Poincaré and complex function theory

Book Section

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1 Introduction

Poincaré is still well known for the mathematical work that first made his name : his discovery in 1880–1881 of automorphic functions. New documents and insights were added in [16], which can also be consulted for references to the well-known history of his work in this area. He is also remembered for one further theorem that grew out of that early work : the uniformisation theorem, which he sketched a proof of in 1883 and then proved rigorously in 1907, as did Koebe independently.[1] The rest of his numerous contributions to complex function theory are more scattered and do not seem to have been the focus of much attention.[2] In this paper I survey what he did and argue that they tell an eloquent story not only about the state of the subject in the years around 1900 but about Poincaré's place in the mathematical community of his day. To understand either of these it is necessary to give a quick summary of the prior development of complex function theory, which was growing rapidly into a central topic in all mathematics, and that will occupy the first half of this paper. The second half will consider Poincaré's contributions. We will see that although he was actively involved in many aspects of the subject, his influence is scarcely to be noticed in the many books that were published, and I will investigate why that was and what it may tell us about relationship between research and teaching in the years around 1900.[3]

2 The first textbooks on complex function theory

Cauchy was the first mathematician to appreciate that within the class of functions from $\mathbb{R}^2$ to $\mathbb{R}^2$ there is a significant subclass of functions from $\mathbb{C}$ to $\mathbb{C}$ and to begin to spell out their distinctive properties. He appreciated complex function theory on intrinsic grounds : it was a new subject,

[1] At great length in [24] and numerous other publications of the time.
[2] For an account from a modern mathematical perspective, see [55].
[3] The first full-length history of complex function theory, in which the issues in this essay are explored in greater detail, is given in [6] (to appear).

P.-E. Bour, M. Rebuschi & L. Rollet (eds.), *Construction, 1–20.*

with fascinating implications for evaluating integrals, but essentially the theory was about what happens when you do mathematical analysis with complex variables. Riemann, on the other hand, was developing complex function theory in order to do something else : to study Abelian functions, differential equations and the distribution of the prime numbers. His appreciation of the merits of the new subject was not Cauchy's. Weierstrass began closer to Riemann's opinion, and indeed on almost the same theme, hyper-elliptic functions, which are a special case of Abelian functions. Later, and consistent with his position in Berlin, he was to build up the subject of complex function theory in its own right (thus endorsing Cauchy's opinion if not his methods) and to put it to use.

So for Riemann and Weierstrass complex function theory, whatever it might be, was a preliminary. The real focus of interest was Abelian functions, a vast and misty generalisation of elliptic functions that had themselves only been in the literature since the late 1820s (the famous work of Abel and Jacobi). Even these functions were not properly understood, and one of the great forces promoting complex function theory was the idea that it would make good sense of the elliptic functions themselves. For, on any account and however formulated, elliptic function theory is about complex-valued functions of a complex variable.

The task of isolating and pulling together the foundations of a theory of complex functions of a complex variable, which interested Cauchy only slightly, was carried to success by two of his close followers and co-religionists, Briot and Bouquet, who, like Cauchy, were attracted to the Catholic Right and the Jesuits, and therefore close to Cauchy in the master's final years. Their book [8] has the distinction of being the first on the subject of complex function theory. 40 of its 326 pages set out the general theory, the rest puts it to work to define the elliptic functions in this way and deduce their major properties, going via the theory of differential equations to elliptic functions as doubly periodic functions.

The first textbook in German on complex function theory was written not many years later. The author was Heinrich Durège, a friend and colleague of Riemann's who had passed through Göttingen, and who also had access to a set of Riemann's lectures published by Gustav Roch [**?**]. Durège's textbook [14] proved to be quite successful, it ran to four editions in his lifetime and was translated into English for the American market in 1896. Mention should also be made of the book by Schlömilch (who, by the way, was the person who encouraged the young Roch to go to Göttingen and study with Riemann). His *Vorlesungen* [48] contains enough material on the subject to count as only the third book on complex function theory to be published, and it ran to several editions. Pages 35 to 111 cover functions of a complex variable, and further chapters look at elliptic integrals and elliptic functions. He, somewhat like Briot and Bouquet, developed the theory of complex function and then used the theory of Riemann surfaces to deduce the properties of elliptic functions from the elliptic integrals.

The books by Briot and Bouquet, Durège, and Schlömilch did more than put elliptic function theory on a sound footing. They established a textbook subject – complex function theory – with reasons for studying it. The subject was more than a preliminary : it had its own methods, distinct from the theory of functions from $\mathbb{R}^2$ to $\mathbb{R}^2$, and its own charm (the residue theorem) quite independent of the fact that it grounded elliptic function theory. With these books it became possible to speak of a genuine new subject within mathematics.

Briot and Bouquet's route up Mont Cauchy proceeded as follows. They said a function is monodromic if it is single-valued in its domain, and monogenic if it is complex differentiable. They then showed that such a function satisfies the Cauchy-Riemann equations, an observation which is a commonplace today but the significance of which had only dawned on Cauchy in the late 1840s. Briot and Bouquet added that such a function is also conformal when the derivative does not vanish. They then defined a function to be synectic (the modern word is holomorphic)n if it is monodromic, monogenic, finite and continuous in the entire plane.

Throughout this period among the canonical examples of complex functions were such functions as square roots, and quite generally nth roots and the logarithm function, none of which are single-valued on the entire complex plane. For that reason they are not considered functions in the modern sense of the term, but they were then, and Cauchy had dealt with them by first cutting the plane to reduce them to a single-valued branch on the cut plane, which could then be studied by letting the independent variable move in the plane and cross the cut. This ad hoc solution to a genuine difficulty was the best that Cauchy had been able to come up with, and Briot and Bouquet could do no better.

Central to Cauchy's theory was the study of the integral of a single-valued complex function taken around a closed path. Briot and Bouquet gave a proof using the calculus of variations to show that a function which is synectic in a portion of the plane if has an integral along arcs in that domain that depends only on the end points. From this they deduced the Cauchy integral theorem : if $f(z)$ is a synectic function in a domain, ζ. is a point in that domain, and γ is a simple closed curve in that domain that enclosed ζ then

$$f(\zeta) = \frac{1}{2\pi i}\int_\gamma \frac{f(t)drt}{z-\zeta}.$$

As they then quickly showed, still following Cauchy, it follows that synectic function is infinitely differentiable, and (by a convergence argument akin to the maximum modulus principle) a function synectic in a disc has a convergent power series expansion in the disc. Moreover, the derivatives of a synectic function are synectic. Cauchy had also shown how to deal with – the concept of residue and the terminology were created by him in the 1820s. As he showed, and in their turn Briot and Bouquet, functions with infinities have (Laurent) expansions. It had been shown by Liouville in the 1840s that a function which monodromic and monogenic everywhere in the plane must become infinite somewhere. Briot and Bouquet took this to mean what the elementary examples of rational and elliptic functions confirm, that therefore every non-constant synectic function will take every possible value, and they deduced that two monodromic and monogenic function with the same zeros and infinities are constants multiples of each other. This was enough abstract theory for them, and they now turned to recap their earlier study of differential equations, upon which they went on to base their theory of elliptic functions. Not only did this free the theory of elliptic functions from any dependence on elliptic integrals with their confusing two-valued integrands, it meant that Briot and Bourquet denied themselves the chance to show how real integrals can be evaluated by Cauchy's calculus of residues.

Durège's route up the Riemannberg in his [14] was more thorough because it was offered as the whole of a book and not a mere preliminary. He explained the arithmetic of complex numbers before saying that a function is a function of a complex variable if it is complex differentiable. Such a function satisfies the Cauchy-Riemann equations (and conversely, he said, as had Riemann) and is conformal when the derivative does not vanish. Many-valued functions are studied by considering the corresponding Riemann surface, not by cutting the complex plane. He used Green's theorem to show that the integral of a nowhere infinite function of a complex variable around a closed contour in the plane vanishes, and then he deduced the Cauchy integral theorem and the Cauchy residue theorem for function of a complex variable with simple infinities. As was standard since the work of Cauchy, and very clear in the lectures by Riemann, such functions are infinitely differentiable and have convergent power series expansions. Durège also followed Riemann in noting that, because the real and imaginary parts of a complex function are harmonic, they take their maxima and minima on the boundary of their domains. He then deduced Liouville's principle, that a function of a complex variable defined everywhere in the plane must become infinite somewhere, and followed Briot and Bouquet in believing that this implied that such a function will take every possible value (or it is a constant). Durège concluded his book with the very Riemannian topic of branch points and non-simply connected domains.

But the person to whom the task fell of getting complex function theory right was the third of the founding fathers, and a very different mathematician indeed : Weierstrass. Weierstrass defined an analytic function by a power series convergent in some disc and by the analytic continuation of this 'function element' as he called it. It is well known, and has recently been described in [52], that Weierstrass had a dislike of the integral, and after a few (unpublished) papers using it in the 1840s it disappeared from his repertoire. Weierstrass also distrusted the Cauchy-Riemann equations on the grounds that they were a pair of partial differential equations and as such examples of a poorly understood mathematical entity and therefore not well suited to the foundations of a subject.

If the work of Cauchy and Riemann lent itself to occasionally misty views the Weierstrass plateau required a steep ascent of its own and then a long march that exchanged the dubious pleasures of intuition for the sturdier virtues of rigour. Weierstrass based his lectures – he never wrote a book on complex function theory but required people to come to him and listen – on a theory of convergent power series, which define function elements, and analytic continuation, Single and many-valued functions are studied in this way. Gradually he isolated the central concepts : he was the first to distinguish between poles or finite singularities and essential singularities, and to show the falsehood of the claim that a non-constant analytic function takes every value. In its place he put the Casorati-Weierstrass theorem, which says that in the neighbourhood of an essential singularity an analytic function gets arbitrarily close to every value. Analytic continuation of an analytic function proceeds until it can go no further ; the points, curves, and regions where it cannot be done form what he called the natural boundary of the function (a concept Cauchy had missed and which Riemann did not attach a central role to).

Weierstrass continually reworked his theory and frequently revised his lectures on the subject, which formed the first quarter of his two-year four-semester lecture cycle in Berlin. In the 1870s he was happy to add his representation theorem, which describes the possible zero set of a complex function, and not long afterwards Mittag-Leffler contributed his complementary theorem on the possible singularities of a complex function. representation theorem.

The emphasis on power series and algebraic considerations, to the exclusion of much geometry and any interest in the integral and Cauchy's integral and residue theorems are simply explained [5]. Weierstrass's lifelong ambition was to do for Abelian functions what had already been done for elliptic functions. Abel and Jacobi had begun the process of giving a formal complex theory of the functions that arise by inverting elliptic integrals. Subsequent mathematicians took up the task of creating a rigorous theory of complex functions and placing elliptic functions securely in the new theory, a task accomplished most successfully by Weierstrass himself. However, not only was there not a satisfactory theory of Abelian functions, it has been argued by Jacobi that any such theory would have to be based on a theory of complex functions of several variables. Weierstrass agreed, and it became his life's mission to create a theory of complex functions of several variables and to show how the theory of Abelian functions grew out of it. This ambition determined his preference for the method of power series over that of either Cauchy or Riemann, because neither the Cauchy integral and residue theorems nor the Cauchy-Riemann equations generalise well to several variables. Weierstrass always sought to cast the single-variable theory in a way that would generalise to several variables, and for all his considerable success with a single variable he was unable to clarify the major features of the several variable theory or to make the progress he had hoped for. In this respect his work was a mixture of some insights, some unproved claims, and some actual errors.

Weierstrass also had an aversion to publishing, and so the first German textbooks on complex function theory were largely Riemannian. A year after Durège's book came Carl Neumann's [29]. Its first edition followed a careful presentation of the idea of a Riemann surface with an account of hyperelliptic functions (the second edition went on to consider Abelian functions in general). Casorati, the leading Italian in the field and the author of the first Italian book on the subject, also chose to give a deliberately Riemannian account in his *Teorica* [11], from the opening definitions to the proof of the Cauchy theorems and the deduction that analytic functions have power series expansions. He intended to write a second volume on elliptic and Abelian functions, but sadly he never did. The first Weierstrassian book was Thomae's second book on complex function theory, his *Elementare Theorie* [50]. It was much more elementary than his first, Riemannian text book, and it took a strictly Weierstrasian route because, said Thomae, there was no Weierstrassian textbook and it was unreasonable to expect the students to learn it on the own.

3 Later French textbooks in complex function theory

Cauchy's approach to complex analysis, however briefly it was treated by Briot and Bouquet, was naturally attractive to a French audience. It offered a natural starting point, a good range of standard techniques, and you did not have to go to Berlin to learn it. When French mathematics began to recover as a result of the reforms that followed the defeat of the Franco-

Prussian war, Hermite and Jules Tannery encouraged their doctoral students to write theses on various aspects of German (largely Riemannian) complex function theory. Bertrand also devoted part of his *Traité* on analysis to complex analysis [2], and while he inclined to present the theory of complex integrals as a way to evaluate real integrals he did not agree with Briot and Bouquet that Puiseux's treatment of integrals of many-valued functions was adequate and instead gave an account of Riemann surfaces, the first, therefore, in French. In his review of the book Darboux welcomed this part as being the first to show a proper understanding of this difficult subject and therefore to offer the hope that Riemann's ideas would not be abandoned. Briot and Bouquet for their part, reverted to Puiseux's account and added the Clebsch-Gordan theory of fundamental loops when they wrote their the second book, the *Théorie des fonctions elliptiques* [9] – which is very different from the first – and they never acknowledged a direct influence of Riemann.

This dichotomy marks the beginning of what one might call a policy decision among French authors. Given that Cauchy and Riemann largely agreed about where to start (with the definition of complex differentiability and the derivation of the Cauchy-Riemann equations) mathematicians faced with the sheer profusion of Cauchy's ideas and the greater focus and clarity of Riemann's had to decide if their treatment was intended to illuminate many-valued functions and, if it was, if the difficulty of understanding a Riemann surface has other merits that outweigh the ad hoc approach via cuts.

Hermite, for example, when he presented his *Cours* of 1880/81 at the Faculty of Sciences at the Sorbonne did not commit himself to a definition of a complex analytic function, and in the opening chapters showed a marked allegiance to the early work of Cauchy. But then he gathered speed and used Riemann's method (a Green's theorem argument) to prove the Cauchy integral theorem and so climb Mont Cauchy to the result that holomorphic functions are analytic. Then he continued along a Weierstrassian path : poles and essential singularities are distinguished and the Casorati-Weierstrass theorem proved ; the Weierstrass and Mittag-Leffler representation theorems are proved, and then he turned to elliptic functions.

In the years 1885 to 1891 the other Laurent (H., not Pierre after whom the Laurent series is named) published his seven-volume *Traité* [26]. As one might suppose in a work of this size an attempt was made to cover everything and, as one might also expect, not everything was described correctly. For example, the existence of the higher derivatives of a function that is once complex differentiable is assumed without proof. Nor did he see that there was anything to prove when he turned to (Pierre) Laurent's theorem. In fact, oddly enough, the book improved when its author turns to the Riemannian parts of the theory.

To draw this survey of textbooks on complex function theory to an end some general remarks can be made. There was something of a consensus in France that the elementary theory would be treated as Cauchy, or perhaps Briot and Bouquet, had presented, but that the higher reaches of the the theory would be presented in Weierstrassian terms, the bridge from the Cauchy-Riemann equations and the use of the integral to the use of power series methods (and infinite products) being provided by the Cauchy integral theorem. Then, with the foundations of the new theory in place the young researcher could be expected to move into the realms of elliptic function theory, or, less likely, into the deep waters of Abelian function theory. But if that route was to be taken nothing prepared anyone for what they might find except the difficult, and frankly sketchy original literature. In this respect Simart's doctoral thesis of 1882 is instructive. Simart described Riemann's theory of algebraic functions and (Riemann) surfaces as far as the Riemann-Roch theorem, and at the end he noted that he had just come across Klein's little book emphUeber Riemanns Theorie der algebraischen Funktionen und ihrer Integrale [23], which he hoped his 'preliminary study' would make it easy to read. Altogether a painful measure of the gap between French and German work on this important subject.

4 Poincaré

Poincaré was, of course, only recently out of his student days in 1880, and the origins of his work on Fuchsian and Kleinian automorphic functions lay in a prize competition of the Paris Academy of Sciences organised at that time by Hermite which also aimed to push French mathematicians in the direction of their German peers. But he was no ordinary student. His work on automorphic functions in 1881 and 1882 produced a class of functions that were considerable generalisations of the familiar elliptic functions.[4] He studied them by imposing non-Euclidean geometry on their maximal domain of definition, which was a disc, and his work marks the first use of non-Euclidean geometry outside the realm of pure geometry. This work occupied him for something like four years, and by the time he abandoned the field he had already taken up the study of lacunary functions – a topic to be defined in a moment. He had also begun to think about complex functions in two variables. He was to return to some of these themes some 20 years later and enrich them considerably. He also raised, even if he did not fully solve, the question of how many values a many-valued complex function could take, and investigated the theory of complex partial differential equations (a topic

[4] See [44] and the literature cited there.

outside the them of this paper).

In the course of his work on automorphic functions he was led to propose, and to sketch a proof of, the uniformisation theorem. As such, it is one of the first studies in which Riemann surfaces play an essential role. There is no question that Poincaré and Picard were the first to bring the Riemannian approach successfully to France, given the stated dislike that Hermite had for the subject (Darboux might have regarded Bertrand's book as giving Riemannian function theory some chance of life, but it took the next generation to bring it fully alive). But Poincaré's was not a wholly Riemannian understanding of the subject. The Fuchsian and Kleinian functions, as he noted, typically have the boundary of the non-Euclidean disc as their natural boundary, even if they can be made to define another analytic function outside the disc. This means that they cannot be continued analytically across the circle that bounds the disc. The concept of a natural boundary is very much a Weierstrassian one, present in the work of neither Cauchy nor Riemann.

The presence of a natural boundary in these examples raises the question of whether the two functions on the two domains (inside and outside the circle) should be considered as nonetheless one function, or as two. In 1883 [32] Poincaré 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.

Another aspect of Poincaré's work that is both Riemannian and Weierstrassian was his contribution to the Poincaré-Volterra theorem. This is the claim that a many-valued analytic function can take only countably many values at a point, or, alternatively, that a Riemann surface can only be a countable covering of the sphere. It seems that Weierstrass was aware of this result in 1885, to judge by letters he wrote to Schwarz and Kovalevskaya, but he did not publish anything about it. He had learned the result some years before from Cantor, or so Cantor claimed when in 1888 Vivanti published a flawed version of it. It was Vivanti's flawed but publicly available proof that inspired first Poincaré and then Volterra to give their own, rigorous versions. The reader is referred to (Ullrich 2000) for a full account.

Another important theme in Weierstrass's work was his proof that every analytic function can be written as an infinite product. Indeed, the product representation is in many ways more informative, because it locates the zeros of the function and says something about its rate of growth. Weierstrass's work of 1876 was followed by Laguerre [25]. 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.[5] To this end he drew attention to primary factors of the form $e^{x/a}(1-\frac{x}{a})$ which he called of order ("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 Emile Borel, who savoured Laguerre's habit of giving precise and interesting results without any systematic presentation of the underlying ideas (see [3]).

Poincaré, however, picked up the baton at once. In [**?**] he defined an entire function to be of genre n if its primary factors were of the form $e^{P(x)}\left(1-\frac{x}{a}\right)$ 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 $\exp\left(\alpha re^{i\theta}\right)$ tends to zero as r increases (θ being fixed), then $\exp\left(\alpha re^{i\theta}\right)F\left(re^{i\theta}\right)$ 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 $\exp\left(\alpha x^{n+1}\right)$ dominated F.

More troublingly, as he noted in a longer but inconclusive paper the next year [31] 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.

Indeed, he said, one could not be sure that the results were true. He was right to register a doubt : Boutroux [7] 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, as the remarks in his *Fonctions méromorphes* attest [4, p. 113]. Blocked in this direction Poincaré turned aside, publishing in April on another topic pioneered by Weierstrass, the theory of lacunary spaces [35] [36], and in May on the uniformisation theorem [31] [37].

As noted earlier, the only accepted formulation of the hope that Abelian integrals would produce a theory of Abelian functions was to seek to create

[5] Among the functions that behave very like a polynomial is Riemann's ξ function.

a theory of complex functions in several variables. This remained the case even though Poincaré's admittedly difficult theory of Fuchsian and Kleinian functions offered a single-variable alternative, and in any case a theory of complex functions of several variables would surely be an interesting thing to have.

In this context he and Picard first moved beyond Weierstrass in the study of functions of two variables [45]. This step from one to two is enough to raise the most salient difficulties. One mathematical aspect of the problem that Poincaré emphasised was the utility of the equations that replace the Cauchy-Riemann equations in the several variable setting. There are, in a sense, too many of them, so the system of equations need not have a solution, and certainly there will be harmonic functions that are not the real or imaginary part of a complex analytic functions – contrary to the case for functions of a single variable. But Poincaré was nonetheless able to show that in fact one can solve important problems in the function theory of several variables by working with harmonic functions first.

A function of several variables whose only singularities are poles – points at which it can meaningfully be said to tend to an infinite value – are called meromorphic. Weierstrass had claimed in 1880 [53] that a function that is meromorphic everywhere is also rational, a result that is true for functions of a single variable, but which he could not prove for functions of several variables. It was , however, soon proved by Hurwitz and Poincaré independently. It implies that if a function is meromorphic on a domain but not a rational function everywhere then its domain of definition is bounded. Hurwitz's argument [21] used induction on the number of variables. Poincaré, in the papers that mark his first involvement with the complex function theory of several variables (see [32], [33] and especially [34]), 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.

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) :

$$\frac{\partial^2 u}{\partial x_r^2} + \frac{\partial^2 u}{\partial y_r^2}\,, \quad r = 1, 2, \ldots, n \;\;, \tag{1}$$

$$\frac{\partial^2 u}{\partial x_r \partial x_s} + \frac{\partial^2 u}{\partial y_r \partial y_s} = 0 \;\;, \tag{2}$$

$$\frac{\partial^2 u}{\partial x_r \partial y_s} = \frac{\partial^2 u}{\partial y_r \partial x_s} \;\;. \tag{3}$$

These equations seem to have been written down for the first time by Poincaré in 1883. Weierstrass did not do so, because 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 with harmonic functions of several variables, for there are more differential equations than variables. In particular, there are harmonic functions of four real variables that are not the real part of a complex function of two complex variables. This is just one mathematical reason why generalising from one variable was to prove difficult.

Poincaré 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. For each ball he found a harmonic function analytic outside the ball and tending to zero at infinity. These functions enabled him to define a harmonic function Φ such that if at an arbitrary point$F = \frac{N}{D}$, then $\Phi - \log|D|$ was analytic. For this, he said, it was enough to apply Weierstrass's proof of Mittag-Leffler's theorem. Although the function Φ was not an analytic function, he claimed that one could always find an entire harmonic function G satisfying equations (1)-(3) and such that the difference $\Phi - G$ was the real part of an analytic function Ψ of two variables. It followed that the functions G_1 and G_2 defined by the equations

$$e^{\Psi} = G_1 \text{ and } Fe^{\Psi} = G_2$$

were entire and that the function F was their quotient everywhere, $F = \frac{G_2}{G_1}$.

It seems that this work did not satisfy Weierstrass, who, without mentioning Poincaré by name, noted in 1886 in his [54, p. 137], that the question was unresolved and some considerable difficulties seemed to lie in the path of a solution.

The next significant result came in 1890, when Appell gave a proof in [1] 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 [13] Cousin generalised Poincaré's result to any dimension and so Appell's proof became valid for all g.[6]

Then in 1897 Poincaré announced detailed proofs of two of the essential steps in Weierstrass's programme for Abelian functions. The second of these said that every Abelian function can be written as a quotient of theta functions, and Poincaré remarked that Weierstrass had never published a proof of this result. Details of the proof of this second claim appeared in 1898. To prove it, Poincaré went back to his work of 1883 [34], in which he showed that a meromorphic function is a quotient, and modified it to establish that the entire functions forming the quotient can be taken to be theta functions.

[6] Cousin's paper and Poincaré's work on this topic are discussed in Chorlay (to appear).

In a paper of 1902, written in answer to a request from Mittag-Leffler, Poincaré proved, amongst other things, that every $2n$-fold periodic function whose periods satisfy the Riemann conditions can be expressed by means of theta functions. He gave a geometrical demonstration that a modern commentator [22, p. 163] observes is interesting but not quite satisfactory. This theorem 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 III 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 then given other proofs. This new one by Poincaré occupied a middle position between his first proof and the methods of Cousin.

5 Poincaré and conformal maps in two variables

In his paper of 1907 [41], Poincaré raised the question of the conformal 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 conformal 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.[7]

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 a conformal 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 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

[7] The first explicit examples of domains with inequivalent boundaries were given in [46], where he introduced what are often called Reinhardt domains. As he 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.

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 is 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 [27], and Campbell's [10] 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 § 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 § 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.

6 Conclusions

Poincaré contributed in important ways to three topics in the complex function theory of his day : Fuchsian and Kleinian functions ; genre and lacunary series ; and functions of several variables including Abelian functions. Of these, he is well remembered today only for the first. But after him very little was done in that area of lasting significance – one might say of comparable significance – for a long time. In the field of genre and lacunary series his work was dissolved into the much more elaborate theories of Hadamard and, especially, of Emile Borel. As for functions of several variables and Abelian functions, he is well remembered for his discovery of different domains of holomorphy, but the rest of his contributions are hard to evaluate because we lack a good history of complex function theory in several variables, and the modern theory starts with the work of Oka and Henri Cartan in the 1930s (although Renaud Chorlay has begun to work in this area, see his paper to appear in *Archive for History of Exact Sciences*).

How then should we regard Poincaré's contributions today ? From the standpoint of elementary complex function theory we can note the following. Throughout the period and indeed ever since, the elementary theory is exclusively concerned with a single variable. Textbook writers increasingly started by climbing Mont Cauchy to the point where they proved that a holomorphic function is analytic, then they slid over to the Weierstrass plateau for a selection of the topics that are best done that way : the distinction between poles and essential singularities, the Weierstrass and Mittag-Leffler representation theorems. Even the Weierstrass theory of elliptic functions, which is generally regarded as the best one to start with, is omitted from most accounts of complex function theory today, and Weierstrass's aspiration to bring about a complex function theory in several variables is entirely forgotten. This consensus emerged in the 1890s, it is visible early on in Hilbert's lectures at Göttingen in 1896/97, and it was forged in Germany. The French textbooks made more of the transition from Cauchy to Riemann, for those who wanted to venture so far, reflecting an acceptance of many-valued functions that we also do not share. It will be evident that Poincaré made no attempt to contribute to forging this consensus, and of course this reflects the fact that professionally his teaching was in branches of physics. But it is also the case that the whole theory of how properties of functions are encoded in their Taylor series never became part of the consensus either. It remained firmly part of the research enterprise, one with a strongly French flavour.

That raises more puzzles than it might seem. The Weierstrass approach emphasised power series and natural boundaries, but there was little done to connect the two until Borel, Fabry, and others took up the subject. It would be interesting to understand better why what might seem to be an obvious topic of interest for any pupil of Weierstrass's was not taken up by any of them but prospered instead in France. The way that work on power series and convergence never became part of the elementary theory is also worth reflecting upon. One might say that such work is simply not elementary, and that is true – the approach via the Cauchy integral theorem is much more direct, in the opinion of all but Weierstrass and his most loyal disciples. But there is also a component of what makes a teaching package, a package that others can accept and teach on their own account ; what makes a textbook, in other words, and at that level what swayed most authors was the fact the the theory of the integral was known to all students from their study of real analysis before they ever encountered complex function theory.

In the last years of Weierstrass's life it became clear that the next generation in Berlin (Fuchs, Schwarz, and Frobenius) would not be carrying on his work, and at the same time in Paris Emile Borel drew around him people who shared his interests in real and complex function theory. Whether or not they formed a school in Parshall's sense of the term [30] they formed the first research group in mathematics in France. Borel's famous series of monographs testify to the shared interests of this group, and famously its emergence marks a generational shift in French mathematics. The influence Poincaré had on this group was slight, another instance of the fascinating question of the impact Poincaré had on his contemporaries, and how slight it seems to have been. The difference is that in this case, instead of doing too much and leaving nothing easy for any followers to get started on, in this case he did not do enough and was soon pushed out of the way.

Much of Poincaré's work on complex function theory in one or several variables lay well beyond any teaching frontier. With the exception of his work on lacunary series, and perhaps the Poincaré-Volterra theorem, nothing he did picked up on issues that might engage the beginning graduate student of his time. This may well reflect his official position as a physicist - he was not lecturing on complex function theory, and need not attend to its development. But his work on the complex function theory of several variables was influential, it drew contributions from Appell and Cousin, and it did so by shifting the formulation of problems away from those laid down by Weierstrass.

The work on Fuchsian and Kleinian functions was driven by a vision, a desire to create a new class of functions that generalised and extended a range of known examples. This was not the case with Poincaré's work on function theory in several variables, which respected the structure of the theory as Weierstrass had expected it to be but brought to it a (Riemannian, one might say) wish to exploit what remained of the connection to harmonic function theory. This was difficult enough, but one can always ask of a piece of research why and when it ended. In 1884 Poincaré abandoned Fuchsian and Kleinian functions because he had established the fundamentals of the theory, and identified and even imperfectly proved the most important theorem to which it led. If he also thought that further progress with Kleinian functions would be hard to come by, and he did not say so, he was proved to be right : the theory rested more or less where he left it until the 1960s.

In looking at several complex variables Poincaré was much more conservative. His most unexpected result was his proof that there are domains which are topologically but not conformally equivalent. Overall, his discoveries are piecemeal, spread out over a number of years, the product undoubtedly of considerable thought and insight, but not a programme. It would be unfair to call them opportunistic, because they do not seem to have capitalised on the recent work of other mathematicians. Rather, they reflect a mature mathematician, aware of a number of important issues, who carries around with him a number of ideas that he explores, some of which turn out to be fruitful. In the topic at hand, it is most likely that Poincaré's deepening interest in the 1890s in harmonic function theory and the partial differential equations of mathematical physics that brought him back to the subject, until he was able to find a way to resolve the major questions.

The fact is that the subject was difficult, the advances Poincaré and others made were restricted, the best ideas some 30 or 40 years in the future. Piecemeal progress was all anyone could achieve, the hoped-for analogy with the complex function theory of a single variable was insubstantial. Poincaré's work reminds us what historians of mathematics and historians of science too easily forget : there is no guarantee of success in the best research, and if a breakthrough is made, there is no guarantee that another will follow. Researchers know this very well, and the best, like Poincaré, deal with it by cultivating several topics at once until good fortune strikes. His work belongs to an honourable short list of achievements in a very difficult branch of mathematics that, impressive as they are, did not produce a systematic theory. The reasons for this are measured by the steps Oka and Cartan needed to take to advance the theory beyond the place where Weierstrass, Poincaré and Cousin left it.

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