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Reference #:

Journal Title: Studies in the history of modern mathematics.

Volume: **Issue:** 44
Month/Year: 1996 **Pages:** 85-111

Article Author:

Article Title: Bottazzini, U. and Gray, J.; Complex function theory from Zurich (1897) to Zurich (1932)

Imprint: Palermo ; Circolo matematico di Palermo,

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RENDICONTI DEL CIRCOLO MATEMATICO DI PALERMO
Serie II, Supplemento N. 44 (1996), pp. 85-111

COMPLEX FUNCTION THEORY FROM ZURICH (1897) TO ZURICH (1932)[1]

UMBERTO BOTTAZZINI - JEREMY J. GRAY

1. Introduction.

The theme of our paper is developments on complex function theory from Zurich 1897 to Zurich 1932. It is not merely the coincidence of venue that invites such a topic, nor indeed the suggestions of the organising committee. In fact, the interest that attaches to such a topic has already drawn two historical papers on this theme over the years: Hurwitz's address at the first ICM was largely historical, and so was Julia's in 1932, which he expanded into an *Essai* the following year. With Julia, we can only agree that the subject is vast and subscribe to his epigram that: "Who does not know how to restrict himself will never write anything" ("Qui ne sait se borner ne sut jamais écrire").

The core of the talk is function theory as it has been presented at the International Congresses. This was also the thread that Julia followed. But we shall also look at the wider field of function theory, and see what was, or was not, represented at the Congresses. We have restricted ourselves to function theory of a single variable, and to what may be called the elementary theory, avoiding connections with other branches of mathematics, such as number theory and differential

[1] This paper is a revised version of the ICHM lecture we gave on August 6, 1994 at the International Congress of Mathematicians (ICM) in Zurich.

equations, even in the complex domain. This concentrates our attention on complex function theory in the strict sense. One of our interests is how such a subject grew up after the truly pioneering work of Cauchy, Riemann, and Weierstrass was done, and these founding fathers were no more.

From Zurich in 1897 to Zurich in 1932 and beyond, to Oslo in 1936, there were slow but steady changes in every aspect of complex analysis, with some marked bursts of activity. For example, consider the question: 'what is a complex function?' In the period from 1897 to 1932, the concept of a complex function passed from being fluid to being fixed. So too did the idea of analytic continuation. As to the question of what constituted the best proofs (the most appropriate, say, or the most instructive) the key topic here is undoubtedly Picard's theorems. The choice of the central or governing ideas was likewise controversial. What, if any, should be the role of geometry in classifying what is going on? What is the connection between a function and the coefficients of a power series defining it? What of the dichotomy between meromorphic and entire functions?

Zurich 1897.

On Monday 9 August, 1897 Adolf Hurwitz rose to address an audience of perhaps 200 mathematicians on the subject of complex function theory (Hurwitz 1898). He began by saying that the subject was doubly interesting. Once because of the interest that attached to the study of particular classes of functions: algebraic functions and their integrals, the new transcendental functions of Klein and Poincaré. And again because of its own intrinsic interest as the foundational subject in analysis. It was this aspect, he went on, that he proposed to address.

On the Weierstrassian approach, one considers function elements: each is a power series defined on some open disc with a certain radius of convergence. A family of these that behave suitably on the overlaps, defines a function. This type of definition recalls the approach that was common in the 19th century and before. A function is something for which one can write down some sort of rule for its values: a polynomial, something in trigonometric functions, a convergent power series. The Weierstrassian approach starts here. There is not some antecedent definition of a function, or of a complex function, after which comes a representation theorem asserting that every complex function can be exhibited as a family of functions.

Given a single-valued analytic function it was interesting to enquire about the set of points that lay inside some disc, and those that did not. For a given function the points interior to some disc formed the function's domain of continuity, while the points that did not formed its natural boundary. What could be said about a domain of continuity (in today's terminology, a domain of holomorphy)? To answer this question led straight to topology. Hurwitz defined a continuum K as a set of points such that for every point $z \in K$ there is a disc containing z and contained in K, and given any two points z and z' of K there is a chain of points $\{z_1, \dots, z_n\}$ in K such that $z = z_1$, $z' = z_n$ and the distance between any two consecutive z's can be made arbitrarily small. (This is a weaker condition than connected, it is satisfied by the plane of complex numbers with the real axis removed.)

Hurwitz then said:

It is a theorem that, given any continuum, there is always a single-valued analytic function whose domain of continuity is precisely the given continuum.

For the proof, he referred to the work of Mittag-Leffler, later simplified by Runge and Stäckel.

Hurwitz then turned to the study of the set of singular or boundary points and explained that, using Cantor's theory of transfinite sets, a complete topological characterisation of the set of singular points of an analytic function could be given.

Hurwitz then observed that there were analytic functions having natural boundaries, and that many interesting special cases had been published. This raised the question of the behaviour of a function in a neighbourhood of an isolated singular point. If the function did not simply tend to infinity, then the singular point was said to be essential. Hurwitz said:

> By a classical theorem of Picard, only two cases are possible: either the function takes every finite value on every neighbourhood

> of the singular point, or it takes every value except one. Picard based his proof of this theorem on the properties of the modular function. Special cases of this theorem have later been proved by Hadamard and Borel by elementary means.

It seemed, he said, that no analogous theorem was known about non-isolated singular points.

As for analytic functions that were not single-valued, Hurwitz suggested the best way forward lay with the idea of Riemann surfaces, and with Poincaré's ideas about the uniformisation of analytic functions.

Hurwitz then turned to the Cauchy-Riemann approach, which, he said, had only recently been given anything like the necessary degree of rigour. Here the key idea was that of a synectic function (for which read holomorphic), that is, a function that has a differential quotient everywhere it is defined which is independent of direction. The crucial theorem now was the Cauchy integral theorem: 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.

Here 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?

These questions were all the more urgent in the aftermath of the space-filling curves of Peano and Hilbert. Partial answers had been given by Jordan, in his *Cours d'analyse*, and later by Schoenfliess. The situation, Hurwitz said, was that Jordan had proved the Cauchy integral theorem for rectifiable curves.

It was well-known, he said, that another central theorem was the Riemann mapping theorem, which established the existence and (under suitable restrictions) the uniqueness of a conformal map of one simply connected domain onto another. This had now been proved, for a wide class of surfaces, by Neumann and Schwarz, Harnack, Poincaré, and others.

Hurwitz's choice as a speaker was surely in part a matter of politics. As it was, the Berlin school was conspicuous only by its absence, for, as Minkowski had predicted, they took exception to the

choice of Klein as head of the German delegation. But an eminent function theorist, working in Zürich, an undergraduate student of Weierstrass's even if a doctoral student of Klein's, could surely be presumed to be even-handed in the treatment not just of Prussian matters but in adjudicating between French and German styles. So, in the event, he was. As the leading spokesman for the Weierstrassian approach, Mittag-Leffler would have had good cause for feeling that it had been presented as the rigorous style of complex analysis. The contrast with the slippery nature of the Cauchy-Riemann approach had been made evident. On the other hand, the new generation of French mathematicians might have felt their work was a little slighted, in the text itself if not so obviously in the footnotes that accompany the written text.

Contemporaries would not have disputed the central role Hurwitz ascribed to complex analysis. Nor would they have grumbled at the stark separation Hurwitz went on to make between complex function theory in the style of Weierstrass on the one hand, and that of Cauchy and Riemann on the other. The two theories differed in the approach to what constituted a function of a complex variable. For example, the Cauchy integral theorem is entirely missing from the Weierstrassian theory. Any unification of the two approaches would have to face a thorough-going disagreement as to what sorts of operations were fundamental. Hurwitz also underplayed the importance of results about the representation of functions by series of rational functions. Examples that were well-known by 1897 showed that it was possible for one analytic expression to represent two analytic functions in two domains, spelling an end to Riemann's express belief that his class of analytic functions was the same as the class definable by infinitely many algebraic operations.

Note next the growing importance of ideas drawn from Cantorian set theory and point set topology. A good theory existed to characterise domains of holomorphy and sets of singular points. Hurwitz did not commit himself to any general discussion of the Riemann mapping theorem, but left it open. Finally, we observe that Hurwitz singled out the big Picard theorem for particular mention. This allusion to the work of Hadamard and Borel was to prove but the start of a long-running story.

The subject of entire functions is interesting historically for two reasons. It represents the first successful new branch of complex function theory, the first intrinsic development of the subject one might say for its own sake. And it represents the way the Weierstrassian approach might have developed had not the new Berlin generation turned aside to other subjects. The spur was the big and little theorems of Picard, which describe the distribution of values of a function having an isolated essential singularity. What was at stake was a so-called elementary proof, by which is meant, as so often, a harder proof but one which makes less call on advanced parts of the theory. (In this case, what was objected to was Picard's use of the modular function from elliptic function theory). To this they connected the natural question: what can be said about a function and in particular its singular points given its Taylor series.

The background here is interesting. Many of the ideas that were to be developed to such spectacular effect by Hadamard, Borel, Fatou, Julia and others can be traced back to a series of short papers published by Poincaré in the 1880s, apparently without much effect at the time.

In 1882, Poincaré had responded to two short papers of Laguerre. He defined an entire function to be of genre n if its primary factors were of the form $e^{P(x)}\left(1-\frac{x}{\alpha}\right)$ where $P(x)$ was a polynomial of degree n (Poincaré 1882). He then showed that if F is of order zero, 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.

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:

the sum of two functions of genre n is also of genre n;

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: in 1902 Boutroux showed that pairs

of certain types of functions of genre n had a sum of genre $n+1$.

Blocked in this direction Poincaré turned aside, publishing in April 1883 two papers on lacunary spaces, which are interesting because they were to be the spur to later work of Borel. Weierstrass had given examples of one analytic expression convergent inside, and outside, but never on the unit circle; for example,

$$\sum_{n=0}^{\infty}\frac{1}{z^n+z^{-n}}.$$

Others, including Hermite and J. Tannery had given further examples. In his paper, Poincaré constructed functions of the form

$$\sum_{n=0}^{\infty}\frac{a_n}{z-b_n}$$

which were analytic everywhere in the exterior of a convex region S of the plane. This hole was called the lacunary space for the function. Poincaré's contribution was to show that the obvious notion of a natural (even if not analytic) continuation of such functions made no sense. He gave examples of his thetafuchsian functions to show that in fact the natural continuation was not uniquely defined.

Then in May 1883 Poincaré published his first attempt at establishing the uniformisation theorem. This had arisen as a conjecture during his work on Fuchsian and Kleinian functions, and Hilbert was to cite Poincaré's uniformisation theorem in his list of 23 problems at the ICM in Paris in 1900.

In 1890 Poincaré published an important paper on iteration theory, not in the context of flows on surfaces and in dynamics, but during a study of complex functions that admit multiplication theorems. A function f admits a multiplication theorem if for some fixed m, $|m|>1$:

$$f(mu)=R(f(u)),$$

with inverse $f(u)=S(f(mu))$, where and $f(0)=0$, and R, S are rational functions. Poincaré studied the transformation

$$C: f(u)\to f(mu).$$

The power series for f will depend on a single parameter b. Setting trivial cases aside, Poincaré asked if, by varying this parameter, $f(u)$ can be arbitrary, or is $f(u)$ confined to some domain. By looking at the backwards iterates $C^{-p}(a)$, he found that:

If $\forall a$, $C^{-p}(a) \to 0$, then yes, $f(u)$ can be arbitrary.

If only some a are such that $C^{-p}(a) \to 0$, say those in a domain D, then $f(u)$ is in D.

He then raised but could not answer the question:

When does $C^{p}(D) \to$ fixed point? He found this partial answer: it is necessary but not sufficient that $\dfrac{dCa}{da} < 1$.

Poincaré then showed that the inverse function g is a single-valued function of z, developable as a power series everywhere it is defined, having no essential singularities inside a domain on which is defined, although it will have essential singularities on the boundary.

Borel, in his thesis of 1894 (examined and approved by Appell and Poincaré) proposed a different definition of analytic continuation. On this definition it was possible to say that at least for certain identifiable cases of this type analytic continuation was possible from one domain to another. The shift in definition was to allow analytic continuation along arcs, rather than strips. He gave examples of functions f and g defined respectively inside and outside the unit circle and which could not be analytically continued across the unit circle, but for which one could cross the circle on 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.

Paris 1900.

Mittag-Leffler gave two talks at the ICM in Paris in 1900. One

was on the life and work of Weierstrass, the other on a topic in function theory. In it, he raised and solved a problem to do with an analytic expression which was convergent inside a star-shaped domain. The expression, however, would converge outside the star, so he asked for a new analytic expression that converged precisely inside the star. This question he then answered. Even the pages of the *Comptes Rendus* bristle with what happened next. Borel, they record, asked for the opportunity to reply. He went on to observe (if we may paraphrase the account) 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 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. Their public disagreement was to rumble through the next several Congresses. Unfortunately, we do not know enough of the background to account for this extraordinary confrontation, but it is hard to understand the French selection of speakers and why Borel was not asked to speak at Paris. As Hurwitz 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.

Borel's starting point in 1894 had been the paper by Poincaré on the concept of genre. Borel wrote an entire function of genre p in the form

$$e^{Q(z)} \prod \left(1 - \frac{z}{a_n}\right) e^{\left(\frac{z}{a_n} + \frac{z^2}{2a_{2,n}} + \ldots + \frac{z^p}{pa_{p,n}}\right)}$$

where the series $\sum \dfrac{1}{|a_n|^{p+1}}$ is convergent. He observed that Poincaré had proved that if $f(z)$ is a function of genre p, then on the circle $|z|=r$, for every positive number α,

$$\lim_{r\to\infty} f(z)e^{-\alpha r^{p+1}}=0.$$

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

$$M(r)<e^{\alpha r^{p+1}}.$$

He then defined the order of the function as

$$\rho=\limsup\frac{\log\log(M(r))}{\log r};$$

it evidently lies between p and $p+1$. This was the crucial step, for order is a much better behaved concept than genre. For example, the order of an entire function and its derivative are the same, and the order of the sum of two entire functions of order n is again n. Using Hadamard's results about the maximum modulus function, the location of the zeros of entire functions and the properties of the concept of order, Borel was now able to give a proof of Picard's little theorem.

Heidelberg, Rome and Cambridge.

In 1904, when the ICM was in Heidelberg, interest was focussed on hitherto unpublished work of Riemann, then being edited by Schlesinger and Wirtinger. But this time the new (one might say, not so new) French theory of entire functions got an airing. Boutroux, a cousin of Henri Poincaré, reported on entire functions of finite integral order, and Mittag-Leffler on a particularly simple class of entire functions. These are his E-functions, defined by the equations $E_\alpha(z)=\sum_{n=0}^{\infty}\frac{z^n}{\Gamma(1+n\alpha)}$, so for example $E_1(z)=e^z$. They have particularly simple growth properties, as we shall see.

In 1908 in Rome, Mittag-Leffler spoke (for the third consecutive time) on the theory of analytic expressions and complex functions. His was not the only paper; Boutroux and Schlesinger also spoke. Koebe talked, with uncharacteristic brevity, on his recent successful and very thorough proof of the uniformisation theorem. Koebe's proof includes the Riemann mapping theorem as a special case. A paper he had published in 1907 concludes with a very interesting comment. After noting the work of Runge on the representation of a function by a series of rational functions, Koebe wrote (we paraphrase slightly):

> The fundamental problem with Weierstrass's theory of analytische Gebilde 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. [1907b, 210].

But Mittag-Leffler's paper was the one 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 advocated their use as domains of convergence of series in explicit opposition to Borel's theory of analytic continuation. This led him to outline an interesting philosophy of what constitutes the best proofs in analysis, and thus to a defence of Weierstrass. We paraphrase slightly:

> 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.

On the other hand, 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. To this question he offered some answers, in terms of the functions E_α that he had discussed at an earlier Congress: The function E_α is bounded in the sector $\frac{\alpha\pi}{2} < \arg z < 2\pi - \frac{\alpha\pi}{2}$ but no larger sector. And for such work, he relied on the Cauchy integral theorem - what he called 'the incomparable instrument of higher analysis'.

Not surprisingly, Borel took the opportunity finally presented by his invitation to address the Cambridge Congress of 1912 to present his ideas on analytic continuation. This took the form of a restatement of his position in opposition to that of Mittag-Leffler. However, at this point the Congresses provide a poor picture of the state of the complex function theoretic art. The Cambridge organisers, swayed presumably by the much stronger Cambridge tradition in applied than in pure mathematics, invited none of the emerging younger British generation to speak on complex analysis. The next Congress, which was to have been organised in Stockholm by Mittag-Leffler, would presumably have done better by the strong Scandinavian and Finnish school of analysts, but that Congress was of course abandoned because of the First World War. A 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). It was surely likely, too, that Hermann Weyl might have talked on his recently published work on the concept of a Riemann surface, in which topological ideas were used to give the first rigorous treatment of Riemann's ideas on algebraic functions and their integrals.

Strasbourg 1920.

The Congress after Cambridge was therefore the one in Strasbourg in 1920. The political atmosphere was highly charged. With the return of Alsace and Lorraine to France, Strasbourg was once again French, and the venue was certainly chosen with this in mind. There was, of course, a new French Faculty in the university. But this was a mere speck in the French-led hostility to all things German, in which Picard was prominent, and the result was that German mathematicians responded with a petition calling for a boycott of the Congress. While some, like Hilbert, refused to sign it, and others, among them Bieberbach, helped to organize it, it underlined the extent to which

German function theorists were simply unwelcome. For the first time in his career, Mittag-Leffler did not attend an ICM, in protest at the treatment of the Germans whose side he had supported in the War.

Had politics not intervened, Bieberbach might well have talked on his recent results generalising Picard's theorems to angular sectors, in which Mittag-Leffler's E_α functions played an extremal role. However, this theory was subsumed under Julia's theory of rays, outlined at the Congress by Valiron. This theory established the existence of arbitrarily narrow angular sectors on either side of a crucial ray (later called the Julia direction) within which the function necessarily took all but at most one value. The corresponding theory for meromorphic functions was held up by Ostrowski's discovery that Julia directions need not exist for meromorphic functions; progress had to wait for Valiron to introduce ideas drawn from Nevanlinna theory.

Otherwise, at the Congress itself, complex function theory was well represented in the Section on analysis by Rémoundos, Boutroux, and in the absence of Germans such as Courant and German supporters such as Carathéodory, by the Spanish mathematician Rey Pastor, who spoke on his extension of Carathéodory's explicit method in the theory of conformal representations.

Alas, complex function theory was barely discussed at the next ICM, in Toronto in 1924, even though the Canadian mathematician Fields had worked in the area. The consequences of this omission were rather serious. Missing from the Congress, for example, is any report on the work of Montel and his profound idea of normal families. The idea of normal families went back before Montel to work by Arzelà, Ascoli, Osgood and others on the convergence of a sequence of analytic functions to an analytic function.

As Montel observed at the start of his thesis, if convergence of even only a subsequence could be found, then, for example, one could look for a proof of at least special cases of Dirichlet's principle. With this motivation Montel said a family $\mathscr{F}$ of functions analytic on D is normal if every sequence of functions in $\mathscr{F}$ has a subsequence converging uniformly on all compact subsets of D to a function that is either analytic or identically infinite. Similar definitions apply for families of meromorphic functions.

In this thesis he proved the theorem that now bears his name:

Montel's theorem (A sequence of analytic functions on a disc D which is uniformly bounded has a subsequence converging uniformly in D to an analytic function). In his paper [1912] Montel showed that if a family has at least two exceptional values, then it is normal. He proved this theorem in two ways; once using Picard's theorem, and again using Schottky's theorem (incidentally vindicating a passing remark in his thesis). In 1916 Montel reversed the order of ideas, using his theorem on normal families to prove Schottky's theorem and hence the Picard theorems. Here is a brief summary, following Carathéodory, of this proof of the big Picard theorem. He assumed that $f(z)$ was a non-trivial analytic function on the punctured disc $D^\circ := D - \{0\}$ and omitting the three values $\{0, 1, \infty\}$. He defined the sequence of functions

$$\left\{ f_n(z) := f\left(\frac{x}{2^n}\right) \right\}.$$

In the annulus $\left\{ z : \frac{1}{2} < |z| < 1 \right\}$ each function is analytic and omits the values 0 and 1. The family is therefore normal. Pick a convergent subsequence and look at the limit function. If it is analytic then the original function f can be shown to be bounded on the punctured disc D° and so it extends (by the removable singularities theorem) to a function analytic at $z = 0$. If the limit function is identically ∞, then the same argument but applied to the reciprocal function $\frac{1}{f(z)}$ again yields a function analytic on all of the open disc D.

Montel then applied this result to a disc surrounding an essential singularity of a function $f(z)$ that omitted three values in the disc and deduced that the function could be extended analytically across its essential singularity. From this contradiction he deduced Picard's big theorem.

Also missing from the Congresses, and only briefly alluded to in Julia's address, is one topic that perhaps is more lively today than any other we are discussing: the Fatou-Julia theory of the iteration of rational functions. This has recently been well-described by Daniel S. Alexander in his recent book *A history of complex dynamics* so we may be brief. (See also Alexander's paper in the present volume).

The roots of the subject, and the focus of attention of almost everyone who participated in the early work, were in functional equations. People like Koenigs and Schröder, Léau and others, established, with varying degrees of rigour, a local theory of iterative processes with a view to solving certain functional equations. It is often suggested that Poincaré's work on the first return map in his theory of dynamical systems and celestial mechanics was influential in stimulating interest in iteration theory, but Alexander finds it is doubtful that Poincaré's work was the primary motivating factor (p. 110). Instead he emphasizes the then-central, now rather forgotten role of functional equations, and Montel's recent introduction of normal families, that was to play such an important role in the work of Fatou and Julia.

Indeed, Julia never referred to Poincaré's work on iteration, and Fatou only mentioned Poincaré's paper on complex functions that admit multiplication theorems. Lattès also took this paper as his starting point in his note of 1918, from which Julia, so to speak, took the Lattès example of a function whose Julia set is the entire plane.

The theory of normal families was to prove central to the work of Fatou and Julia. If, for example, points $z_0 \in D$ converge under iteration of a function $f(z)$ to a fixed point, then the family $f^n(z)$ is normal in D. But if z_0 belongs to the Julia set of $f(z)$, then the family $f^n(z)$ is normal in neighbourhood of z_0. Both Fatou and Julia divided the extended complex plane up into regions of normality and non-normality. Both then studied the set of non-normality, J (to give it its current label). They showed that it is the closure of its periodic repelling points.

Both then looked with interest (one might conjecture: amazement) at the sorts of Julia sets that can arise. They found examples of totally disconnected perfect sets, continuous curves lacking tangents at an infinite set of points; closed curves with infinitely many double points, closed Jordan curves, a line segment (Julia only). They noted that some of these conditions persist under perturbations of f. And they noted that the sets J (as Fatou put it) 'Have the same structure for all its parts' or in Julia's words 'from any small portion of J, one can generate J in its entirety'.

Bologna 1928.

So we must pass to the ICM in Bologna in 1928, where handsome amends were made. A large number of papers on complex function theory were presented in the Section on analysis. Among them, Koebe was brought back to report on conformal representation and uniformisation. Polya gave an instructive range of examples of what could happen on the boundary, drawing on the work of Ostrowski, Faber, Mandelbrojt and himself. The organizing principle was his ideas about the density of the coefficients. Szolem Mandelbrojt gave a short, but technical, report on singularities of Dirichlet series. Valiron's report, on singular points and Taylor series, drew on the work of Nevanlinna.

Undoubtedly, the most important paper from our point of view was Rolf Nevanlinna's account of what he called uniqueness theorems for single-valued functions, made possible by his work on value-distribution theory. Papers by Lehto, Ahlfors, and Hayman, have described in fascinating details the route Nevanlinna took to his discoveries, and the immediate acclaim with which they were met. We note Hermann Weyl's comment, that Nevanlinna's work (with that of his brother Frithiof) was "One of the few great mathematical events of our century". Hilbert was similarly fulsome. We cannot enter into the technicalities here. As Einar Hille (1962) wrote of the second fundamental theorem:

> [R. Nevanlinna's] proof is fairly elementary but certainly not simple. A simple but far from elementary proof was given ... by Frithiof. ... A third proof is due to Ahlfors. ... This method is both simple and elementary.

To pick up the story at its peak, in his paper in Acta Mathematica in 1925, later expanded into his book of 1929, Nevanlinna defined the central problem in the theory of entire functions as being to relate the growth of $f(x)$ to the density of the roots of $f(x)=z$. He said that the central result was Borel's generalisation of Picard's theorem to show that

$$n(r;z):=\#\{x: f(x)=z, |x|<r\}$$

was of exactly the same order as $\log(M(r))$ for all values of z except at most one.

However, the theory did not generalize to meromorphic functions, because the first important property of $\log(M(r))$ lapsed: it was no longer increasing. Indeed, it is infinite at every pole of the function. Borel's attempted generalization of 1903 was open to the objection that $n(r,\infty)$ had to be finite and, more seriously, it depended on the form in which the function was expressed and was not intrinsic.

Nevanlinna had the idea of defining the function

$$\log^+ x = \max(\log x, 0), \text{ for } x \geq 0,$$

so

$$\log x = \log^+ x - \log^+ \frac{1}{x}, \; x \geq 0,$$

and introducing $\log^+$ into the Valiron-Jensen formula.

He then defined the functions

$$N(r,z) := \int_0^r \frac{n(t;z)}{t} dt,$$

$$m(r,a) := \frac{1}{2\pi} \int_0^{2\pi} \log^+ \left| \frac{1}{f(re^{i\theta}) - a} \right| d\theta,$$

and

$$m(r,\infty) = \frac{1}{2\pi} \int_0^{2\pi} \log^+ |f(re^{i\theta})| d\theta.$$

Nevanlinna described the function $m(r,a)$ as measuring the strength of the mean convergence of f to a as r tended to ∞. He could then show that

$$m(r,\infty) + N(r,\infty) = m(r,0) + N(r,0) + \log|f(0)|.$$

Ahlfors commented that "This is the moment that Nevanlinna theory was born". Nevanlinna then defined

$$T(r,f) = m(r,\infty) + N(r,\infty)$$

and deduced (from the first Fundamental Theorem of Nevanlinna theory) that for all a such that $f(0) \neq a$:

$$m(r,a) + N(r,a) = T(r,f) + O(1).$$

As Hayman observed, the function $T(r, f)$ gives an excellent description of the growth of the function f in a disc or in the plane. It is, as H. Cartan showed, a convex increasing function of $\log(r)$. If f is entire, then T has roughly the growth rate of $\log(M(r))$, while N measures the number of roots of $f(z)=a$ in $|z|\leq r$ and m the average closeness of $f(z)$ to a on $|z|=r$. Thus Hadamard's inequality immediately follows:

$$\rho(a)\leq\rho \qquad \text{for all } a.$$

To obtain Borel's result, Nevanlinna then proved that in general, $N(r,a)$ is the dominant term.

Indeed, he established the second Fundamental Theorem, from which it follows (after some work) that

$$\limsup\frac{N(r;a)}{T(r)}\geq\frac{1}{3} \text{ for all but at most two values of } a,$$

from which Borel's result follows, and $\rho(a)=\rho$ for all but at most two values of a, which is a much more precise result than Hadamard's inequality.

In his paper at the Bologna ICM, 1928, Nevanlinna discussed the question of what could be said about meromorphic functions f and g which have common values. That is, for some $a\in\mathbb{C}$ the sets

$$\{z: f(z)=a\} \text{ and } \{z: g(z)=a\}$$

are the same. He included values of a which are not taken at all, the so-called lacunary values. So for example, if f and g are entire functions and f is never zero, then $f(z)$ and $e^{g'z)}f(z)$ have two common lacunary values, 0 and ∞. Plainly, given f there is an infinity of functions of the form $e^{g(z)}f(z)$. But it is only for a small class of functions f that there are functions having three common values with f, and they had recently been exhibited explicitly by Polya and H. Cartan. Polya had also found the condition on f which allowed it have 4 common values with an other function. However, Polya had shown that if two meromorphic functions have 5 common values, then they are identical.

Picard's little theorem followed, Nevanlinna observed, by hypothesising that f was a meromorphic function with 3 lacunary values, and letting S be a Möbius transformation permuting those values cyclically and fixing two other points. Then the functions f and Sf would have 5 common values, which is impossible.

Nevanlinna proceeded to generalize these results. He defined $N(r)$ to be the maximum value of $N(r,z)$ over all values of z, so the quotient

$$\frac{(N(r)-N(r,z)}{N(r)}$$

takes values between 0 and 1, and he defined

$$\delta(z):=\liminf_{r=\infty}\left(\frac{N(r)-N(r,z)}{N(r)}\right).$$

This attached to a function f another function δ with values in the interval [0, 1]. Nevanlinna called this second function the defect function, and he showed that it characterised the density of the roots, a, of the equation $f(a)=z$. If the roots were relatively dense, the defect vanished. If z was a lacunary value, then the defect took its maximum value, 1.

It follows from the second Fundamental Theorem that there are only countably many points at which the defect does not vanish. Moreover, the sum of the corresponding defects is finite; indeed

$$\sum\delta(z)\leq 2.$$

As he observed, the little Picard theorem follows immediately from this result, and there is a straight-forward generalization to Polya's result.

It is not possible here to indicate the remarkable range of results that can be drawn from the two Fundamental Theorems of Nevanlinna theory. There was a great rush of activity by many authors, and since then whole books have been written on the subject. Nevanlinna both rounded off a whole generation's work and enabled a new period to begin.

Zurich 1932.

The Zurich ICM of 1932 was, nonetheless, not a particularly successful one for complex function theory. A profusion of short papers was presented, as befits a subject in the full flood of development. The main plenary address on the subject was Julia's, which, as we have said, was an historical overview. In its expanded form, as his *Essai*, Julia described some topics that we have not had the time to mention. The relationship between the coefficients of a power series convergent inside the unit disc and the behaviour on the boundary, investigated initially by Fredholm and then in more detail by Hadamard and others is as prominent there as it is absent here. But Julia's omissions are equally striking. Weyl's book on Riemann surfaces plainly lay outside the subject of complex function theory as the French conceived of it. The topic of conformal representation and univalent function theory was discussed, but the line of enquiry that ran from Schottky's theorem to work by Landau, Carathéodory and others on holomorphic functions on the disc was hardly acknowledged. It is tempting to see this as the natural view of a French function theorist in 1932: the Hadamard-Borel direction had been shown to be more profound and fruitful.

Before we turn to our coda, the ICM in Oslo in 1936, we should like to present some concluding remarks. An energetic follower of complex function theory from 1897 to 1932 would have seen many changes in the subject. Among those we have discussed:

- An increasing preference for the Cauchy-Riemann definition of a complex function to the Weierstrassian one, with all that that entails for the acceptance of integration and the role of the Cauchy integral theorem.
- A theory of entire functions, based on the concept of order, that established characteristic properties of complex functions.
- A vigorous debate about the nature of analytic continuation.
- The best proofs of Picard's theorems: elementary in the sense of Borel; via normal families as Montel proceeded; within Nevanlinna theory.
- A prolonged, largely unsuccessful search (prior to Nevanlinna) for a theory of meromorphic functions that would match the theory of entire functions.
- Conformal mappings and ideas of Riemann surfaces were not much in evidence, especially among the French. However,
- Geometry could not be kept away indefinitely, as Ahlfors was about to demonstrate.

The sweep of ideas has one aspect that is so obviously important it is tempting not to mention it: Despite Hurwitz's insight into the state of the art around 1897, the later development of the subject was markedly French, and almost as much Scandinavian. After Poincaré come Borel, Hadamard, Fatou and Julia; after Weierstrass come Mittag-Leffler, Lindelöf, the Nevanlinnas, and Ahlfors. The German interest, although represented by Schottky, Landau, Bieberbach and Ostrowski, is less effective. The French influence is very strong, determining even the choice of language in which the papers tend to be written. The shifts in attitude that we have described are at least in part the result of this cultural achievement.

Coda: Oslo 1936.

Perhaps nothing indicates better the high esteem the new theory of complex functions was enjoying than the ICM at Oslo in 1936. Naturally, we wish to conclude by mentioning it briefly, because it was not only the occasion for the award of the first Fields Medals, but for the award of a medal to Lars Ahlfors for his work on function theory, specifically his work on covering surfaces. The medals were presented by Carathéodory (1937). In presenting the medal, he said

> I do not know which is the more remarkable: that Ahlfors has been able to describe Nevanlinna theory in just 14 pages, or that Nevanlinna had been able to have the profound insights he had had without geometry. [1937, 85].

As this remark illustrates, the twist that Ahlfors (and independently of him, T. Shimizu) gave to the ideas of Nevanlinna was to rework them in geometric terms. In particular, in one of the commentaries in his *Collected Papers* Ahlfors writes that is was well-known early on that Nevanlinna's second main theorem was connected in some way with the Riemann-Hurwitz formula for coverings. Ahlfors extended this formula to coverings where the boundary of the covering space is sometimes mapped onto the interior of the space being covered. This had the effect of replacing Nevanlinna's functions by integrals expressing average values. The remarkable result that emerges was the connection with the Gauss-Bonnet formula, culminating in the explanation of the number 2 in Picard's theorem. The number of excluded values is determined by the Euler characteristic - which, for the Riemann sphere, is 2. And, to quote from the penultimate paragraph of his Oslo address:

> The second theorem of Picard says, in our formulation, that every covering surface of a surface of genus greater than 0 must be hyperbolic. [Ahlfors 1936, 248].

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Only after our paper had been presented to the ICHM Symposium of the International Congress of Mathematicians (Zurich 1994) did we come across an article which should certainly be consulted for the historical view of one of the most distinguished modern exponents of complex function theory:

Hayman W. K., *Function Theory 1900-1950*, in: *Development of Mathematics 1900-1950*, (Pier, J.-P. ed.), Birkhäuser, Basel, (1994) 369-384.

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