can say without exaggeration that all of modern function theory is concentrated in these two integrals.

Kronecker then proceeded to obtain the Cauchy residue theorem, which, he said, had a surprisingly lovely aspect to it: an integral over an area was expressed as a sum, called by Cauchy the sum of the residues. Even though, he added, the idea of residues could be found in Jacobi's doctoral dissertation. The familiar formula for the number of zeros minus the number of poles was then obtained (Kronecker disdained the term "pole"). As applications, Kronecker showed first that an elliptic function has as many zeros as poles in any period parallelogram, and second that Liouville's theorem held. He stated it in a restricted and oddly muddled form: If a function and its derivative are everywhere finite and continuous and never become infinite either in the finite plane or at infinity, then the function is a constant.

Then "one of the principal applications of Cauchy's theorem" was obtained: the derivation of the Taylor series of a function. The insight was to apply the Cauchy residue theorem to the identity

$$\frac{1}{z - \zeta} = \sum_0^{n-1} \frac{\zeta^j}{z^{j+1}} + \frac{\zeta^n}{z^n} \frac{1}{z - \zeta}.$$

A brief indication of how these ideas could be extended to many-valued functions was then given. In the next lecture Kronecker dealt with the theory of theta functions in this spirit, but we shall turn aside.

It is not possible in a few sentences to indicate the vast extent of contemporary analysis that Kronecker managed to cram into these lectures. Netto admitted in his preface that Kronecker was incapable of being elementary, and his choice of topics, whether developed in detail or only outlined, could make his lectures seem esoteric. What is evident is that if, on the one hand, he refused to grant complex function theory a special status, on the other, he presented a series of theorems that was remarkably traditional. It is Weierstrass who, by comparison, appears as the man who had a vision of a new theory, in which complex functions of one or several variables were singled out and treated by newly created special methods. It was, of course, Weierstrass who drew the large audiences so often mentioned in any discussion of Berlin. But Kronecker drew his audience from a select group of the best students, those who often went on to careers in mathematics. So one may say that Berlin presented two faces to the next generation of German complex function theorists. How did they proceed?

The first book to be published after Kronecker's lecture courses had begun is the most polemical in its claims. This is the third edition of Thomae's *Abriss* (1890). In this book, Thomae was once again a strong advocate of the importance of Riemann's ideas for the study of algebraic functions. In his introductory remarks he characterised the difference between elementary function theory and function theory pure and simple in these terms. Elementary theory is the theory of power series, in which the successive derivatives of a function appear inside the coefficients of its Taylor series expansion. General function theory deals with derivatives as defined via a limiting process and obtains the power series expansions after some work. There is, however, a strong role for intuition, in the theorems to do with connectivity and Riemann surfaces. Thomae argued the case this way:

To those who might reject this and who would consider Riemann surfaces foreign to pure analysis and to be kept out of function theory, I must raise the question if they consider it possible today to have a complete insight into many-valued and in particular algebraic functions and their integrals with the eye of intuition closed and the *analytischen Gebilde* only considered with the eye of discursive thought (Thomae 1890, 2).

The book, which was aimed at the theory of theta functions, doubly periodic functions and elliptic integrals, then began, as the earlier editions had, by defining an analytic function to be one that satisfied the Cauchy–Riemann equations. Poles and residues were defined, and an analytic function shown to be generally conformal. Then complex integrals were introduced, Riemann's proof of Cauchy integral theorem given, the principle of the argument was proved, and the Laurent series obtained. The distinction between poles and essential singularities was described, and then, turning towards his main theme, where we shall leave him, Thomae

used Riemann surfaces to study doubly periodic functions, and proved Liouville's theorem (but only for elliptic functions). He also showed how to use the modular function to map circular arc triangles onto a half-plane. In an appendix he gave a proof of Picard's little theorem, the first time this had been presented in a textbook, and a ten-page list of useful formulae. Hurwitz liked this latter feature of the book highly and praised it highly (Hurwitz *Fortschritte*, 21.0384.01).

The book by the Austrian mathematician Otto Stolz (1896) is exactly what one would expect from this careful and well-read author: a precise mathematical account with many historical notes. Lampe's review in *Fortschritte* (27.0217.01) summarised the contents and added "The author has performed a great service by this careful presentation of the foundations". Stolz is the person who put Klein on to the works of von Staudt that contributed so firmly to the Erlangen Programm when they were together on Berlin and Göttingen, but it was Weierstrass who made the greatest impression on Stolz. Complex function theory occupies the second volume of his three-volume *Grundzüge der Differential- und Integralrechnung* of 1896. Rather as Riemann had done, Stolz first noted that there are maps from $\mathbb{C}$ to $\mathbb{C}$ but he then switched to the Weierstrassian side and selected out those that he called holomorphic at a point z_0 (borrowing the term from Briot and Bouquet (1875, 14)) as the ones that have a convergent power series expansion in some disc centred on z_0 . He then gave numerous examples, including the traditional elementary functions, rational functions, multi-valued expressions, and implicit functions. Roughly half way through the book he obtained the Cauchy integral theorem, by an argument about radii of convergence that Stolz was pleased to note was more rigorous in its discussion of the boundary than the arguments of Jordan and Pringsheim. From it, Stolz derived the Taylor series, Weierstrass's theorem that an analytic function cannot be analytic at every point on its circle of convergence, the Laurent series expansion, the principle of the argument, and results about Lagrange's series. Rather reasonably, he endorsed Kronecker's opinion that "The recent developments in analysis derive essentially from Cauchy's theorem".⁶

Burkhardt had studied in Munich and Berlin under Brill, Weierstrass, and Schwarz, but habilitated at Göttingen before going on to teach at Zurich and Munich. In the preface to his (1897), which was to reach its 4th edition in 1912, he lamented the fact that almost all the numerous German books were either one-sidedly Weierstrassian or Riemannian. It had been left to French and English authors (Picard, Forsyth, Harkness and Morley) to try to bridge the gulf that lay between the two methods. For himself, he would begin with the Riemannian geometrical way of presenting things, and then move to the more precise Weierstrassian presentation that "no-one can abandon who has opened their eyes in the school of Weierstrass". Thus was Thomae's question answered. Moreover, he went on, function theory as Riemann had presented it was well suited to the study of algebraic functions, but in the 40 years since it had appeared other important topics had come along. He singled

⁶Stolz (1896, vol. 2, 224), quoting Kronecker (1894, 1, 52).

out the work of Poincaré and Klein on automorphic function and linear differential equations, and proposed accordingly to teach the idea of fundamental domains.

The book itself started with many examples, before explaining the use of the Riemann sphere. A holomorphic function is, by definition, one that is complex differentiable; he added some remarks about the connection with harmonic functions and conformality. Then came a short chapter on real analysis. In the second and subsequent editions Burkhardt explained the Cauchy integral theorem with the proof by Goursat and Moore and used it to prove Weierstrass's theorem (a uniformly convergent series of analytic functions is analytic and can be differentiated term by term). Then came Taylor series expansions, poles, a proof of the fundamental theorem of algebra, the Cauchy formula, the principle of the argument, and the Mittag-Leffler decomposition theorem. There was a chapter on Riemann surfaces (angle being the motivating example), and then, for the first time in a German textbook, a chapter on singular points, natural boundaries, analytic continuation, and the reflection principle not, here, named for Schwarz). The third and fourth editions gave a full account of the conformal representation of a triangle on a half-plane, with reference to the work of Schwarz and Klein (on the icosahedron). Overall, the modifications Burkhardt introduced all strengthened the Riemannian side of the book, deliberately downplayed in the first edition. This may well suggest that he felt that mathematicians even in Germany were moving towards the approach of Cauchy and Riemann.

In his review, (Bôcher 1899), the German-educated Harvard mathematician Maxime Bôcher welcomed a book that brought together the three schools of function theory in such a successful way. With the exception of the chapter on real analysis, where, for example, the account of continuous curves was already falsified by the example of Peano's space-filling curve, he found it helpful and rigorous. He very much liked the proof of Weierstrass's theorem, which, he said, he knew of in only one other textbook (Demartres' *Cours*) and he approved of the way the topic of algebraic functions and their integrals had been marginalised. But this was only qualified praise. Indeed, Bôcher wondered if the enthusiasm for rigour in texts on complex analysis was not misplaced, given that "the reader may have been accustomed up to this point to the utmost laxity". This comment may say something about American universities at the time, or it may reflect on the hegemonic character complex function theory was acquiring.

The last of the books we consider in the present group is by Klein's hard-working colleague, Fricke. His (1900) was aimed at presenting complex function theory to applied mathematicians. So it gave what we can now recognise as a standard selection of topics: the Cauchy integral theorem (via Green's theorem), Taylor series, singular points, Laurent series, the principle of the argument, two-leaved Riemann surfaces, elliptic functions, and the applications of elliptic functions.

This survey suggests some interesting questions. It is clear that the atmosphere was a charged one, with strong claims being made for the merits of either Riemann's or Weierstrass's approach. Or rather, it is clear that authors felt that this was the case. But many are more eclectic than partisan. Thus Thomae's elementary book is strongly Weierstrassian, but that is plainly his view as a teacher about how to get

students started; his more advanced text is rather stridently Riemannian. Biermann's book is deliberately Weierstrassian, but it does not represent a tide—nor, for that matter, did it travel with the approval of the master. Instead one suspects that what mathematicians were responding to was increasingly programmatic utterances coming from Berlin, either from Weierstrass himself or from his students: Schwarz, certainly, and one might wonder about Frobenius. The dominant current in the published literature flows neither from Berlin nor Göttingen, but from Paris. Many of these books cut out an elementary part of the subject that is essentially due to Cauchy, as the authors are happy to acknowledge. And not least among these authors is the other sage of Berlin, Leopold Kronecker.

Where Kronecker differed from most of these authors is in his wish to treat complex function theory as but one part of a unified subject, the calculus of one and several variables. He was strangely adamant that there was not a special subject marked out by the choice of a complex variable. But where he agreed is that when one is studying functions of a complex variable, there are two key results: the Cauchy integral theorem and the Cauchy formula. This agreement goes beyond the central role attached to the theorems themselves. It extends to their proofs, whether by a calculus of variations type argument or via Green's theorem. It embraces the main applications, the Taylor and Laurent expansions and the principle of the argument. It even concurs in the omission of a topic used by Riemann but central to Weierstrass's approach: analytic continuation. Indeed, one might argue that where these treatments differ most is in their aims, the topics they have in mind for later or future work. There the authors had to decide whether or not to discuss algebraic functions and Riemann surfaces, elliptic functions (and, if so, two-leaved Riemann surfaces in particular), and theta functions. It is not surprising that texts aimed at the upper end of a difficult subject chose one or another part for particular emphasis; it is more surprising that a consensus was already emerging as to what constituted the elementary core of the subject.

An interesting perspective on all this activity is provided by a lecture course given in the Winter Semester 1896/97 at Göttingen by David Hilbert (unpublished; some of the notes are in the hand of H. Dörrie, some in another, perhaps for a later course). Hilbert recommended the book by Durège as a first introduction to the subject, and more strongly Thomae's *Abriss* of 1890 for the Cauchy–Riemann tradition, and [Thomae \(1880\)](#) for the Weierstrassian approach, while he found Biermann's [\(1887\)](#) less reliable. A later scribe noted that Burkhardt's [\(1897\)](#) was recommended as the book to buy. Jordan's *Cours* and Picard's *Traité* were also mentioned favourably. Hilbert then started on the Cauchy approach. After defining complex numbers he defined analytic (or “regular”) functions as those that satisfy the Cauchy–Riemann equations and whose first partial derivatives are continuous on some domain. Indefinite integrals on simply connected domains were defined as anti-derivatives, and definite integrals as limits of sums. He proved the Cauchy integral theorem and the Cauchy residue theorem and obtained their first consequences: the existence of higher derivatives and the fundamental theorem of algebra. Morera's theorem was proved, and the Cauchy integral theorem used to establish the theorem on removable singularities. Then he discussed the theory of power series and their

various modes of convergence, Taylor series, Lagrange's inversion theorem, Laurent series expansions, and went as far as the theorems of Weierstrass and Mittag-Leffler on constructing functions with prescribed singularities. There followed a chapter on special functions, including the Riemann zeta function. Finally came material on conformal representation, circular-arc triangles, and Picard's theorem. The course shows a steady attention to detail: the concept of a path is defined and often required to be twice differentiable; domains are distinguished from their boundaries, natural boundaries are discussed and it is explained that domains of definition and convergence may not be the same. And there is a little Göttingen rhetoric to keep the spirits up: towards the end of these 271 pages of notes Riemann's paper on Abelian functions is hailed as "the masterwork of the greatest spirit of our science".

One must not insist too much on the importance of an unpublished lecture course, even by Hilbert, and even though the lecture notes were kept outside the Mathematics Library (where they remain to this day).⁷ But it is worth noting that this course, presented with Hilbert's rigour and clarity, is at least a model for the later synthesis presented by Bieberbach (who studied at Göttingen in 1910).

10.5 The French Textbook Tradition

The French textbooks are *Cours de M. Hermite, 1882* edited by Andoyer; *Cours d'analyse* (2 parts) (1892) by Demartres; Jules Tannery *Introduction à la théorie des fonctions d'une variable* (1 vol. 1886, 2nd ed. 2 vols 1904–1910); H. Laurent *Traité d'analyse*; Jordan *Cours d'analyse*, 3 vols (1882–1887; 2nd ed. 1893–1896); Picard *Traité d'analyse*, 3 vols (1891–1896), Goursat *Cours d'analyse mathématique* (3 vols, 1902–1915), E. Fouët *Leçons élémentaires sur la théorie des fonctions analytiques* (3 vols, 1902–1904), and the French text by Ernst Lindelöf *Le calcul des résidus et ses applications à la théorie des fonctions* (1905). It is striking how large most of these have become; evidently complex function theory had a growing claim on people's attention.

It is of course not surprising at all that the French textbooks of the period placed Cauchy's ideas at their centre. A more interesting question is the extent to which French textbooks took a Riemannian line or a Weierstrassian one, especially in the higher reaches of the theory.

The first of these is Hermite's *Cours*, based on his lectures at the Faculty of sciences at the Sorbonne. Although Cauchy's approach is in the background of virtually all the arguments treated in these lectures, Hermite's book provides a remarkable mixture of points of view. It opens with some chapters devoted to geometry: the quadrature and rectification of curves surfaces and the evaluation of

⁷There was at least an informal distribution process: there is a set of these notes at the University of Illinois at Urbana-Champaign. Presumably these were Townsend's notes, for Townsend took his PhD under Hilbert in 1900.

the volume of some solids. Complex numbers and variables, and their geometrical interpretation, are introduced in Chap. VI where Hermite avoided giving a general definition of a complex function. Instead, he discussed the properties of elementary functions such as $u = z - a$ and $u = (z - a)(z - b) \dots (z - l)$ and $e^u = z - a$ before devoting the subsequent chapter to the integration of real and “imaginary” functions. Hermite introduced integrals in a complex domain using Cauchy’s 1825 definition (and indeed at this stage he also used Cauchy’s outdated terminology which he abandoned later on in the course). Interestingly enough, when proving that the value of the integral is independent on the path of integration he switched to Riemann’s methods that were based on Green’s theorem as given by Carl Neumann (Neumann 1865a,b, in). Having obtained the Cauchy formula and, from it, the Taylor and MacLaurin expansions of a “finite, continuous and single-valued” function $f(x)$, in Chap. 2 he stated Cauchy’s theorem that $f(x)$ can be expanded in convergent MacLaurin series for values of x smaller than the least among the quantities for which the function ceases to be continuous and finite. At this point, following Briot and Bouquet, Hermite called such functions holomorphic in their convergence domain.

Next, he introduced Weierstrass’s representation formula as given in (Weierstrass 1876b) and translated into French by Picard in 1879, but he omitted the proof. In order to obtain Weierstrass’s prime functions Hermite followed Mittag-Leffler’s method, then he obtained Weierstrass’s expression for $G_a\left(\frac{1}{x-a}\right)$ following the method introduced by Bourguet in his doctoral thesis (1880). Having introduced poles and essential singularities he stated the Casorati–Weierstrass theorem and introduced the notion of residues and their applications. Then Hermite considered single-valued functions with infinitely many poles (or with infinitely many essential singularities) established Mittag-Leffler’s theorem, and turned to meromorphic functions. He established Riemann’s identity theorem—two single-valued functions which agree on an arbitrarily small line-element are identical—using the easier proof given by Picard, and applied it to the study of the Gamma function. In Chap. XVI Hermite tackled the study of the function $\Phi(z) = \int_{\alpha}^{\beta} \frac{F(t,z)}{G(t,z)} dt$, and stated, remarkably, that the idea of Riemann’s cuts lies naturally at the beginning of the integral calculus. Then he introduced Lagrange’s series for the roots of the equation $z = a + \alpha f(z)$ and, as a particular case, he applied it to Kepler’s equation. Finally, in Chap. 20 Riemann’s method of cuts was applied to elliptic integrals. This introduced elliptic functions, to which he turned in the last four chapters of his *Cours*.

The 4th revised edition of the *Cours* appeared in 1891. Noteworthy changes with respect to the previous edition include, in the 3rd lecture, the reduction of elliptic and hyperelliptic integrals to the canonical form. The account of the transformation of elliptic functions was enlarged, and material included on the differentiation of elliptic functions with respect to the modulus, and the proof of Gauss’ relation for the hypergeometric function $F(a, b, c, 1) = \Gamma(c)\Gamma(c - a - b)/\Gamma(c - a)\Gamma(c - b)$.

Hermite’s approach was essentially followed in Demartres’ *Cours* (1892). Part II of this book deals with analytic functions and their properties. He began by considering a region of the complex plane $z = x + iy$ and two “well-defined”

functions $X(x,y)$ and $Y(x,y)$, so $u = X + iY$ is “well determined” and is what Demartres called a function of z . Then he defined the continuity of X and Y as functions of two variables in a region C , and proved that if a function is continuous within a domain C and on its boundary, it is also uniformly continuous there. Then he differentiated u with respect to x and y , thus obtaining the Cauchy–Riemann equations, which turned out to be necessary conditions for the function to be complex differentiable. If the partial derivatives are continuous, these conditions are also sufficient. Accordingly, Demartres defined an analytic function as a monogenic function with a continuous derivative. He proved that such a function realises a conformal map of $\mathbb{C}$ whenever its derivative does not vanish. A holomorphic function is by definition a single-valued, continuous, finite, and analytic function.

After this general theory, Demartres introduced elementary functions, power series, and uniform convergence and established when it is legitimate to differentiate and integrate power series term by term. In the 4th lecture he introduced definite integrals along a closed path with the example $\int \frac{1}{z} dz$ then, more generally, $\int Pdx + Qdy$ and proved that the condition $\frac{\partial P}{\partial x} = \frac{\partial Q}{\partial y}$ is necessary and sufficient for $Pdx + Qdy$ to be an exact differential. The Cauchy integral theorem was obtained as a straightforward consequence; Demartres also gave Goursat’s proof (Goursat 1884) “because of the importance of this theorem”. The Cauchy formula followed immediately. Chapter 5 was devoted to harmonic functions and the Dirichlet problem, which was solved in the case of a circle. In the general case Demartres claimed that the problem “always admits one and only one solution”. Here he invoked the Dirichlet principle, which he did not prove although he claimed that it “has been established with all rigour” but contented himself with showing how it had been reduced by Dirichlet to the question of minimising an integral.

Meromorphic functions entered the picture in Chap. 6, where Demartres obtained Cauchy’s formula for the n -th derivative of a function, then the Maclaurin and Taylor series. He proved the identity theorem following Hermite’s method and established some properties of meromorphic functions before proving Liouville’s theorem and stating Picard’s (without proof).

Chapter 7 dealt with Laurent and Fourier expansions. Having introduced essential singularities, Demartres stated and proved the Casorati–Weierstrass theorem without naming it. Then he introduced sets E of infinitely many singular points, and their derived sets E' whose points he called singular points of the second class. He illustrated this with the example of the function $\sin \frac{1}{z}$ whose zeros form a set whose derived set is 0.⁸ Thus, $\frac{1}{\sin \frac{1}{z}}$ has 0 as a limit point of poles. He introduced the theory of residues, and then Riemann identity theorem for functions U and V which he proved as Hermite had done.

Chapters 8 and 9 were devoted to the representation of uniform (single-valued) functions. Following Hermite he gave Mittag-Leffler’s proof of Weierstrass’s decomposition in prime functions. Then, in Chap. 10 he analysed functions having an

⁸This passage is reminiscent of du Bois–Reymond’s (1882), but that was devoted to real analysis.

addition theorem and discussed Weierstrass's $\sigma(z)$ and $\wp(z)$. The following chapter was devoted to algebraic functions. He referred to Puiseux and Briot and Bouquet before treating elliptic and hyperelliptic functions and integrals in the last lectures in some detail.

The next of these is Tannery's *Introduction*. Complex function theory is only discussed in the second edition, in the second volume (Chaps. X–XII), and even then the flavour is a mixture of real and complex analysis. Definitions are careful and given in a leisurely fashion; examples are particularly carefully discussed when they involve multi-valued expressions and have to be handled by the method of cuts. Tannery took quite some pages to get to the definition of the derivative of a complex function, which he treated as a special case of differentiable functions of two real variables. He took even longer to get to the Cauchy integral theorem, the integral formula and, discursively, to the proof that every holomorphic function is analytic. On the other hand, one has the feeling that the treatment was designed to be followed by students, and even that it could have been successful. Certainly a generation of mathematicians who passed through the École Normale Supérieure spoke warmly of his influence.

The next, and least impressive, of these books is the 7-volume *Traité d'analyse*, written by H. Laurent (an entrance examiner to the École Polytechnique) and published between 1885 and 1891. Complex functions make a brief appearance in Volume 1, Chap. 8, where they are defined correctly as functions with a derivative independent of direction, but it is then assumed without further ado that they possess all further derivatives. This oversight mars the rest of the treatment of the rest of the theory. In Volume 3 Chap. 6 *synectique* or holomorphic functions are defined (Laurent saw no reason to change the term), and the main features of the residue calculus are given including applications to standard definite integrals. Green's type theorems are proved and attributed to Riemann, reasonably enough, and used to give a second proof of the Cauchy integral theorem; the first used total differentials. In Chap. 7 he proved that a series of holomorphic functions uniformly convergent in some domain may be integrated and differentiated term by term and therefore defines a holomorphic function. Given the initial mistake in Volume 1, this is a fair attempt at the result.

Laurent knew that Cauchy's original proof that such a series defined a continuous function was flawed and argued at this point that Cauchy evidently elided the concepts of convergent and uniformly convergent. For all that Laurent's views, here and elsewhere, seem unduly patriotic, there is a sense in which his reading of Cauchy is one that historians have recently taken a long time to come back to (see [Giusti 1984](#)).

Next Laurent gave the Cauchy integral formula, after which he missed the opportunity to prove that indeed a holomorphic function possesses all its derivatives. He followed this up by failing to see that there was anything to prove in Laurent's theorem (the other Laurent, Pierre). He then described the principle of the argument and gave an account of Lagrange's theorem (but not Kepler's equation, which he left to texts on celestial mechanics). Similarly sketchy accounts of the theorems of

Weierstrass and Mittag-Leffler on functions with prescribed singularities followed. All in all, a disappointing work to this point, but then matters picked up.

In Volume 4 he gave an extended account of algebraic functions, their integrals (with geometrical applications) elliptic integrals, elliptic functions, modular functions, and Abelian functions. From the start he took a Riemannian approach, which he regarded as simpler and more rapid than Cauchy's, arguing also that it was used, in any case, by every German mathematician(!). He defined the genus of a curve in terms of its degree, its class (the degree of its dual curve) and its branch points, and proved that the genus was unaltered by invertible transformations of the curve; a result first indicated by Riemann but proved, he said for the first time and in its true meaning, by Halphen and Smith. He discussed Lüroth's theorem on canonical forms for paths connecting any point on a Riemann surface with any other, proved Abel's theorem about sums of Abelian integrals on curves with at most simple singularities, and observed that the proof would be general if Noether's theorem on the resolution of singularities was valid. However, he said, (p. 187) "to our knowledge there is no truly rigorous proof of this general proposition". He then moved on to the Riemann bilinear relations (not so-called) and Riemann's theory of theta functions, finally closing the Volume with Weierstrass's theorem on the existence of an algebraic relation between any $n + 1$ functions in n variables with $2n$ common periods, and Riemann's theorem, recently proved by Poincaré and Picard, on the representability of a function of several variables by means of theta functions (see Sect. 9.3).

The overall impression of Laurent's *Traité* is that it lacks rigour, often simply failing to see what needs to be proved. Where it overlaps, it is a falling away from the standards of Briot and Bouquet. But where it is new it is a readable account of the material for anyone prepared to look for the details elsewhere. Such an account was by then being published.

In the first volume of Jordan's *Cours d'analyse* (1882) complex variables occasionally occurred in Chap. 3 Sect. VI in the expansion of exponential and trigonometric functions in power series. The treatment of theory of functions of an "imaginary" variable proper was postponed to vol. 2, published in 1883. There in Chap. VI he defined a function of an "imaginary" variable to be one that satisfies the Cauchy–Riemann equations. Then, following Cauchy (1825a), he defined the integral $\int_{z_0}^{z_1} f(z)dz$ along a line L joining z_0 to z_1 , and echoing Cauchy's 1825 proof, he showed (pp. 275–276) that its value does not change under small variations of L that do not cross critical points. The Cauchy integral theorem (p. 280) followed as an immediate corollary. Having established the Cauchy integral formula Jordan was able to obtain both Taylor and Lagrange series expansions, Weierstrass's product theorem, and Mittag-Leffler's theorem as well. Then, a thorough treatment of elliptic functions followed (pp. 339–432). Remarkably, the Jordan curve theorem was stated and proved in a note appended at the end of vol. 3 (1887, 587–594) among various notes "on some points of the theory of functions".

The second edition of the first volume of Jordan's *Cours* was published in 1893, and is notable for the way its account of real analysis is frequently grounded in considerations of point-set topology. So, the note on the Jordan curve theorem moved to vol. 1 (pp. 92–100), as did the theory of functions of a complex variable.

He defined a *synectique* (i.e. holomorphic) function to be one that satisfies the Cauchy–Riemann equations. Then he proved the Cauchy integral theorem in a novel way (pp. 185–195), reducing it by successive approximations to the case of a triangle. He then proved it directly for a triangular contour ABC in which he assumed, without loss of generality, that the vertex C was vertically above the vertex B by an amount m . The argument proceeded by varying C and showing this had no effect on the integral, so the triangle could be collapsed to the line segment AB . The variation was calculated by differentiating the integral with respect to the parameter m and using the Cauchy–Riemann equations to show that the integral did not change. The first stage of the reduction, replacing a simple closed curve with a polygonal approximation recalls Jordan’s own attempts on his eponymous curve theorem, which was indeed motivated by the need to know which points are inside a closed contour. Familiar theorems followed: the Cauchy integral formula, the existence of all derivatives of a holomorphic function, the existence of the Taylor series and its convergence; the existence of a critical point on the boundary of the circle of convergence; the fundamental theorem of algebra. Conversely, convergent power series are *synectique*; this converse is not so much a nod in Weierstrass’s direction as Cauchy’s. An elementary treatment of algebraic functions followed, based on Cauchy’s simple but ultimately inadequate idea of a *coupure*, and then the usual circular and trigonometric functions were introduced, starting with the logarithm, defined as a solution of the differential equation $df = \frac{dz}{z}$.

The second edition of the second volume of Jordan’s *Cours* was published in 1894. In Chap. 6, devoted to “complex integrals”, he introduced Cauchy’s residues, stated and proved the representation theorems of Weierstrass and Mittag-Leffler, the Liouville and Casorati–Weierstrass theorems (without naming them) and gave the Picard’s little theorem (with Picard’s name attached but without a proof). The level of rigour in the treatment of complex analysis was increased, but more importantly Jordan gave a new, 230-page account of elliptic functions, one that immediately praised by Harkness in his review (Harkness 1894–1895) and which is so good that it was reprinted again as (Jordan 1981).⁹ Jordan now followed the accounts of Schwarz and Halphen and presented the Weierstrassian theory with elegance and precision. A final chapter considered Abelian integrals, following the accounts of Riemann, Picard, Laurent, and Forsyth as far as Abel’s theorem, the Riemann–Roch theorem, and the inversion of the integrals.

Picard expounded the theory of complex functions in the second volume of his *Traité* (1891–1896). This is harder to evaluate, because it has taken a lesson taught by Riemann and Poincaré only too firmly to heart. Uniquely, this is an approach that sees complex function theory as part of the theory of harmonic functions. So although the familiar theorems are all there, the proofs are different. So too was his ultimate aim, which, as he said in the introduction, was the theory of Riemann surfaces “whose study has been left a little too much to one side in France”. He

⁹Pierpont, in his (1899, 490), however, lamented that Jordan’s treatment of Jacobi’s theory in his *Cours* seemed to him “to rob the reader of half the beauties of this theory”.

had in mind not only the simplification Riemann's ideas provided when studying algebraic curves, but also what he called the essential idea of Riemann's theory, the existence a priori of such surfaces and the difficult but vital question of the existence of analytic functions upon them. This gives his *Traité* an altogether more advanced character than the other books we have been discussing.

Goursat's two-volume textbook ([Goursat 1902–1905](#)) on analysis rapidly became famous and was translated into English between 1904 and 1917 by E.R. Hedrick at Osgood's suggestion. Complex function theory occupies part of the second volume. Goursat defined a complex function to be analytic if its derivative exists, and deduced that it satisfies the Cauchy–Riemann equations. He also correctly claimed that the converse is true if the real and imaginary parts of the function are continuous. Then he observed that such functions are conformal except when the derivative vanishes. Then came the Cauchy integral theorem (with the “Goursat proof”), the Cauchy formula (and the observation that it can be used to create holomorphic functions), a proof that all derivatives of a holomorphic function exist, the Taylor and Laurent series expansions, and Liouville's theorem. He also observed that the zeros and poles of a holomorphic function are isolated and, for the first time in a textbook followed the discussion of the concept of an essential singularity with a proof of the Casorati–Weierstrass theorem and Picard's little theorem. The second volume of the second French edition has an Appendix by Hadamard in which he states and proves the Brouwer fixed point theorem (before Brouwer's own, different, proof appeared).¹⁰

The two-volume work by Édouard Fouët, a professor at the Institut Catholique de Paris, shows certain new features among a number of classical ones. In the first volume, there is an early chapter on Cantorian point set theory. Then the concept of a continuous function is introduced, and then analytic functions are introduced as those having a derivative. The Cauchy–Riemann equations follow, as does conformality in general. Algebraic functions and Riemann surfaces come next, before functions defined by series (as for example the elliptic functions in their traditional and their Weierstrassian garb) and the elementary transcendental functions. Only after a chapter on functions of several variables come the Cauchy integral theorem (with Goursat's proof and a proof using Green's theorem), the Cauchy formula, the Cauchy residue theorem, the fundamental theorem of algebra and the Taylor series expansion (but not Laurent's). The book concludes with an account of analytic continuation d'après Weierstrass, but actually discussing lacunary spaces, the theorems of Poincaré and Volterra, and non-analytic continuation.

Two years later he offered the second volume. It began with some 70 pages on differential equations, ordinary and partial, a mere 10 on trigonometric and elliptic functions, and then an account of “The theory of analytic functions from the point of view of Cauchy, Riemann, and Weierstrass”. Here we find, under Cauchy, the “Cauchy–Liouville” theorem, the Laurent and Mittag-Leffler series, more about residues and some applications to the evaluation of definite integrals and the solution

¹⁰We thank Jean Mahwin for this information.

of ordinary differential equations. Under Weierstrass, we find the theory of entire functions and the concept of genre, meromorphic functions and the Mittag-Leffler decomposition, and 16 pages on analytic functions of several variables. Finally, under Riemann, we find results about harmonic functions, conformal representation, and minimal surfaces, with Schwarz's solution of the Dirichlet problem.

Lampe, in his reviews of these books in *Fortschritte* (33.0391.01), remarked that the book would find many readers because of its breadth and its clarity, upon which French authors always lavished so much effort. Jules Tannery, in his review (1905, 84–89) welcomed the abundance of material that was characteristic of the author, with his liking for overviews, historical remarks, and indications of points not to be developed. The range of recent material was particularly welcome, and it is clear that Fouët was concerned to put many current research topics into a textbook.

Humbert's *Cours d'analyse* can be passed over briefly. It announced itself as following the classic texts by Briot and Bouquet and by Jordan, staying closer to Cauchy than Weierstrass, and putting the methods of Riemann on one side as "useless here". As advertised, it then gave Cauchy's theory, proved Liouville's theorem and the principle of the argument, and then added to what he said he lectured on by presenting the representation theorems of Weierstrass and Mittag-Leffler. There then followed chapters on applications, including the Fresnel integral; on elliptic functions, with applications and a discussion of how the sigma and theta functions can be evaluated numerically. The rest of the *Cours* was on differential equations.

Ernst Lindelöf's *Le calcul des résidus et ses applications à la théorie des fonctions* was published in Paris in 1905 as the ninth in Borel's series of monographs. He began by defining a complex function $f(z)$ as one that satisfies the Cauchy–Riemann equations. Therefore $f(z)dz$ is an exact differential on any simply connected domain on which it is defined, and therefore the Cauchy integral theorem holds. Lindelöf, in one of many insightful historical notes that are a feature of the book, observed that a proof via Green's theorem could also be given, as Cauchy had done for a rectangular domain in 1814, and Kronecker too in his *Vorlesungen*. The Cauchy formula and its variant for the higher derivatives followed, whence the Taylor series expansion. Lindelöf observed that, on the one hand, it had taken Cauchy a long time to write them down, even for a circular contour, and, on the other hand, they were already well known to Frullani, Poisson, and Parseval, who, however, had started from the Taylor series.¹¹

Then we get the calculus of residues, with the Cauchy $\mathcal{E}$ symbol, the principle of the argument, Rouché's theorem, and summation formulae, a topic which Lindelöf felt was particularly important. He traced their history back to (Plana 1820) and (Abel 1823). However, he said, only complex function theory could provide the foundations for such formulae. However, they had seemingly slipped from sight for a generation, to be revived by Kronecker (who knew only Abel's paper) and then (Petersen 1898) (see below). But recently there had been several papers: by Hermite,

¹¹Discussed in (Smithies 1997, 129).

Mellin, Jensen, and Weber. Lindelöf gave several applications. The evaluation of definite integrals was also described. Then came a chapter on the Γ and ζ functions, and the book concluded with a chapter on analytic continuation and asymptotic expansions. It also brought to an international audience some recent results of Mittag-Leffler that were at that time only available in Swedish.

De la Vallée-Poussin's *Cours d'analyse*, which came out as a two-volume work in 1903–1906, need not be considered because it is devoted to real analysis and makes only scattered reference to complex function theory at the very end of volume 1.

If there is a simple pattern to which these texts conform, it is the acceptance of an agreed starting point—the Cauchy–Riemann equations—and the existence of at least one route to a proof of the theorem that a holomorphic function is analytic. At this point it becomes possible to graft on a large amount of the Weierstrassian theory. That said, several are encyclopedic, and some are eclectic in their choices. Only one, Fouët's, is explicit in acknowledging that there are three traditions in the subject. There is more of a consensus, visible in the placing of the subject in the middle of the larger works, that complex analysis can rest on a certain basis of real analysis, which further commends the use of the integral and renders Weierstrass's dislike of it harder to understand.

10.6 The Italian Textbook Tradition

The early involvement of the Italians in complex function theory did not lead to a rush of textbooks. Indeed, even though Casorati began one, as we have seen he never finished it, and there is no evidence of other lecture notes of courses on the subject in the 1870s and 1880s. With the obvious exception of Casorati in Pavia, there is no evidence either that complex function theory was taught at Italian universities in those decades, the courses in analysis being generally devoted to real analysis, and to calculus in particular. Apparently, the first lecture note of a course on complex function theory, admittedly late in the period under consideration, was Dini's in 1890.

As sets of lithographed lecture notes show, at the Scuola Normale in Pisa Dini taught such related subjects as *Funzioni sferiche e funzioni di Bessel* (lith., 1877), followed by a course on the *Teorica delle funzioni ellittiche* (lith., 1884). In 1890 he turned to complex function theory proper. The lecture notes of his course *Funzioni di una variabile complessa* (lith., 1890) are divided into two parts. The first one opens with chapters on integrals of functions of two (real) variables and, in particular, on the integral of Laplace equation in a connected domain. Following Riemann's approach, Dini obtained Green's formula and functions, and established the main properties of harmonic functions. He solved both the Dirichlet and Neumann boundary problems (without naming them in this way) by means of a Poisson integral argument, occasionally referring to his own paper on the subject (1871–1873). In fact, with the exception of some occasional reference to his own work, this set of lectures shows a remarkable lack of references.

After giving Riemann's definition of a function of complex variable, Dini established some of Cauchy's major results—his integral theorem and formula, the related expansions in power series of a function, including the identity theorem, the Laurent expansion and eventually Cauchy's principle of the argument as well. This was followed by a chapter on singular points, where he established both the Liouville and the Casorati–Weierstrass theorems. After summarising the results he had established in the opening chapters, he dealt with the problem of defining a function in a given bounded domain according to Riemann's approach. Then he switched to Weierstrass's, and introduced functions with prescribed zeros and infinities according to the latter's product theorem and the Mittag-Leffler theorem (again without naming them). The last chapter of the first part was devoted to the applications of the theory of residues and of contour integration. The theory of elliptic function was the subject of the second part of the course.

Pincherle's course is another example. As we have seen, as a research mathematician he was strongly drawn to the Weierstrassian approach, although he could see the force of Klein's re-presentation of Riemann's ideas. In 1892–1893 he lectured on the theory of analytic functions at Bologna, and these notes were published as (Pincherle 1893). Unlike Dini's, Pincherle's lectures were rich in references to primary sources, and to secondary ones (such as the treatises of Jordan and Picard), and he offered a wide-ranging account of the theory according to Weierstrass, Cauchy, and Riemann.

The course began with some topological notions taken from Cantor's set theory, followed by the definitions of a continuous (and uniformly continuous) function, and of a uniformly convergent series of functions, and statements of the related theorems of Weierstrass. This was followed by a chapter on path integrals in a connected domain. Then came a chapter on power series, including the identity theorem, the Cauchy(–Hadamard) theorem and Pincherle's related theorem, double series and Weierstrass's theorem, and Taylor series. After which came the introduction of the notion of analytic continuation and, rather unusually, natural boundaries and gap series.

In Chap. V Pincherle switched to Cauchy's theory. A function was defined to be analytic if and only if it satisfied the Cauchy–Riemann equations, and the earlier examples could now be seen to be analytic almost everywhere. The integral of a complex function along a path was now defined, and the Cauchy integral theorem proved (under the hypothesis that $f'(z)$ is continuous), as an immediate consequence of the theorem that the integrals of an analytic function along two paths with common endpoints in a bounded domain are equal. In order to prove this, Pincherle argued in a way reminiscent of Jordan's approach, but he was forced to admit that he had only intuitive grounds for accepting a crucial step in the approximation process. Having proved Cauchy's integral formula he boldly concluded that “Cauchy's concept of monogenic function, and Riemann's concept of complex function are included in Weierstrass's concept of analytic function”. Then, unusually for this time, he established Morera's theorem. Having established the Laurent theorem and Laurent expansions, he then presented the theory of residues and their applications. This elementary part was followed by a more

advanced one, where Pincherle introduced entire functions, Weierstrass's prime factors and infinite products (mentioning in passing Betti's related theorem and Laguerre's "genre"). Then he gave examples of expansions of functions in infinite products, including Weierstrass's σ function, established Mittag-Leffler's theorem, expounded recent related work by Hermite (1881) including his theory of *coupures*, and eventually devoted a chapter to the Laplace transform.

In the second part of his course Pincherle introduced algebraic functions and their integrals, and the notion of Riemann surface. Pincherle also presented Puiseux's results and offered a detailed discussion of the order of connectivity of a Riemann surface as an introduction his treatment of Abelian integrals and their classification that culminated in the statement and proof of Abel's theorem.

In 1900 a second, rather different edition of this course was published. We limit ourselves to pointing out the main differences. In 1900 Pincherle began with the definition of complex numbers and their geometrical representation on the plane and the Riemann sphere, and the elementary theory of Möbius transformations. Then examples of functions were given, such as $\frac{1}{z-a}$, $\sqrt[m]{z}$, and the circular and exponential functions.

As for Cauchy's theorem, in 1900 Pincherle took the opportunity to survey the proofs of this "fundamental" theorem and classified them into two types: those inspired by Cauchy's original proof of 1821, and those deriving from Riemann's. He gave an example of each kind, observing that Riemann's proof, which used Green's theorem and the Cauchy–Riemann equations to show that the complex differential $f(z)dz$ was exact, was both simpler and had the merit of drawing on a theorem that was widely used in mathematical physics. He took his version of Cauchy's proof from Jordan's *Cours* (1883). As in his (1893), standard applications followed: Taylor and Laurent series expansions, the definition of poles, and Morera's theorem. Meromorphic functions were defined, the principle of the argument was established, and rational functions characterised.

This concludes what might be termed the elementary part of the book. Then, still following his (1893), he presented more recent material: entire functions, Mittag-Leffler's theorem (including this time the special functions Γ , ζ , and $\wp$) and an extensive treatment of algebraic functions, Riemann surfaces, and Abelian integrals. Contrary to his (1893), in 1900 Pincherle's course concluded with elliptic functions, from the Riemannian, Weierstrassian, and Jacobian points of view.

In 1901 two books were published on complex function theory in Italy by Bianchi and, resp. Vivanti. Bianchi's *Lezioni sulla Teorie delle funzioni di variabile complessa* in two volumes was the printed version of a previous lith. edition (1898–1899). The material was presented by Bianchi after the model offered by the course of his colleague, and former teacher, Dini (1890) that Bianchi enriched with substantial, more recent, and advanced developments. Like Dini's, Bianchi's book was eclectically Weierstrassian, by which we mean that having defined a complex function as one that satisfies the Cauchy–Riemann equations, the motivating examples were defined by power series. Chapter II dealt with discontinuous groups of Möbius transformations, Chap. III with harmonic functions, Chap. IV with Cauchy's theory (the Cauchy integral theorem, Cauchy's formula, Laurent

series, theory of residues and Cauchy's principle of the argument). There followed a chapter on singular points, with ingenious examples of essential singularities including the Casorati–Weierstrass theorem and the statement of Picard's theorem that was proved in the second half of the book. This chapter included expansions in series of transcendental entire functions and the Mittag-Leffler theorem, followed by Weierstrass's prime factors and product theorem. Then, unusually for a textbook, there was a chapter on complex functions of several variables, preliminary to a chapter on Riemann surfaces and Abelian integrals. A second half to the book treated elliptic and modular functions including their application to the solution of algebraic equations.

Stäckel, in his review of Bianchi's book in (*Fortschritte* 29.0330.02) suggested that the book was best regarded as an introduction to automorphic functions. As for the first part, Stäckel was pleased to see both the Cauchy–Riemann and the Weierstrass approaches developed, since both had become necessary in research, but he felt that using the Cauchy integral theorem to prove that an analytic function can be developed as a power series was using a sledgehammer to crack a nut and robbed the Weierstrassian approach of its truly elementary character.

Vivanti's *Teoria delle funzioni analitiche*, published in the Manuali Hoepli series of pocket books, cut the theory down to the required size by presenting just the Weierstrassian theory; as with Biermann's book (1887) Cauchy's theory of complex integration is never mentioned. But entire functions are treated and generalised to meromorphic functions with any kind of singular points. And a selection of recent results are given on lacunary functions and boundary behaviour, due to Poincaré, Borel, Hadamard, and Pringsheim. The book was given a favourable review by Gutzmer (*Fortschritte*, 32.0387.01) who promptly abandoned his plan of writing a similar book in favour of producing an enlarged, and revised, translation of the Italian's: Vivanti–Gutzmer, *Theorie der eindeutigen analytischen Funktionen*, (1906). The additions mostly concerned the recent, more advanced material, and we need not discuss them.

Tannery, in his review of Vivanti–Gutzmer (*Fortschritte* 37.0407.01) made an interesting comparison between contemporary function theory and the algebraic geometry of a few decades ago. Geometry had been split into two camps, synthetic and analytic, but now every one understood that proofs in the one style could be translated into the other. But function theory was far from reaching this end. The two most recent German books showed this only too well. The geometrical approach taken by Osgood could be said to be synthetic, while Vivanti–Gutzmer was algebraic analysis. It not only avoided the Cauchy integral theorem, but anything that was not arithmetical in character. Bringing the two approaches into harmony was the reviewer's implied wish for the future.

10.7 English and American Imports

British and American universities were slow to catch up with the European centres in France, Germany, and Italy. Their textbooks therefore illuminate the phenomenon of importing mathematics. Such work is often done not by a group of experts themselves conducting ambitious programmes of research, but by the first scholars capable of reaching the required level. Their judgements about what to include and how to treat it are therefore informative both about the subject, as they saw it, and about their sense of the domestic audience. Their aim is not so much to add to the subject, as to enable their national communities to do so in due course.

The American scene has been described by Parshall and Rowe (1994), and the British one by June Barrow-Green (1999). Their work makes it clear that in both countries there was a deliberate attempt being made to catch up with the best of Europe. Curiously, in each case the most approachable figure, the man shrewd enough to realise that making contact with the Anglophone world had something in it for him, was Felix Klein. His influence on many American students who came to Göttingen is well known (see Parshall and Rowe 1994). Among the English, his effect was most pronounced on H.F. Baker; more might have been advisable.

A view of what was the accepted opinion in Britain in 1891 is provided by Henrici, in his review of Schwarz's *Gesammelte Abhandlungen* for *Nature* (vol. 43, pp. 321–323).

of an analytic function in its generality we know almost nothing . . . we do not know how far the mathematics of the infinitesimal calculus and the calculus of variations can safely be applied to an unknown analytic function in all its generality . . . Objections of this kind, first raised by Kronecker and Weierstrass in their lectures, have since been repeated in more specific terms by various mathematicians, and it has long been generally accepted that Riemann's theorem cannot be considered as proved by him.

But here mention should be made of the Scottish mathematician George Chrystal's *Introduction to algebra, for the use of secondary schools and technical colleges*, 1st edition 1889, which was revised for its 2nd edition with more function theory in 1900. The book had the ambition of being a version of Euler's *Introductio in analysin infinitorum* for its day, and in large measure it succeeded—indeed it was given a 7th edition in 1999. Chapter XXVI gives the first account in an English textbook of continuity and uniform convergence; Chaps. XXIX and XXX give an account of complex function theory from the perspective of power series, including the theory of infinite sums and products.

The man who brought complex function theory fully to Britain was Cayley's successor as the Sadleirian Professor at Cambridge, Andrew Russell Forsyth. He had written a dissertation in 1883 on theta functions of two variables which earned him a Fellowship at Trinity College, and in the same year he gave a novel generalisation

of Abel's theorem.¹² In 1886 he became a Fellow of the Royal Society. Ironically, it was his attempt to follow Cayley in trying to set Continental mathematics before the English that was to be his undoing, specifically his *Theory of functions of a complex variable*, 1893. The scale of the problem facing Forsyth can be measured by his much later observation that at Cambridge in 1880 the symbol $\sqrt{-1}$ was suspect even in trigonometric formulae, 140 years after Euler had introduced it there. Even Forsyth seemed unaware that complex variables were used in Maxwell's *Treatise on Electricity and Magnetism* of 1873.¹³

Forsyth set out not only to explain complex function theory in English, but to bring together the best features of the approaches due to Cauchy, Riemann, and Weierstrass. With his extensive knowledge of mathematics, his passion for exposition, and his influential position, Forsyth should have been the right man for the job. He defined a function to be a function of a complex variable if and only if it satisfied the Cauchy–Riemann equations (but omitted any discussion of the continuity of the partial derivatives). Such functions were conformal except where their derivatives vanish. The Cauchy integral theorem and Cauchy formula followed with the by-now standard deductions that a complex function is infinitely differentiable and has a Taylor series convergent inside some circular domain. Then he described the distinction between poles and essential singularities, and proved the Casorati–Weierstrass theorem. He characterised polynomials. He proved the constancy theorem. He discussed functions with a finite and then with an infinite number of essential singularities, proved Mittag-Leffler's theorem and discussed natural boundaries of functions defined by series of rational functions. Chapters on multiform functions, elliptic functions and pseudo-periodic functions led up to the topic of functions defined by algebraic addition theorems (a typically Weierstrassian classification). Then came material on Riemann surfaces, their connectivity, and on the integrals of algebraic functions.

The second half of the book (where, in later editions, volume 2 begins) opens with Schwarz's proof of the existence of complex functions on a Riemann surface, proceeds to the Riemann–Roch theorem and thence to Abel's theorem. Then we get extensive material on conformal representation: Möbius transformations are introduced and classified, and the Riemann mapping theorem proved. Forsyth first used Schwarz's argument to solve the Dirichlet problem for a disc, and then gave Schwarz's alternating method to extend show how the problem can be solved for a wide class of regions. As he noted, the Dirichlet problem is solved for any simply connected regions that can be mapped conformally onto a disc in a way that maps the boundary continuously onto the boundary of the disc. So, after giving specific examples of Schwarz–Christoffel transformations, he turned to the general task, and tackled it by following the argument originally given by Riemann in his (1851).

¹²The Fellowship essay gives a single formula covering 4,096 identities and is the more remarkable for being written in 3 months by a man who, as he said, was “reared to graduation in applied mathematics” (Whittaker 1942–1944).

¹³It is not mentioned in his drily amusing reminiscence (Forsyth 1935).

However, he restricted its range of application to regions that are single-sheeted and whose boundaries that are either analytic curves or composed of finitely many analytic curves.

This leads up to the introduction to Fuchsian and Kleinian groups, and the book ends with an account of automorphic functions, including the theory of theta-fuchsian functions and the connection to Riemann's theory of functions. It was a bold book, and remarkably up-to-date. It did indeed put the work of Cauchy, Riemann, and Weierstrass into English, with, moreover that of Klein, whom Forsyth frequently brought to England. One hesitates to imagine what Klein made of the ignorance of the Cambridge school, where "[the] college lecturers could not read German, and did not read French". One of the most eminent of them in the 1890s used to speak of the discoverer of the Gamma-function as "Yewler", and the founder of the theory of functions as "Corky": and one at least of the four professors used to refer to the complex variable $x + iy$ as a "semi-imaginary quantity", but it is a measure of the journey that Forsyth had to make (Whittaker 1942–1944, 219). If the book emphasises formal arguments over conceptual ones, that was not only Forsyth's training but that of his readers.

As a result, for his local audience, the book was a complete success. It "burst in 1893 with the splendour of a revelation" (Neville 1942, 245), and Whittaker wrote that the book "had a greater influence on British mathematics than any work since Newton's *Principia*" (Whittaker 1942–1944, 218). All who read it could see how far the British lagged behind in and at first Forsyth's reputation soared. But on the Continent and in America the reception the book earned was only lukewarm, for it was not rigorous, nor did it truly bring together the three strands its author had hoped to interweave. Gradually it became clear that while Forsyth could manipulate huge formulae with skill, as befitted a student of Cayley, he lacked much appreciation for rigour in analysis. His later books confirmed this impression only too well. He had stimulated a taste for rigour, however, and therefore became gradually reduced to the role of one who can only encourage but not keep up.

Prominent among the reviewers was the American W.F. Osgood, who had a sharp eye for rigour. He wrote a lengthy review (Osgood 1895), where he found much to criticise and some to praise. He wrote (p. 143): "But we cannot stop with pointing to the loose form in which theorems are often stated and proofs given; it only too often happens that the ideas on which the proofs rest are lacking in rigour, or that important matters are overlooked". This was his objection to Forsyth's assumption that the student will learn rigorous (real) analysis somewhere else, and while he was surely right to doubt this, the objection was made forcefully because Forsyth's touch was so unsure. Osgood noted of the careful proof that differentiation under the integral sign is permitted that "Usually, such matters are not touched upon". A particular failing of Forsyth's had to do with the uniform convergence of series. Osgood listed numerous lapses, and observed that while absolute convergence may occur only within a circle, uniform convergence occurs both on and inside any circle within the circle of convergence. In particular, a power series may converge absolutely everywhere in the plane, but it cannot do so uniformly unless it is a constant. Another instructive example was provided by Forsyth's use of the

language of infinitesimals in stating the theorem that locally constant functions are globally constant.

Osgood's review is a catalogue of errors great and small. One more must suffice, although no chapter escapes censure. In Sect. 87, Weierstrass's theorem on the expression of a series of analytic functions of the form $\sum f_j(z)$ as a series of positive and negative powers "is stated inaccurately and the proof is confused". Since Weierstrass had long since published his proof ([Weierstrass 1880c](#)) "the author would have done well to translate it literally". Worse, although Forsyth did not prove that such series may be differentiated term by term, he nonetheless did treat them away without comment. Insecure foundations, then, which the British were to endure for nine years until Whittaker's *A Course of Modern Analysis* came along (the first edition, (1902), was without Watson).

The alternative open to them was the American text by the Cambridge mathematicians then resident in America, Harkness and Morley, *A Treatise on the Theory of Functions*, (1893, 2nd ed. 1925). They began with geometrical theorems about complex numbers, then gave a chapter on real functions of a real variable, and then turned to complex function theory, beginning with series à la Weierstrass. They then studied algebraic functions, the topology of Riemann surfaces being analysed in the manner of Clebsch and Lüroth. Then they considered complex integrals, proved the Cauchy integral theorem using Goursat's proof and going as far as the Mittag-Leffler and Weierstrass representation theorems. Then they picked up the topic of Riemann surfaces, looked at their topology, and discussed Abelian integrals. Then they provided a chapter on elliptic functions, one on double theta functions, one on Dirichlet's problem, and finally one on the standard advanced topics in the theory of Abelian integrals. All-in-all, an ambitious book, and one well-received by the reviewers, such as Maschke in the *Bulletin of the New York Mathematical Society* and Meyer in *Fortschritte* (25.0653.01). Meyer noted the absence of American and English work on the subject and welcomed the growth in the number of researchers on both sides of the Channel and the Ocean in the interests of international collaboration. He found much to praise in the book, although he commented on its occasional lack of precision, and he liked its instructive examples.

However, in a short but sharp paper ([Osgood 1904](#)) referred precisely to Harkness and Morley's *Treatise*—as well as ([Biermann 1887](#)) and the 2nd edition of Jordan's *Cours d'analyse*—as examples of treatises written by followers of Weierstrass and Méray who "point with pride" to the fact that, when the theory is based on a single infinite process, namely that of power series, intuition is completely eliminated". After remarking incidentally that "the process of integration would have served the purpose better", he considered such a basic theorem as the following: Let a power series $\sum c_n(z-a)^n$ define an element $P(z|a)$ of an analytic function that can be analytically continued along any path in a finite closed region S whose boundary is given by a simple closed curve, then there exists a function $f(z)$ that is single-valued in S , can be expanded in power series at any point of S and coincides with $P(z|a)$ in the neighbourhood of a . Osgood remarked that all the proofs provided in those treatises depend "essentially on geometric theorems, frequently not even

formulated, the truth of which in every case rests on intuition, or rather on analogy” with what happens in the case of squares, circles and similar simple figures. As he commented caustically (Osgood 1904, 295):

The followers of Cauchy and Riemann, who have based the theory of functions on the calculus and thus followed the lines of least resistance, may well take the ground that there are more important things to do in analysis than to arithmetise the geometry of the domain of the independent variables. But for a school to take this stand who for puristic reasons are not willing to admit the process of integration into the theory of functions of a complex variable, appears to be straining at a gnat and swallowing a camel.

Osgood then gave a proof of the geometric theorems involved in the proof of the above-stated theorem.

For their part Harkness and Morley, as they wrote in the Preface to their next book, *Introduction to the Theory of Analytic Functions* (1898) came to feel that “Subsequent experience and inquiry has only strengthened our belief that if English and American students are not to be placed at a disadvantage as compared with their foreign brethren they should have ready access to text-books which discuss topics of the kind indicated above”, i.e., preliminary notions. The theory of functions having become central to mathematics, it was all the more necessary to ensure that elementary mathematics was recast in the light of recent knowledge, specifically the number concept. That they did. They had then to decide which formulation of complex function theory to present. They opted for the Weierstrassian one, quoting Weierstrass’s famous letter to Schwarz. They argued that especially for an introductory work “it was important to secure the advantages of homogeneity, intrinsic logical consistency, and the gradual passage from the simple to the complex in place of the reverse, which form so marked a feature of Weierstrass’s system”.

The result was a book that described the geometry of complex numbers, Möbius transformations, treated the logarithmic and exponential functions geometrically, and then moved into chapters on limits, convergence of series, uniform convergence, and then power series in detail. They explained the Weierstrass factorisation theorem, Laurent’s theorem, and the distinction between finite poles and essential singular points. Finally they looked at more traditional topics: elliptic functions, algebraic functions, and, in the last chapter of the book, Cauchy’s theory, the potential function, and Schwarz–Christoffel transformations. The strongly Weierstrassian character of these two books may well have motivated Fisher and Schwatt to publish their translation of Durège (1896), and Hedrick to translate Goursat *Cours d’analyse*.

The year 1896 also saw the publication of a small book on complex function theory by Thomas Fiske. Unusually for an American mathematician of the period Fiske took his some of higher education in England rather than Germany. He studied for 6 months in 1887–1888 with Forsyth, Richmond, and Glaisher in Cambridge before returning to the University of Columbia to finish his PhD, and it was the example of the London Mathematical Society (LMS) that inspired him to found founded the New York Mathematical Society in November 1888 while still a student. (Interestingly, the LMS had also been founded by students.) Fiske

worked hard to promote the Society, but it grew only slowly until he arranged for it to publish a *Bulletin* in 1891. Then it became a success, and under pressure from mathematicians in Chicago the society changed its name to the American Mathematical Society in 1894. But if the idea of a mathematical society was one he took with advantage from his time in Britain, a profound and accurate grasp of mathematics was not. His short book *Functions of a complex variable* of less than 100 pages came out in 1896 as one of eleven chapters by eleven authors in a book called *Higher Mathematics*. That attempt to launch a series failed, and Fiske reissued it, with some revisions and additions, as a book in 1906. It is generally clear and derivative without being unfailingly accurate and can charitably be described as a simplified introduction for students.

It slips almost too easily between informal description and precise technicalities, so, for example, on p. 5 a complex function of a complex variable is said to be continuous at a point when, however, the variable approaches the point the function tends to a limit. This is followed immediately by the correct $\epsilon - \delta$ definition; nothing is wrong but the transition from one formulation to another lacks a few lines of explanation. Matters become too loose on p. 52, after the correct result that the sum of a uniformly convergent series of continuous functions is continuous, when it is claimed that the sum of a uniformly convergent series of holomorphic functions on a region is holomorphic, and that term-by-term differentiation yields the derivative of the sum. These claims are true but only for compact regions or if the convergence is uniform on every compact subset of the region; worse, the term “region” is not defined in Fiske’s book.

But if we look not for depth but breadth, then Fiske did validly indicate what students could expect to find in other books at greater length: a definition of holomorphic function as one that has a derivative independent of direction, a proof that it is conformal except where its derivative vanishes, some discussion of singular points, the connection of holomorphic functions with harmonic functions and an indication of their importance, the Cauchy integral theorem and the deduction that a holomorphic function is analytic, the representation theorems of Weierstrass and Mittag-Leffler, natural boundaries, algebraic functions, and their integrals. The book, in its imperfect way, delineates what was emerging as the best route through the subject: start somewhere with Cauchy, take the power series approach from Weierstrass as the next stage up, and reserve the Riemannian approach for the study of algebraic functions and their integrals.

For English students it was naturally important that Cambridge taught a rigorous course in complex analysis. The delicate task of supplying this, in the shadow of Forsyth’s, was taken up by E.T. Whittaker, who published the first edition of his *A course of modern analysis* in 1902. In broad outline it resembles the later editions, written jointly with G.N. Watson; in particular, there is the same division into two parts: the processes of analysis and the transcendental functions. The subject matter of the first part includes a description of complex numbers and then chapters on convergence, absolute, uniform, and asymptotic (following Poincaré (1886) and Schlesinger’s *Handbuch*), and the behaviour of power series and the functions they define. The fundamental properties of analytic functions include their

Taylor and Laurent series expansions, and Liouville's theorem, a discussion on the nature of the singular points of analytic functions, the point at infinity, and multi-valued functions. In the theory of residues, the principle of the argument is proved and real integrals are evaluated. Lagrange's Theorem on the inversion of series is proved, with its extensions due to Rouché and Laplace. The transcendental functions discussed included the Gamma, Legendre, hypergeometric and Bessel functions, with applications to physics, the Weierstrassian and Jacobian elliptic functions, but not the zeta function.

Bôcher, in his review of the book (Bôcher 1904), found much to praise and some omissions to regret, notably the failure to prove theorems about indefinite integrals, such as the validity of differentiation under the integral sign. He also noted that Weierstrass's theorem that a uniformly convergent series of analytic functions converges to an analytic function was not given; it was to be given in the subsequent editions of Whittaker and Watson. More interestingly, Bôcher saw fit to comment that

While the book before us is still far from attaining the standard of rigour which French, German, and Italian writers now regard as almost a matter of course, it is a gratifying sign of progress to find in an English book such an attempt at rigorous treatment as is here made.

This comment underlines how much better placed American mathematicians were than British in attempting to catch up with their continental European peers, but it should also be noted that Whittaker's book was good enough to be improved into a classic that is still used today. This was not simply due to Whittaker's choice of topics and level of detail, which made the book in its later editions an excellent reference work, but also, to his following the English tradition of providing a large number of problems to be solved, as Bôcher noted, "by the student himself, be it understood, not, after the approved German fashion, by the writer of the treatise" (Bôcher 1904, 354).

10.8 Danish, Portuguese, Spanish, Russian, and Polish Textbooks

Other countries also imported complex function theory. In Denmark, the first to write a textbook was Julius Petersen, whose *Forelæsninger over Funktionsteori* was published in Copenhagen in 1895 and again in Copenhagen in a German translation in 1898. Petersen began with the geometry of complex numbers and stereographic projection, simple Riemann surfaces, complex integration, and more on the topology of Riemann surfaces. Then he proved Abel's theorem and the addition theorem for curves of genus 1. Then came infinite series and products, the Cauchy integral theorem with its standard implications, and the determination of functions with pre-assigned singular points. A final part of the book dealt with special functions: Γ , ζ , ϑ , Θ , elliptic functions, and modular functions.

The book was followed some years later by a book by his successor at Copenhagen. This was Niels Nielsen's *Elemente der Funktionentheorie. Vorlesungen, gehalten an der Universität Kopenhagen* of 1911, which was also based on lectures at the University of Copenhagen. The first half was, he said, a thorough reworking of his Danish textbook of 1908–1909, the three-volume *Laerebog i elementaer Funktionsteori* of 1908–1909, while the second half was more-or-less unaltered. The first half was devoted to real analysis and covered continuity, differentiation, and integration for functions of one or two variables. The second half was on complex analysis, and proceeded quickly to the Cauchy–Goursat theorem, as Nielsen called it, from which the series expansion of complex functions was deduced. In due course the Weierstrass product theorem and the Weierstrass sigma function were obtained, and then Nielsen turned to fractional linear (or Möbius) transformations and some preliminary results in conformal representation and the location of the zeros of an analytic function. A third part of the book was devoted to the elementary transcendental analytic functions, algebraic functions and their singularities, doubly periodic functions, and results about asymptotic series and the Gamma function.¹⁴ Faber in a review (*Fortschritte* 42.0412.01) noted that the laudable attempts at strict rigour made the book difficult but had not always been successful, and that there was not much emphasis on geometry, but that the many historical remarks made the book valuable. Hermann Weyl also reviewed it (Weyl 1912a) and found the book very careful in its discussion of many points when the local theory of functions was concerned, but too vague in its concept formation and the conduct of proofs when the global nature of the two-dimensional domain was involved. He also objected to the author's claim that this approach was the *only* way to a real understanding of the idea of a function.

In Portugal, the pioneer was Gomes Teixeira, whose (1892) covered complex integration, Euler integrals, elliptic integrals and functions with applications to geometry, algebraic and transcendental functions, and ended with some remarks about the calculus of variations and minimal surfaces. The opening chapter emphasises the Cauchy and Laurent theorems, but the treatment of elliptic functions starts with the inversion of an elliptic integral and derives the Weierstrass $\wp$ function. In Spain, he was followed in 1904 by Zoel G. de Galdeano, whose *Tratado de Análisis Matemático* first described the theory of real functions before treating complex function theory. He began with the Weierstrassian theory, then described Cauchy's approach, and concluded with 50 pages on Riemann surfaces and material on geometry in higher dimensions.

There does not seem to have been a textbook in Russian in the theory of analytic functions in the period, perhaps because educated Russians were expected to be fluent in French. There were, however, more advanced works, such as O.I. Somov's *Foundations of the Theory of elliptic Functions* of 1850, which Markushevich describes (Markushevich 1996, 167) as “basically an exposition of the works of

¹⁴The choice of topics may have been influenced by the fact that he had written at length about infinite series in his (1909), where he followed and extended (Pringsheim and Faber 1908).

Jacobi”. M.E. Vashchenko-Zakharchenko’s doctoral thesis on “Riemann’s theory of functions of a complex variable” (1866) was certainly more up-to-date, but we cannot tell what influence it had, if any.

We learn from Markushevich’s book (1955) that the first to lecture on analytic function theory in Russian was Y.V. Sokhotskii, who lectured on the subject at Petersburg University at the end of the 1860s, the same time as he wrote his Masters thesis on the Cauchy calculus of residues. This where he published his independent version of the Casorati–Weierstrass theorem, which says that a function takes every value at a point where it has a pole of infinite order. This confusion was typical at the time and was shared by Briot and Bouquet among others, but Markushevich argues strongly in his (1996) that Sokhotskii proved this claim by interpreting the values at the singular point as limiting values, which makes the result sound much more insightful, and that Sokhotskii at least deserves credit for being the first to appreciate the distinction between a pole of finite order and an essential singularity. It seems to us, however, that Weierstrass was already clear on the point in 1863 and one could argue that Riemann too made this distinction by only studying finite poles.¹⁵

Another noteworthy Russian mathematician active in this period was M.A. Tikhomandritskii, a professor in Khar’kov, who in 1885 published a book on hyperelliptic integrals inspired by Weierstrass’s approach. Ten years later he wrote a book on the Weierstrassian theory of elliptic functions (1895a) and another more eclectic work on the theory of Abelian integrals (1895b) in which he used ideas from Riemann surface theory and the methods of Brill and Noether. The second book was revised, enlarged, and published in French in 1911.

The mathematician Józef Pużyna was the first to publish in Polish on the subject of complex functions. He had studied in Lvov and then went to Berlin to study under Weierstrass and Kronecker. On his return to Poland he eventually became a professor of mathematics at Lvov—for many years he was the only one—and, in 1900, he was elected a member of the Academy of Sciences and Letters in Krakow. His book, in two parts *Teoria funkcji analitycznych* (1898–1900) began with an overview of set theory, the theory of power series, the definition of complex numbers. Pużyna then turned to the algebra of rational functions, and reached the theory of power series and the elementary theory of analytic functions (defined via power series) in the final two of its six sections. The second part was devoted to complex function theory, covering, in this order: the elementary functions, the Weierstrass representation theorem, algebraic functions, Weierstrass’s prime functions, topology and Riemann surfaces, the Cauchy integral theorem and its consequences, Abelian integrals and elliptic functions, harmonic functions, the Schwarzian derivative and conformal maps of triangles. He also managed to fit in an account of the work of Cousin, Poincaré and Picard on several variables, and some of his own original work. Pawlikowska–Brożek comments (Pawlikowska–Brożek 1996, 297) that “Pużyna’s work should have become a classic book on

¹⁵The information in this paragraph and the next comes entirely from the two cited works by Markushevich.

analytic functions; this did not happen because it was written in Polish, and the book remained unknown outside of Poland”.

10.9 Fifty Years of Textbooks, 1859–1909

A book review by Maschke sheds an interesting light on the situation in mathematics in 1894. After lamenting the inevitable specialisation that nonetheless impeded the discovery of the most beautiful results, he wrote ([Maschke 1894](#), 155):

There exists, however, one branch of mathematical science whose bearing on all the other parts is so evident that it appears to be, with perhaps a few exceptions, indispensable for special work of any kind—that is, the *Theory of Functions*. Even in applied mathematics it seems to gain a foothold: the theoretical astronomer as well as the mathematical physicist has frequently enough, when he wants to feel safe ground under his feet in his calculations, to fall back on the theory of functions.

Maschke then proceed to deplore the absence of a unified and complete treatment of the subject in any language.

When we turn to see how this safe and central ground was created, it is striking to see the extent to which, from the first, there was a high degree of consensus among those who adopted the Cauchy–Riemann approach. There are a variety of attempts on the vital Cauchy integral theorem, culminating in Goursat’s second proof, but the theorem is, as Kronecker suggested, the key to function theory. Inevitably it leads on to the Cauchy formula, the Cauchy residue theorem, results about power series expansions, Laurent series, and the principle of the argument. It is usual, but not obligatory, for such textbooks to reach the Cauchy integral theorem and the associated formula, Liouville’s theorem, the fundamental theorem of algebra, the characterisation of rational functions, and the constancy theorem. Rather less often, the books run to a discussion of the distinction between poles and essential singularities and the Casorati–Weierstrass theorem. On the other hand, results about analytic continuation are seldom given in detail, and the Schwarz reflection principle is almost never mentioned. It is also usual to say little more about a Taylor series once its radius of convergence has been determined, although the Cauchy–Hadamard theorem is often proved once it had become available.

In the opinion of many authors in this tradition, the elementary theory was much more Cauchy’s than Riemann’s. It was largely confined to functions defined on the plane, the Riemann sphere, or a region of the plane. Many-valued functions, such as the logarithm or the angle, were handled by cutting the plane. The classical functions were shown to belong to the theory, a variety of real integrals were evaluated. Riemann’s theory would be presented when the complex variable varied over a different surface, especially one with an obscure topology, requiring cuts. The sense one has is of Riemannian function theory being the next stage: in some books, especially the French ones, it is presented in detail; in other texts it is merely

broached. Abelian functions, being functions of several variables, are less often discussed.

The Weierstrass approach was written up much more slowly. The oft-repeated claim that there were two schools, separated by a wide gulf, is oddly hard to evaluate. Clearly, there was a gulf. It extended from the initial preferences (Cauchy–Riemann equations or convergent power series) past the use of integrals (for or against) to the very notion of what the subject is about. In the Weierstrassian scheme of things, complex function theory is about functions: the elementary functions, which are to be defined by power series; elliptic functions; special functions; ultimately, Abelian functions. It is therefore much more open to the theory of several variables. It is perhaps more attuned to point-set topology. It is certainly more interested in what happens on the circle of convergence, and in analytic continuation. But what made this gulf seem unbridgeable is less a matter of content than of style. The high Weierstrassian style is rigorously algebraic, if you like that sort of thing, and extraordinarily hostile to geometry, if you look at it from another angle. That is why Kronecker's account is so surprising, but he seems to have had a life-long predilection for Cauchy's approach.

What was at stake was Weierstrass's attempts to build up the entire subject from algebraic precepts with a minimum of additional concepts. This is the burden of his frequently quoted letter to Schwarz (see 6.4.2) and that letter must be taken very literally. It is perhaps heroic, perhaps a little perverse, but it is one man's attempt to take the central subject of his working life, one of the central mathematical topics of his day, and recast it entirely in his own terms. No wonder that it polarised the German community. And it is because this struggle was at once so personal and, within the academic mores of its day, so impersonal that references to other mathematicians are so few. Cauchy and Riemann are written out, inevitably, as the subject is reformed; but the point is not to have the name of Weierstrass writ large in their place. Weierstrass, like Galileo, believed that it could only be thus, the methods and the results what he made them to be, because they were so in reality. And one notable success was achieved that the Riemannians had to acknowledge: the theory of analytic continuation was much more subtle than Riemann had expected. Theorems on series of rational functions, natural boundaries, lacunary series, and the representation theorems of Weierstrass and Mittag-Leffler were eloquent testimony to the power of Weierstrass's approach.

Yet his attempt failed. The true home for a theory of power series became the one created by Hadamard and Borel, a major part of the advanced theory of functions. Despite the supposedly huge audiences for Weierstrass's lectures, the veneration in which he was held by so many, the books by Thomae, Biermann, Vivanti, and Harkness and Morley, the exciting new researches of Borel and others, Weierstrass's vision of the elements of complex function theory did not gain wide acceptance. The most those in the Cauchy–Riemann tradition would offer was the hope of a reconciliation, a blending of the two. That can perhaps be done, but only up to a point. Either the Cauchy–Riemann equations lie at the heart of the definition of a complex function, or a family of compatible power series does; it cannot be both.

There are many reasons why the Cauchy–Riemann approach won out. It was always securely established in France, because it was Cauchy’s. Fricke was surely right that students familiar with a course on real analysis would not see why they should start all over again in complex analysis, and throw away their hard-earned knowledge. Nor is it clear that Weierstrass’s elements are elementary, in the sense of being easy. And if Weierstrass himself was clear, the same cannot be said for Biermann. There is something intimidating about a book on complex functions that takes more than 100 pages to reach their definition. One begins to wonder if the intricacies of analytic continuation are after all any clearer to the mind than the Cauchy–Riemann equations that Weierstrass rejected. The refusal to name names denies the teacher the chance to quote results easily (not “the Casorati–Weierstrass theorem”, but “for every $r > 0$, there is”, etc). Weierstrass won the struggle on his own terms: the absolute minimum (algebra plus uniform convergence) suffices for the creation of the entire theory. But it was a Pyrrhic victory; once readers fail to see what is so wrong about complex integrals, they lose sympathy with the whole enterprise.

Every textbook is a balancing act. It must be satisfactory in its own right, and it must prepare students for the future. The Weierstrassian approach was not only good for the study of power series and what they mean, it tried to keep alive the study of complex functions of several variables. The Cauchy integral theorem did not generalise; the Cauchy–Riemann approach is naturally a one-variable theory. So it is in the Weierstrassian line that one finds many results that might belong with the theory of algebraic varieties. Yet it is surely the disdain for geometric intuition that weakened it in the end. It not only denied students that avenue of insight, it tended to make the whole Riemannian tradition literally unreadable. Therefore it risked producing mathematicians who were cut off from the ideas of Klein and his students, Poincaré, and Picard. This is evident even in small ways. It is possible to graft on to Weierstrass’s theory an account of the Riemann sphere, and so give a rigorous explanation of the composition of Möbius transformations; Biermann did and Thomae did not. But it is visibly a transplant. More significantly, by denying itself Cauchy’s path integrals, the Weierstrassian theory made the principle of the argument unavailable (which leads to the maximum principle), cut itself off from the evaluation of real integrals, and could not embrace Schwarz–Christoffel transformations.

Taken as a whole, these books have some general omissions, which show up when they are compared with a truly encyclopaedic treatise such as Forsyth’s. Although it is usual to say that an analytic function is conformal at every point where its derivative is non-zero, explicit conformal maps are seldom given. The theory of Schwarz and Christoffel is mentioned only seldom. There is very little on complex differential equations, even the hypergeometric equation. Two theorems so characteristic of complex functions are often not stated: the constancy theorem and the maximum principle. The intimate connection Riemann established between complex and harmonic functions is usually played down (although not by Picard, Bianchi, Klein or Fouët). The geometrical aspects of analytic continuation are often

left out. To get a sense of how strongly these lacks were felt we can look at some more treatments written between 1900 and the First World War.

10.10 Complex Analysis in the German and the French *Encyclopädie*

Inspired by Felix Klein, and led by F. Meyer and H. Burkhardt, the ambitious project of an encyclopaedia aiming at presenting the current status of the whole of mathematics took shape in Germany at the end of the nineteenth century. The editors strove to make it a truly international project, and indeed many of the outstanding mathematicians of the time became involved in it, no matter what their nationality. The first volume of the *Encyklopädie der mathematischen Wissenschaften*, devoted to “Arithmetic and Algebra”, appeared in 1898. Soon after its publication Julius Molk had the idea of editing a French translation of the *Encyklopädie*. He succeeded in convincing first-rate French mathematicians such as Fréchet, Borel, E. Cartan, and others to collaborate on the project. The French edition of the *Encyklopädie* was not a mere translation into French of German texts, for most of the articles were completely rewritten by the French authors, with the result that the French *Encyclopédie* was a rather different work from the German *Encyklopädie*. In complex function theory, the difference reflected the different views that the landscape of complex analysis presented in each of the two countries, and the lack of an unitary, accepted version of the theory. So looking at the way in which it was presented in the German and the French editions of the *Encyclopédie* offers an interesting picture of the subject as viewed on the opposite sides of the Rhine.

The German *Encyklopädie der mathematischen Wissenschaften* presented the chapters related to complex function theory in a rather surprising order. The theory of complex numbers was included in the volume “Arithmetik und Algebra”, as the first part of Study’s article (1898) on ordinary and higher complex quantities. An “Addition” to that volume included an article by [Pringsheim](#) (1904) on infinite processes with complex terms. Complex analysis as such was the subject of the second part of volume II. Osgood’s article on the general theory of analytic functions of one and several variables was followed by articles on algebraic functions and their integrals, elliptic and Abelian functions, and eventually (linear and non-linear) differential equations. By and large, this was the corpus of the theory as developed in the nineteenth century. (As we have seen, iteration of complex functions was treated in Pincherle’s essay (1906) on functional equations). Curiously enough, this was followed by an “Addition” to the volume which included Pringsheim and Faber’s chapter on “algebraic analysis” devoted to elementary analytic functions ([Pringsheim and Faber 1908](#)).

In the French edition this subdivision of the matter was respected only in part. Study’s article (as reworked by Cartan) was still included in the volume

“Arithmétique”, followed by Pringsheim’s article on unlimited algorithms of complex numbers edited by Fréchet. But the “Addition” on algebraic analysis by Pringsheim and Faber, edited by Molk, was moved to the opening of vol. 2 to serve as an introduction to Osgood’s article (1901) (edited into French by Boutroux and Chazy). Articles on more advanced topics such as elliptic and Abelian functions had been not translated when the French edition broke off because of the First World War, and therefore are not considered here. Nor shall we consider the original, lengthy chapter on “contemporary researches on function theory” edited by Fréchet with essays by Zoretti, Montel, and Fréchet himself, for it dealt with real variables and functions.

10.10.1 *The Arithmetics of Complex Quantities*

The content of Study’s article on ordinary and higher complex quantities would have been no surprise for readers who were familiar with Weierstrass’s lectures. Indeed, Weierstrass used to devote a great deal of his introductory lectures on analytic function theory to the arithmetics of numbers with one or several principal units (see Chap. 6 and [Weierstrass \(1884\)](#)).

Study’s article has an introductory character but, contrary to the habit of the majority of the *Encyklopädie* articles, he did not provide an extensive bibliography to the subject, and limited himself to a few references—[Hankel \(1867\)](#), a chapter from [Lie \(1893\)](#), [Berloty \(1886\)](#) and [Stolz \(1885, vol. 2\)](#)—for the theory of complex quantities with several units, “this still entirely new branch of algebra and group theory” ([Study 1898](#), 148).

The historical remarks summarised in a few lines the seventeenth and eighteenth-century approach to imaginary numbers, including a passing mention of Euler’s contributions and the statement that Gauss’s authority, as well as Abel’s and Jacobi’s works on elliptic functions, in the end overcame all doubts about complex numbers. In Study’s view, Hamilton’s theory of ordered couples represented the best way to introduce complex numbers. In addition, he mentioned Cauchy’s theory of equivalences, and eventually he introduced the geometrical interpretation of complex numbers due to Wessel, Argand and Gauss. Then Study turned to complex quantities with several units, of which Hamilton’s quaternions offered a remarkable example.

The French edition of this chapter was completely rewritten by E. Cartan ([Cartan 1908](#)) to make a chapter that was four times longer than Study’s 35 pages. Cartan’s systematic treatment of higher order complex numbers amounted to three-quarters of the whole article. As for the history of the subject, Cartan provided a remarkable account, enriched by Eneström’s erudition.

Wessel’s and Argand’s geometric approach was completed by Cartan with a reference to Bellavitis’ theory of equipollences. Then he expounded Hamilton’s arithmetic approach and Cauchy’s theory of algebraic equivalences. In a paragraph that has no counterpart in the German text [Cartan \(1908, 360–362\)](#) tackled the

question of the extension of the concept of number. He began by recalling a theorem by Weierstrass, known via an oral communication of Schwarz to Study, according to which no extension of the complex numbers is possible if one want to keep the standard properties (associativity and commutativity) of the rational operations in the algebra of real numbers. Weierstrass, it was said, had deduced this from the fundamental theorem of algebra in a course he gave in 1863. Accordingly, Cartan discussed in some detail the properties that are preserved in the theory of higher order complex numbers.

The article by Study–Cartan is followed by Fréchet’s article (1908) on “Unlimited algorithms of complex numbers”, following the German article by Pringsheim (1904). Thus, contrary to the German one, the French edition of the volume “Arithmétique” shows an evident symmetry between real and complex: to the articles “Irrational numbers and notion of limit” and “Unlimited algorithms” by Pringsheim–Molk devoted to real numbers and variables correspond the articles by Study–Cartan and Pringsheim–Fréchet on the complex case.

10.10.2 Algebraic Analysis

When the Pringsheim–Fréchet article was about to be published—the relevant issue appeared on April 2, 1908—Pringsheim (jointly with Georg Faber) was working to the article “Algebraic analysis” for the German *Encyklopädie* which was completed by the end of that year and published as an “Addition” to volume II, Part II devoted to complex analysis. At first sight, the subject was very classical, rooted in a tradition going back to Euler’s *Introductio in analysis infinitorum* (1748a) and Cauchy’s *Cours d’analyse* (1821). But a closer look at the references provided by Pringsheim and Faber shows that in their eyes the subject had a modern meaning. Algebraic analysis was nothing other than the elementary, introductory, and in short algebraic part of Weierstrass’s theory of analytic functions, against which a number of authors had reacted, including (Schlömilch 1845, 5th ed. 1873; second reprint 1888), (Lipschitz 1877–1880), (Thomae 1880, 2nd ed. 1898), (Stolz 1885 and Stolz and Gmeiner 1904–1905), (Biermann 1887), (Pincherle 1893), (Burkhardt 1903), and eventually Pringsheim’s *Leçons sur la théorie élémentaire des séries et des fonctions analytiques*, announced as being in preparation but never published.

As Faber and Pringsheim did, it is worth remarking that no treatise in English is listed among the references. According to them, the reason for the lack of English references lay in the historical development of the subject. The critical, exact approach inaugurated by Cauchy and Abel had found its completion in Weierstrass’s lectures. Accordingly, algebraic analysis had to be thought of as an introduction to Weierstrass’s theory of functions. This viewpoint explains why the complex (number and variables) stand in the foreground.

This was also Molk’s point of view. He explicitly emphasised both the “essential role” of complex quantities, and the influence of Cauchy in France with respect to the German combinatorial school. According to Molk, Weierstrass in Germany and

Méray in France, in conformity with Cauchy's approach, systematically extended the notion of analytic function—introduced by Lagrange for real variables—to the functions of a complex variable. Thus, algebraic analysis lost its peculiar status to become the basis of their theory of analytic functions. Summing up, Molk concluded, one may say that properly speaking algebraic analysis disappeared to become the elementary theory of analytic functions, as distinguished from the theory of analytic functions that is based on Cauchy's theory of complex integration.

Accordingly, one has to distinguish the “elementary theory” due to Weierstrass—and to Méray as well, Molk added with a touch of nationalism—from the “transcendental theory” where Cauchy's integral theorem plays a fundamental role. Therefore, the subject of algebraic analysis reduced to the “study of unlimited algorithms of real or complex numbers” and of the special methods that allow to represent elementary functions by means of those algorithms. As Pringsheim and Faber had done, Molk limited himself to completing the treatment given in previous articles by treating the theory of power series.

From Pringsheim–Faber's (and Molk's) point of view, the theory of power series was the first chapter of the theory of analytic functions according to Weierstrass and his school. Thus, standard results (disk of convergence, absolute and uniform convergence, derived series and so forth) were presented, including the statement of the existence of a singular point (at least) on the boundary of the disk of convergence and of the existence of “natural boundaries” for the elements of analytic functions defined by convergent series. In fact, as we noted in Sect. 8.5.1.1, far from being exceptional, the general case turned out to be the one in which the circle of convergence was the natural boundary, as Pringsheim himself proved, and Borel, Fabri, Hadamard, and others later.

Pringsheim and Faber looked at the behaviour of an entire series at a point on the boundary of the disk of convergence disk, only to conclude that study could not be pushed very far without resorting to considerations that no longer belong to algebraic analysis. Instead, more precise results could be obtained by representing the coefficients of the series by Cauchy or Fourier integrals and by studying their properties.

Among the fundamental results on entire series Pringsheim and Faber listed Cauchy's majorants, extended by Weierstrass to the case of several variables, as well as Weierstrass's identity theorem which yields the uniqueness of a power series expansion and underpins the method of undetermined coefficients.

By means of elementary, algebraic considerations the addition, multiplication and division of power series can be defined, as well as the inversion theorem for these series established “with all rigour” by Thomae in 1880. “A completely analogous theorem can be established in the case of entire series of several variables”, Pringsheim and Faber remarked. In turn, the latter theorem can be considered a particular case of a more general theorem, Weierstrass's preparation theorem, which plays a fundamental role in the theory of analytic functions of several variables. This said, Pringsheim and Faber turned to more classical subjects in algebraic analysis, such as the recurrent series and the binomial theorem (in the case of real and complex exponents) and the trigonometric and exponential

functions. The final paragraph was devoted to the introduction of the hypergeometric series, leaving its full treatment to the general theory of functions and linear differential equations.

10.10.3 Comparing Images

In spring 1911, when the first issue of Volume 2 (Analysis) devoted to complex function theory appeared, the article by Pringsheim and Faber as edited by Molk was almost all the French reader could find on this subject in the *Encyclopédie*. The same issue included three pages from the introduction to the article “Analytic functions” that is the French translation of Osgood’s German article, edited by P. Boutroux and J. Chazy. The galleys of the continuation of the article were ready for printing in 1912, but for some reason they were not published.¹⁶ Thus, when looking for a comparison with the eyes of a contemporary reader we can stop here.

What was at stake? Was the French edition of the *Encyclopédie* merely a translation of the German one was it rather a re-shaping it? The goal that Molk explicitly set himself and his collaborators was “to reproduce the entire essence” of the German articles. The outcome, the image of complex function theory that it offered, was nonetheless slightly different from the German one. Indeed, it was a combination of the prevailing images in France and Germany, respectively, a medley of the heritage of Cauchy’s and Weierstrass’s traditions, with Riemann’s geometric approach left in the background. It was a provisory mixture, well represented by Osgood’s article, and therefore destined to be outdated very soon.

10.11 Texts from 1907 to 1914

Gerhard Kowalewski, then in Prague, published his *Die Komplexe Veränderlichen und ihre Funktionen* in 1911. He began with a geometric discussion of the complex numbers and the meaning of the transformations $z' = z + a$, $z' = az$, and $z' = \frac{az+b}{cz+d}$, the latter being discussed (a little carelessly) as reflection in circles. Homogeneous coordinates, and the (Riemann) sphere were then introduced, linear transformations classified, and finite groups of them discussed, notably the icosahedral or dodecahedral and the modular group. A fundamental domain for the modular group was described. Groups of translations were then discussed, and it was shown that Möbius transformations preserve cross-ratio.

Chapter 2 introduced some of the necessary real analysis, in the form of a study of complex functions of real variables and mean value theorems for integrals of

¹⁶They were added to the reprinted edition of the *Encyclopédie* in 1992.

complex variables, and some point-set topology: accumulation points and convex hulls. The third chapter built slowly towards the foundations of the theory of functions of a complex variable. The derivative was at first defined only formally, with rules for manipulating it, and a connection was established to the fundamental theorem of algebra. Then infinite series were introduced, with a careful treatment of their convergence and term-by-term differentiation; the exponential and principal part of the logarithm function were then defined as power series. The integral along rectifiable paths was defined in Chap. 4, term-by-term integration of power series was shown to be permissible, and that of $1/z$ discussed. It was then shown that the derivative of a function exists only when the function satisfies the Cauchy–Riemann equations.

Then in the fifth chapter the Cauchy integral theorem was proved, following the approach of Goursat and Moore but only for reasonably simple contours, an approach that Weyl in his review (Weyl 1912b) found entirely consistent with the book’s stated aim of being an introduction. The familiar consequences were then derived: the Cauchy integral formula, power series expansions of analytic functions, corresponding results for harmonic functions, Laurent series expansions, Liouville’s theorem, the evaluation of definite integrals (with examples) and the inversion of series. In the final chapter, infinite series and infinite product expansions were shown to exist for analytic and harmonic functions, the Mittag-Leffler theorem was proved and applied to elliptic functions, and elliptic integrals, and finally the Weierstrass theorem was proved.

The most prominent of these texts is Osgood’s *Lehrbuch der Funktionentheorie* (1907). Written in German by an American mathematician, and published by Teubner, it should probably be regarded as a German, rather than an American, book, although G. Birkhoff recalled that Osgood’s “course on functions of a complex variable remained the key course for Harvard graduate students until World War II”.¹⁷ Osgood set out his stall in the opening words of the Preface: “The first volume of this book will give a systematic development of function theory on the basis of the infinitesimal calculus and in the closest connection with geometry and mathematical physics”.

So it will be in the Cauchy–Riemann tradition (relying as it shall on the infinitesimal calculus) and emphasise geometry and physics. It is no surprise, therefore, that the Cauchy integral theorem, Cauchy formula, Taylor and Laurent series, the fundamental theorem of algebra and Liouville’s theorem are here. But a closer look is interesting. In fact the book opens with the basic theorems of real analysis, functions of a real variable, and uniform convergence, general results about path integrals and Green’s theorems, and point-set topology. Only in Sect. 2 do we find the familiar and expected material, expounded with the concept of a Riemann surface available. The third section describes several conformal maps explicitly, and the Schwarz reflection principle for harmonic functions is given. But the bulk of it is assigned to two other topics. One is the logarithmic potential, harmonic

¹⁷Quoted in (Parshall and Rowe 1994, 13).

functions, and some implications for mathematical physics. The maximum principle is stated and proved. The other is the Weierstrassian theory of elliptic functions and the Weierstrass and Mittag-Leffler representation theorems. Osgood, it would seem, deliberately set out to enrich the literature in exactly the ways indicated above.

His American colleague Pierpont at Yale did likewise. His book, the *Functions of a Complex Variable* (1914) opens with a great deal of material on series, with some real analysis folded in. It only gets to complex analysis on page 210. When it does the approach is in the Cauchy style. The Cauchy integral theorem is proved along the lines of Green's theorem. Weierstrass's theory of elliptic functions is put before Legendre's. The strength of the book is its final 130 pages on the linear ordinary differential equations of mathematical physics in the complex domain: the hypergeometric equation in various forms, Legendre and Laplace functions, Bessel and Lamé functions.

In the same year the 15th Cambridge Tract in Mathematics was G.N. Watson's *Complex Integration and Cauchy's Theorem*. He opted to follow the texts of Goursat and de la Vallée-Poussin, but he began with a chapter on topology, notably the Jordan curve theorem. This enabled him to claim that he could manage without relying on geometrical intuition. He proved the Cauchy integral theorem along the lines of Goursat's second proof as modified by Moore (see Sect. 8.5.4), but in form he attributed to an (unpublished) argument of his Cambridge colleague Baker. Familiar topics followed: the Cauchy residue theorem, the theory of poles, residues, Liouville's theorem, evaluation of real integrals, Taylor and Laurent series expansions. The domestic audience were assumed to be familiar with Bromwich's *Infinite series* of 1908 which accounts for the lack of emphasis on Weierstrassian ideas (Bromwich's book, strongly influenced by Pringsheim's ideas, discussed the convergence, and manipulation of real and complex power series, and defined the elementary complex functions this way, but stopped short of complex function theory).

E.J. Townsend, a lecturer in mathematics at the University of Illinois, published his *Functions of a Complex Variable* in 1915. It was aimed, he said, at first-year graduate students, for whom he provided exercises but no bibliography. Perhaps he felt he was offering more or less the consensus view of the subject, as indeed we would say he was. He took a somewhat Riemannian route at the start: a function is analytic if its derivative is independent of direction. He said a single-valued analytic function is holomorphic on some domain if it has a uniquely defined derivative at every point of that domain. He gave Goursat's proof of the Cauchy integral theorem (noting the comments of Moore and Pringsheim) and deduced the Cauchy integral formula and its important consequence that the derivative of a holomorphic function is continuous on its domain and indeed holomorphic itself. He now commented that a holomorphic function satisfies the Cauchy–Riemann equations and, conversely, if a function satisfies the Cauchy–Riemann equations and has continuous partial derivatives then it is holomorphic. He then turned to more specific matters for a while, starting with the associated subject of harmonic functions and their applications in physics. He then looked at conformal mappings, the trigonometric and hyperbolic functions, and linear fractional transformations.

Then the treatment became more general once more: he took up the Weierstrassian approach via infinite series, proved that a holomorphic function admits a power series expansion (and that a function defined by a power series is holomorphic), proved Mittag-Leffler's theorem, and ended up by discussing algebraic functions and their Riemann surfaces.

A final indication is the lecture course given by Hadamard to the École Polytechnique in 1913–1914. His *Cours d'analyse* for the second-year students reflects an important debate that had flared up about the place of mathematics in the syllabus of the École Polytechnique, which concluded that physics should be held in high esteem (see [Gispert 2007](#)). Hadamard began with a discussion of real first-order differential equations, then turned higher-order equations and systems of differential equations, before concluding Part I with a discussion of the elementary theory of linear differential equations up to the idea of an adjoint system. He followed this with an account of potential theory and harmonic functions, with applications to electro-statics and magnetism.

He took up complex function theory in Part III. A function of $x + iy$ was said to be an analytic function when its derivative was independent of direction or, he said, equivalently if the Cauchy–Riemann equations were satisfied. He offered a proof later that no hypothesis on the continuity of the partial derivatives was necessary. Hadamard gave a variety of examples of single- and many-valued analytic functions before defining a function to be holomorphic in a region if it was defined and continuous at every point of the domain and had a continuous derivative at every point of the domain. He defined a singular point as a point such that the function was not holomorphic on any disc centred on that point. A pole is a singular point for a function f that is an ordinary point, indeed a zero, of the reciprocal $1/f$. A point is a critical point if it is a branch point. And a function is meromorphic in a region if its only singular points in the region are poles.

Hadamard then ran through the fundamentals of the subject. He defined a complex integral and deduced the Cauchy integral theorem as a consequence of Riemann's formula (i.e., Green's theorem), noting Goursat's observation in his own *Cours d'analyse* that no hypothesis was required on the derivative other than that it exists, whence, said Hadamard, it was not necessary to make any other hypothesis on the derivative for a function to be analytic. Next came the elementary theory of power series, with the observation that inside its circle of convergence a power series converges uniformly, and results about double series.¹⁸ Then came the Cauchy integral formula and the deduction that a holomorphic function is analytic, then the development of the Taylor series of a function, convergence theorems, the theory of analytic continuation and the connection to the theory of analytic functions à la Weierstrass, and the observation that harmonic functions are analytic. The theory of Laurent series was presented, and the exposition of the fundamentals ended with an account of residues and the logarithmic integral, the principle of the argument,

¹⁸In connection with double integrals, not to establish the theory of complex functions in the Weierstrassian manner.

Liouville's theorem, and as a consequence a proof of the fundamental theorem of algebra (d'Alembert's theorem, as Hadamard called in the French manner).

The next chapter of the *Cours* covered the evaluation of definite integrals and the rudiments of the theory of algebraic integrals. Only the most elementary examples were discussed before Hadamard moved to the final topic: Weierstrass's theory of elliptic functions and the connection to the theory of theta functions. An appendix offered an account of the Dirichlet problem for the circle and the sphere, and Part IV of the *Cours* considered analytic differential equations, with an emphasis on electromagnetic waves, the Maxwell–Hertz theory and the theory of light.

Hadamard published his *Cours d'analyse* in 1930. By that time it had acquired a different beginning, with Parts on potential theory and the calculus of variations. The material on differential equations moved to the Part on applications, which included Fresnel integrals and elliptic functions as well. The material on complex function theory still formed Part III and was largely unchanged: the by-then familiar definition of an analytic function, some examples including the Riemann surface of $\sqrt{z}$, the Cauchy integral theorem and its standard consequences, Taylor series, analytic continuation, Laurent series, the residue calculus, Lagrange series and Kepler's equation, finishing, as in 1913–1914 with Liouville's theorem.

Two British writers may now be mentioned. Thomas MacRobert was lecturer in mathematics at the University of Glasgow (he became a professor there in 1927 and retired in 1954), and in 1917 he published his *Theory of Functions of a Complex Variable* with a view to introducing beginners to the theory. He therefore preferred geometric methods to arithmetical ones, and after giving an account of the elementary fundamentals of the subject he moved through the familiar examples of holomorphic functions to Green's theorem, the Cauchy integral theorem, and contour integration. He referred readers to Bromwich's book for all but the simplest aspects of the theory of infinite series, although he did discuss uniform convergence, and turned instead to applications: analytic continuation, asymptotic expansions of the Gamma and Bessel functions, and the Weierstrassian and Jacobian theories of elliptic integrals and elliptic functions. The book ends with four chapters on linear differential equations.

Carmichael reviewed the book in (Carmichael 1919). He liked the opening chapters, was unhappy with the treatment of the Gamma function, and enjoyed the treatment of the elliptic functions. His most significant criticism was that the book often stated matters too loosely, and it is interesting to see that these faults occur when what we would say were topological matters were under discussion; further evidence that topology was growing up underneath complex function theory and refining it.

The other writer worth looking at is H.F. Baker, who wrote the entry on "Function" in the 11th edition of *Encyclopedia Britannica* (1910–1911), vol XII, pp. 310–329. Twenty pages of the essay are given over to a brisk run through complex function theory, from the definition of complex numbers to the theory of several variables. Baker began with the Cauchy version, the Cauchy integral theorem and Laurent series and the classification of singular points. Then he gave Weierstrass's definition of an analytic function on the grounds that it was the

simplest definition of a monogenic function of a complex variable, and proceeded to the elementary properties of such functions: the Mittag-Leffler and Weierstrass theorems. Then he discussed the construction of functions with a given region of existence, the representation of functions by rational functions, and Mittag-Leffler's star theorem. Next came elliptic functions, harmonic functions, conformal properties of complex functions including the Schwarz–Christoffel theorem. Then came algebraic functions, elliptic integrals, modular functions, integral of algebraic functions, and a hint of the theory of algebraic surfaces, which was just beginning to engage Baker's research interests.

At the end of the essay he discussed the available literature: Goursat's *Cours d'analyse* as an introduction, followed by Forsyth for more detail, Osgood's *Lehrbuch* for "greater logical rigour in the more difficult theorems" (a tactful way of handling criticisms of his former Cambridge colleague recently and scandalously forced to leave Cambridge for Imperial College, London) and W.H. and G. Chisholm Young's *Theory of Sets of Points* for "greater precision in regard to the necessary quasi-geometrical axioms". It would seem that Baker had shifted over the years away from the Riemannian inclinations of his youth (in his *Abelian Functions*) towards the merits of the more algebraic methods of Weierstrass, but had not taken on board the further developments of Borel and others in the French school.

These books strengthen the view that there was a growing consensus around several issues. There was a subject—complex function theory—which could and should be studied for its own sake. It had a coherent story that started with the definition of complex differentiability, proceeded along the lines of Cauchy and the elementary parts of Riemann's theory to the point where it could be shown that holomorphic functions are analytic, and then turned to the insights of the Weierstrassian approach. Before then results such as Liouville's and the fundamental theorem of algebra might be picked up; after then perhaps Picard's little theorem. This left the author room to decide what to decorate the account with: Möbius transformations, perhaps, and what to point to at the end. It would be possible to amplify the connection with potential theory, to open the door to the study of elliptic functions, or the special functions—all topics that it was agreed were the neighbours of this vigorous new subject: complex function theory.

10.12 The German Classics

These are the books Hurwitz–Courant *Vorlesungen über allgemeine Funktionentheorie und elliptische Funktionen* (1922), Bieberbach, *Lehrbuch der Funktionentheorie* (1934), and Knopp *Funktionentheorie* (1913), to which we add Pincherle *Gli elementi della teoria delle funzioni analitiche* (1922).

We have come almost full circle, to the classic texts esteemed by Lang. It remains to see how one tradition was produced out of what plainly were two, or at least two extremes. And it is clear what can be done. Because, as Fricke pointed out, most students meet complex analysis after a course in real analysis, Weierstrass's

objection to integrals and Cauchy–Riemann equations loses much of its force. On the other hand, the Cauchy theory can produce the power series expansions of analytic functions defined in its own way. So the definition of analytic as a function that has a derivative independent of direction can move up front, the Weierstrassian theory of analytic continuation can come later, and the elementary formal algebra of series be taught wherever pedagogy suggests. The characteristic properties of complex functions should be emphasised (meanwhile, it might be added, the theory of real functions of several variables is failing to make it to centre stage). Contemporary research enlarges the range and depth of advanced topics which at the same time can be marked off as such. The points of separation may vary, but not by much.

We shall first look briefly at the Italian example, Pincherle's *Gli elementi della teoria delle funzioni analitiche*, (1922), because it spells out the kind of authorial exercise we have in mind. Hardly surprisingly, the book shows several points of departure from the lecture course of 23 years before. The book begins with a definition of complex numbers and their geometrical representation on the plane and the Riemann sphere, and the elementary theory of Möbius transformations. Then he brings forward the definition of an analytic function, which is now that it is differentiable (previously it satisfied the Cauchy–Riemann equations) but the reader must wait for the Cauchy integral theorem. First come two chapters on power series, including Taylor series, the Cauchy–Hadamard theorem, and Weierstrass's theorem, followed by some examples of well-known analytic functions. In Chap. VI the Cauchy integral theorem proved (again with two proofs: via Green's theorem, and Goursat's way), and the usual consequences deduced (Laurent series, residues, the principle of the argument). Liouville's theorem is not proved, but the fundamental theorem of algebra is, and the characterisation of rational functions is given. Then, as before, there followed more recent material: entire functions, Mittag-Leffler's theorem, special functions (Γ , ζ , and $\wp$), and an extensive treatment of algebraic functions, Riemann surfaces, and Abelian integrals, elliptic functions, from the Riemannian, Weierstrassian, and Jacobian points of view. But the book concludes with the hypergeometric equation and the Gamma function. Almost all of the main structure of a modern textbook is here, down to the bench marks.

The classic text of Hurwitz–Courant (1922) (published by Courant after Hurwitz's death in 1919) might also seem to exemplify this trend. It takes Weierstrass's ideas as far as the Mittag-Leffler theorem, for example. But its full title makes the point clear: *Allgemeine Funktionentheorie und elliptische Funktionen*. General function theory and elliptic functions, written by Hurwitz. To this Courant added material on geometric function theory. The boundary markers are evident, but the content is unexpected: the theory of functions is Weierstrass's. Cauchy's ideas enter only in Chap. 5, Riemann's only when dealing with elliptic functions.

The books that finally presented the modern synthesis were Knopp's *Funktionentheorie* (1913) and his *Theorie und Anwendung der unendlichen Reihen* (1922), the two volumes of Bieberbach's *Lehrbuch der Funktionentheorie* (vol. 1 (1921b), vol. 2 (1931)) reprinted with minor, if unpleasant, modifications in 1934. Knopp's little book was very successful, it ran to five German editions and was

translated into English in 1945.¹⁹ The general theory is in the Cauchy style: analytic functions by definition are differentiable; it follows that they satisfy the Cauchy–Riemann equations. The Cauchy integral theorem and Cauchy formula apply, power series expansions are deduced including the Laurent series, analytic continuation is explained, entire transcendental functions are introduced and the classification of singular points is dealt with. Beyond lie entire functions, meromorphic functions, simply and doubly periodic functions, many-valued functions, algebraic functions, and Riemann surfaces.

Bieberbach’s approach was similar, but it was insisted upon somewhat sternly. In his Preface he asked: “What is a Text-book? It should, I think, be a complete, comprehensible, and unified presentation of a scientific domain. These three properties . . . can be made more precise” and he explained how a textbook should differ from a handbook. It should be complete only in its explanation of the essential routes through its subject, not because it explains every detail of the entire subject. It should have a minimum of prerequisites, in respect of both theorems and methods and skills and practice. And it should be unified, which, in the present context might seem to run counter to the need for completeness. “any one who has once heard the old battle cries: here Riemann, here Weierstrass, here Cauchy, will have their doubts”. Having thus raised the stakes, Bieberbach did it his way.

Complex numbers were defined and explained geometrically, and Möbius transformations shown to generate attractive conformal maps. Material on the convergence of series was developed, as far as a Tauberian theorem. Then analytic functions were defined in terms of differentiability, and the Cauchy–Riemann equations deduced. conformality was briefly discussed, and then 50 pages were given over to a careful account of the elementary functions from this point of view. The example of the conformal transformation $w = \frac{1}{2} \left(z + \frac{1}{z} \right)$ was given (it maps a system of confocal ellipses and hyperbolas onto a rectangular grid, and it is traditional, being found also in Thomae’s *Abriss* and Burkhardt (and Zhukovskii, as discussed on p. 551 above), who uses it to conjugate a transformation to $w = z^2$). Then we get the Cauchy theory, with the Cauchy integral theorem distinguished as the fundamental theorem of function theory. All the by now familiar consequences are there, for example the maximum principle and the constancy theorem. The Casorati–Weierstrass theorem is stated, although it is given no name; perhaps because Bieberbach proved Picard’s little theorem in the second volume. In particular, the different types of singular points are classified, Liouville’s theorem proved and used to establish the fundamental theorem of algebra and the characterisation of rational functions.

Then we get more on power series, including Vitali’s theorem, and more on residues, including the principle of the argument and Rouché’s theorem. Inversion of functions, implicit functions, and Kepler’s equation are treated. After that, a chapter on analytic continuation, including the reflection principle, and information

¹⁹It was published in two parts: *Grundlagen der allgemeinen Theorie der analytischen Funktionen* and *Anwendungen und Weiterführung der allgemeinen Theorie*.

about Riemann surfaces and algebraic functions. All this in Bieberbach's sense of completeness, which means to give the beginner enough to get started without too much detail. A feature of the exposition is that sometimes different proofs are given of important results: for example, Gauss's third proof of the fundamental theorem of algebra and also Cauchy's proof of that result. The book ends with a short treatment of elliptic functions, theorems by Mittag-Leffler and Runge on the representation of analytic functions, and a chapter on the Gamma function.

It will be noted that the picture we built up of the standard modern textbook is all here. The main theorems and the route connecting them, and all the familiar features of the modern treatment are present. This is a measure of Bieberbach's success. He was helped in this by being a very clear and stimulating writer. Other authors followed suit. The main difference with today is the wealth of extra material Bieberbach provided, and the topics he included that today would be regarded as specialist options (for example, elliptic function theory). But his lucid vision of what a textbook should be, and his exemplification of it, have served to define the subject for 60 years. Apart from the introduction of section headings and a pruning of the preface, the minor revisions he made in 1934 reflect Bieberbach's Nazi views: Gauss's proof of the fundamental theorem of algebra is explicitly said to be "long before Cauchy's", and the claim that the essential content of the Weierstrass preparation theorem can be found in Cauchy is likewise no longer made.

We end this discussion, as we very nearly began, with the book Ahlfors singled out: Copson's textbook (1935). It too was indicative of the trend we have been following. Like many a subsequent treatment, it assumed that students had already studied enough real analysis. It defined complex numbers via matrices and deduced their arithmetic rules before it gave the geometric description. Then it took the "Cauchy" route, turning to power series and their convergence theory in Chap. 5, and reaching the residue theorem in Chap. 6. There followed chapters in the Weierstrass representation theorem, conformal representation and Schwarz–Christoffel transformations, and then came an account of the hypergeometric equation. It did not go in the direction of contemporary work, such as Nevanlinna theory, but concluded by promoting the traditional topics of the special functions, the Gamma function, and elliptic functions (Weierstrassian and then Jacobian, as Whittaker and Watson had done before). The final flourish was a treatment of Picard's theorems.

10.13 Conclusion

In the 1890s the subject of complex function theory became a mainstream one in almost all countries with a commitment to advanced mathematics. The pages of the *Bulletin of the American Mathematical Society* and the other journals that reported on university courses internationally make it clear that many universities in Europe and America now ran lecture courses in the subject, and we have seen that a large textbook tradition had grown up accordingly.

The central position of complex function theory by 1900 is worth analysing in several respects. One can ask if it eclipsed real function theory, and if so, why? It may be too strong to say eclipsed, but it certainly shone as brightly. The French tradition begun by Cauchy had placed real analysis securely in the syllabus of the École Polytechnique but left complex function theory scattered on the margins. But it ended in a profusion of *Cours d'analyse* that more-or-less devoted one volume to real analysis, one to complex analysis, and one to differential equations. The German situation was even stranger, because Weierstrass's marked preference for complex analysis over real analysis had left the two disciplines unevenly developed in Berlin, and no-one else in Germany had emerged as a champion of real analysis. Modern Italian textbooks in analysis had begun with Casorati's book on complex analysis and only later did authors turn to real analysis.

Reasons for this distribution of effort and its implicit preferences are not hard to seek. If one takes the theory of elliptic functions as an attractive research topic in complex analysis, and the theory and use of Fourier series as a comparable pole in real analysis, one sees a curious shift in phase. From the 1860s to 1914 complex analysis was a subject taking shape: the Cauchy–Riemann equations were generally seen as a suitable starting point, Weierstrass's theory of complex functions could be grafted easily onto a Cauchy–Riemannian base to provide a rigorous theory of complex functions, the Weierstrassian theory of elliptic functions was appreciated as a marvel of elegance and simplicity, and elliptic functions had a charm and a use that was apparent in many branches of mathematics. In the same period the theory of real analysis was grappling with difficulties and its practitioners were unable to forge a consensus. The real numbers were understood for the first time but at the price of admitting naïve set theory, Cantorian transfinite set theory was far from being popular, there were increasing difficulties with the Riemann integral, and eventually the fundamental theorem of the calculus lapsed. Fourier series were shown to possess highly unexpected and counter-intuitive properties. This is rich and fascinating mathematics, but it does not make easily for the growth of a discipline, and the situation was not going to change until the work of Borel and the other French analysts, especially Lebesgue's invention of measure theory and his new formulation of the integral, and the advent of functional analysis.

Furthermore, by an accident of development, many elementary theorems in the analysis of functions of several real variables were discovered only late, so the profound obstacles in the path of a complex function theory of several variables were matched, as it were, by a degree of simple ignorance.

More substantially, there was the question of how much real analysis should be assumed of a student beginning to study complex analysis. This was the most substantial challenge to the textbook tradition in complex analysis, and it could have strengthened a tendency to regard real analysis as fundamental and complex analysis as dependent upon it. However, it proved possible to get a long way in complex analysis with either a flawed invocation of real analysis or else a simple-minded confidence that the necessary real analysis could be made to work. Moreover, deep issues in complex function theory turned out to have a tendency to undercut real analysis: issues of connectedness, path-connectedness, the Jordan curve theorem,

the definition of a neighbourhood, and of course the very idea of a Riemann surface all spoke to topology, not merely to a need for a better understanding of real functions.

Rigorous complex analysis in 1900 was a teachable subject; it had a structured and coherent set of ideas and techniques and a range of applications, pure and applied. Real analysis, by contrast, was not so obviously easy to begin, nor did it have such a convenient shape. Should one start with naïve set theory, with the integers, with a naïve appreciation of the real line and epsilon–delta analysis? Should one aim for Fourier series, for differential equations, for a theory of the integral? It can be argued that the modern answers to these questions all agree in finding real analysis more fundamental, but in the period considered here they could not act to marginalise the growth of complex analysis.

Complex function theory also had its powerful proponents. Gauss was the first, and anyone drawn to number theory after him fell under the same spell: Dirichlet, Kronecker, Hermite. Algebraic geometry worked so much better over the complex numbers than the reals that there was very little discussion of the real case. Whatever Klein may have thought of the problems with Berlin's arithmetisation of analysis he opposed it with intuition and in his own research was drawn strongly to a mixture of geometry and complex automorphic function theory. Nor did anyone think that the well was running dry, as the number of topics that were pursued successfully between 1900 and 1914 make clear.

So it is not surprising that complex analysis not only rose to eminence as a mathematical discipline but also achieved a striking degree of consensus. The German authors mentioned by Lang–Hurwitz–Courant, Knopp, Bieberbach—were indeed in a position to write books that are still worth consulting, and which, their language apart, could still be used in British and American universities today—although they might be found rather demanding. The same can seldom be said of other subjects. Euler surely did not expect, when he wrote the first great textbook in mathematics, his *Introduction in analysin infinitorum*, that his unification of the exponential and trigonometric functions would lead to the complex analysis of 150 years later, but his use of imaginaries was to be the first of many occasions when complex analysis proved to be fundamentally the right way to go.

Bibliography

The annals of bibliography afford many examples of the delirious extent to which book-fancying can go.

Emerson, *Society & Solitude* viii, 168.

The following abbreviations of book and journal titles have been used:

Acta = *Acta Mathematica*

Acta Soc. Sci. Fennicae = *Acta Societatis Scientiarum Fennicae*

Acta Szeged = *Acta Scientiarum Mathematicarum Universitatis Szegediensis*

AHES = *Archive for history of exact sciences*

AMS = *American Mathematical Society*

Amer. J. Math. = *American Journal of Mathematics*

Amer. Math. Monthly = *American Mathematical Monthly*

Ann. di Mat. = *Annali di matematica pura e applicata*

Ann. Sci. Mat. Fis. = *Annali di scienze matematiche e fisiche*

Ann. de math. = *Annales de mathématiques pures et appliquées*

Annales ENS = *Annales scientifiques de l'École Normale Supérieure*

Archives sci. phys. nat. = *Archives des sciences physiques et naturelles*

Astr. Nachr. = *Astronomische Nachrichten*

Atti Torino = *Atti della R. Accademia delle Scienze di Torino*

Likewise *Mem. Torino*

Berlin Abh. = *Abhandlungen der K. Preussischen Akademie der Wissenschaften zu Berlin*

Berlin Berichte = *Sitzungsberichte der K. Preussischen Akademie der Wissenschaften zu Berlin*

Bibl. Ital. = *Biblioteca Italiana ossia Giornale di letteratura scienze ed arti*

Bordeaux Mémoires = *Mémoires de la société des sciences physiques et naturelles de Bordeaux*

Bull. Acad. Belgique = *Bulletin de l'Académie royale des Sciences, des Lettres et des Beaux-Arts de Belgique*

Likewise *Mém. Acad. Belgique*

Bull. AMS = *Bulletin of the American Mathematical Society*

Likewise *Trans. AMS, Notices AMS*

Bull. Férussac = *Bulletin universel des sciences et de l'industrie: sciences mathématiques, physiques et chimiques*

Bull. LMS = *Bulletin of London Mathematical Society*

Likewise *J LMS, Proc. LMS*

Bull. Sci. Math. = *Bulletin des sciences mathématiques*

- Bull. SMF* = *Bulletin de la Société Mathématique de France*
Bull. Soc. Math. Belg. = *Bulletin de la Société Mathématique de Belgique*
Bull. Soc. Philom. = *Bulletin de la Société Philomatique (Paris)*
Likewise *Nouv. Bull. Soc. Philom.*, *Procès-Verbaux Soc. Philom.*
Camb. math. J = *The Cambridge mathematical journal*
Camb. Dubl. math. J = *The Cambridge and Dublin mathematical journal*
CMP = *Collected Mathematical Papers*¹
Coll. Papers = *Collected Papers*
Comm. Acad. Sci. Petrop. = *Commentarii Academiae scientiarum imperialis Petropolitanae*
Likewise *Novi Comm. Acad. Sci. Petrop.*, *Acta Acad. Sci. Petrop.*, *Nova Acta Acad. Sci. Petrop.*,
Mém. Acad. Sci. St. Petersburg
Comm. Soc. Reg. Gött. = *Commentationes (recentiores) societatis regiae scientiarum Göttingensis*
CR = *Comptes rendus de l'Académie des Sciences*
CUP = Cambridge University Press
CW = *Collected Works*
DSB = *Dictionary of Scientific Biography*. Scribners, New York
EMW = *Encyclopädie der mathematischen Wissenschaften*
ESM = *Encyclopédie des sciences mathématiques*
Ex. An. Phys. Math. = *Exercices d'analyse et de physique mathématique*
Ex. Math. = *Exercices de mathématiques*
Ges. Abh. = *Gesammelte Abhandlungen*
Ges. Math. Abh. = *Gesammelte mathematische Abhandlungen*
Ges. Math. Werke = *Gesammelte mathematische Werke*
G. di Mat. = *Giornale di matematiche*
Göttingen Nachr. = *Nachrichten der K. Gesellschaft der Wissenschaften zu Göttingen*.
Mathematisch-Physikalische Klasse
Likewise *Göttingen Abh.*
Hist. Acad. Sci. Berlin = *Histoire de l'Académie royale des sciences et des belles-lettres de Berlin*
Likewise *Mem. Acad. Sci. Berlin*, *Nouv. Mem. Acad. Sci. Berlin*
Hist. Acad. Sci. Paris = *Histoire et mémoires de l'Académie royale des sciences*
Likewise *Mém. Acad. Sci. Paris*, *Mém. Inst. France*
HM = *Historia Mathematica*
HMATH = *American Mathematical Society and London Mathematical Society. Series in the history of mathematics*. Providence, RI.
Istor.-Mat. Issled. = *Istoriko-Matematicheskie Issledovaniya*.
Jahrbuch = *Jahrbuch über die Fortschritte der Mathematik*.
J de math. = *Journal de mathématiques pures et appliquées*
JDMV = *Jahresbericht der Deutschen Mathematiker-Vereinigung*
J Ec. Poly. = *Journal de l'École Polytechnique*
Likewise *Corresp. Ec. Poly.*
JfM = *Journal für die reine und angewandte Mathematik*
Leipzig Berichte = *Berichte über die Verhandlungen der K. Sächsischen Gesellschaft der Wissenschaften zu Leipzig. Mathematisch-physische Klasse*
Likewise *Leipzig Abh.*
LMS = *London Mathematical Society*
Math. Abh. = *Mathematische Abhandlungen*
Math. Ann. = *Mathematische Annalen*

¹Papers included in *Collected Mathematical Papers*, *Collected Papers*, *Gesammelte Werke*, and the like, are referred in the text to the latter's page numbering.

Math. Papers = *Mathematical Papers*

Math. Werke = *Mathematische Werke*

Math. Z. = *Mathematische Zeitschrift*

Mem. Bologna = *Memorie della R. Accademia delle scienze dell'Istituto di Bologna*

Likewise *Rend. Bologna*

Mem. Lincei = *Atti della R. Accademia dei Lincei. Classe di Scienze fisiche, matematiche e naturali. Memorie*

Likewise *Rend. Lincei*

Mem. Soc. Ital. Sci. = *Memorie della Società Italiana delle Scienze*

Misc. Taurin. = *Miscellanea philosophico-mathematica Societatis privatae Taurinensis*

Monatsberichte Berlin = *Monatsberichte der K. Preussischen Akademie der Wissenschaften zu Berlin*

Monatshefte Math. Phys. = *Monatshefte für Mathematik und Physik*

München Abh. = *Abhandlungen der mathematisch-physikalischen Classe der K. Bayerische Akademie der Wissenschaften zu München*

Natur. Gesell. Zürich = *Vierteljahrsschrift der Naturforschenden Gesellschaft in Zürich*

Nouv. Ann. Math. = *Nouvelles Annales des Mathématiques*

O.C. = *Oeuvres complètes*

O.O. = *Opera omnia*

OUP = Oxford University Press

Op. Mat. = *Opere matematiche*

Opusc. Math. = *Opusculs mathématiques*

Phil. Mag. = *The Philosophical Magazine*

Phil. Trans. Roy. Soc. London = *Philosophical Transactions of the Royal Society of London*

Likewise *Proc. Roy. Soc. London*

Proc. Camb. Phil. Soc. = *Proceedings of the Cambridge Philosophical Society*

Likewise *Trans. Camb. Phil. Soc.*

Rend. Lombardo = *Rendiconti del R. Istituto Lombardo di Scienze e Lettere*

Rend. Palermo = *Rendiconti del Circolo matematico di Palermo*

Likewise *Suppl. Rend. Palermo*

Rend. Sem. Mat. Torino = *Rendinconti del Seminario Matematico di Torino*

Sitz. München = *Sitzungsberichte der mathematisch-physikalischen Classe der K. Bayerische Akademie der Wissenschaften zu München*

Wiss. Abh. = *Wissenschaftliche Abhandlungen*

ZfM = *Zeitschrift für Mathematik*

ZMP = *Zeitschrift für Mathematik und Physik*

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