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Poincaré's Path to Uniformization

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Abstract

This study features the enormous conceptual leaps by which Poincaré in 1880, via his study of Fuchs's work, established the existence of a unique (uniformizing) differential equation and thereby his theory of general transcendental automorphic (Fuchsian) functions. Poincaré derived the Riemann surface naturally and established its nature via the hyperbolic metric. Particularly astonishing was Poincaré's linkage of his new functions with quadratic forms in arithmetic, and the unique model of hyperbolic geometry on the hyperboloid he created in establishing this linkage. Poincaré's path into this new world of mathematical action continued to widen and deepen, and the mathematical community persistently probed his arguments, leading in 1900 to Hilbert's problem #22 seeking rigorous proof of Poincaré's 1883 generalized uniformization of analytic curves. Mittag-Leffler played an instrumental role throughout – inaugurating *Acta Mathematica* in 1882 with Poincaré's uniformization theory, publishing Poincaré's 1906 rigorous proof of generalized uniformization, and, in 1923, documenting Poincaré's 1880 engagement with Fuchs' work. We reconcile Poincaré's and Klein's divergent perspectives on uniformization with the philosophical concept of the "fundamental dialectic of mathematics", which recognizes that a mathematical advance exists historically as *both* a rupture and a continuity. We show that Poincaré had, through his unique path to a uniformizing differential equation, added something new and epochal to the understanding of the Riemann surface and Riemannian principles. What was *ex post facto* seen as necessary within Klein's Riemannian program had emerged unforeseen and naturally through Poincaré's unique path outside the Riemannian program.

1 Introduction

On 23 May 1908, several months after the publication of his proof of generalized uniformization (Hilbert's #22), Henri Poincaré captivated his audience at the Institute of Psychology in Paris in a lecture on "L'invention mathématique" [Poi08], explaining how he came to write his first treatise on Fuchsian functions nearly

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three decades earlier. Hilbert's 1900 address at the International Congress of Mathematicians (ICM) in Paris had brought Poincaré's most general uniformization theorem [Poi83] to the attention of a new generation of mathematicians. Recent scholarship in the history of mathematics has emphasized the central role of narrative within mathematics [DM12], and Poincaré's 1908 lecture is among the most well-known — describing the opening drama in 1880 of Poincaré's unique path to uniformization. This paper shows how it soon became a shared story of a high-stakes mathematical adventure, a new high-risk mathematical journal, and the discovery and publication of historically-invaluable 1880 documents — a story within which Mittag-Leffler plays an instrumental role throughout. In a real sense, to tell the story of Poincaré's path to uniformization is to tell a story about Mittag-Leffler's vision of *Acta Mathematica*.

When Mittag-Leffler invited Poincaré on 29 March 1882 to publish the papers on Fuchsian functions in the inaugural issue of *Acta Mathematica*, he enabled mathematicians to have access to the full presentation of the theory before the end of the year. But, as Mittag-Leffler stated 40 years later in his preface to *Acta* 39 (1923) [ML23, p. III], one should not imagine that Poincaré's work "at the beginning" received the kind of praise that it later attained, quoting Kronecker's concern that the new journal was destined to failure for publishing "a work so incomplete, so little ripened, and so obscure" as Poincaré's new theory seemed at that date. *Acta* 39 was publishing for the first time, "with the authorization of the Academy of Sciences" in Paris, Poincaré's un-edited prize-submission paper of May 1880, which finally provided a definitive answer to the "enigma", which was so troubling at the time, of "what connection existed" between Poincaré's Fuchsian functions and "the preceding works of Fuchs". Mittag-Leffler added that "Klein's attitude in this question appears clearly in a correspondence between Poincaré and him", a correspondence also published in the volume.

Section 2 of this paper explores Poincaré's unique path to uniformization in 1880 through the work of Hermite and Fuchs, namely, Poincaré's need to establish the existence of a uniformizing differential equation from whose solutions he derived the Fuchsian functions. Section 3 introduces an interpretative framework from the philosophy of mathematics with which (in Sections 6, 8, and 10) we address Klein's insistence that the theory be derived from Riemannian principles. Section 4 features the Three Supplements — a handwritten record of the reasoning that took Poincaré from the concerns of Fuchs (via Hermite), in three dramatic stages, to the creation of a theory of Fuchsian functions. In Section 5 we find Mittag-Leffler corresponding with Poincaré in April 1881 regarding relations with other functions discovered by Weierstrass and Hermite, and connections to the Jacobi theta series in the theory of elliptic functions. Section 6 examines the Klein-Poincaré correspondence (beginning 12 June 1881) wherein Poincaré learns Riemannian principles from Klein, while Klein learns how Poincaré is advancing those principles. In his second letter, Poincaré creates his hyperbolic ball generalization in disproof of Klein's claim that Poincaré's non-Euclidean geometry approach would not apply for Schottky-type examples. Section 7 focuses on the creation of *Acta Mathematica*, and details how Poincaré addressed the concerns of Schwarz and Klein in the historical section of each *Acta* paper. Section 8 moves forward to Hilbert's #22, the research Seminars at Göttingen that (as Klein would later note) included Koebe in attendance, and Klein's announcement in 1907 of Koebe's proof. Section 9 produces two letters in the Mittag-Leffler archives, the first one establishing Poincaré's submission of his proof of Hilbert's #22 to *Acta* on 13 June 1906. The second letter, dated 4 March 1907, appears to be a clever nudge reminding Mittag-Leffler to print the paper. Section 10, Concluding comments, highlights Klein's 1909 Seminar on mathematical creativity and suggests that Klein at that time had not yet come to understand Poincaré's creative path to uniformization through engagement with works of Fuchs and without awareness of Riemann's work. Such a historical understanding became possible with the 1923 publication in *Acta* 39 of Poincaré's unedited 1880 paper.

Note that the idea of a uniformizing differential equation is not just of historical interest, for it has been key to the formulation and application of multi-dimensional uniformization theorems. See for example [DDH17] in this volume for the ball quotient uniformization and Shimura subvarieties.

2 The uniformization question as Poincaré saw it in 1880: The path via Hermite and Fuchs

Hilbert's problem #22 asked for a rigorous proof of the theorem on the "uniformization of Riemann surfaces" as formulated in 1883 by Poincaré [Poi83], a culmination of his creative surge of 1880 when he created the theory of Fuchsian (automorphic) functions, anchoring them in non-Euclidean tessellations and their groups. Throughout the 19$^{\text{th}}$ century, there were many conceptions of uniformization, and efforts to grasp a theory of uniformization, some of which did ultimately play a role both in Poincaré's theorem as formulated and in his eventual proof of that theorem in 1906. Note the date. But Poincaré's path to the creation of his theorem was distinct, and his reasoning utterly new, for at the time he was not familiar with Riemann's work, or of the related works of Hermann Schwarz on triangle functions or of Klein on modular functions. Poincaré's path to uniformization in 1880 was via the study of second-order linear differential equations, and his goal at the outset was not uniformization but to solve these differential equations. It was by studying a paper in which Fuchs pursued an analogy with the elliptic functions — studying the inverse function of a quotient of two independent solutions of a second order differential equation — that Poincaré first engaged the issue; it was in trying to assess the global nature of such solutions that Poincaré came to construct Riemann surfaces naturally from discontinuous groups [Gra00, pp. 199–200] [dSG16, pp. 141–142].

Thus, to formulate and assess the uniformization question "à la Poincaré" is to address the problem of the existence of a "uniformizing" differential equation [dSG16, p. 209] and the birth of the "Fuchsian functions" that are derived from its solutions. As explained in [dSG16, p. 207],

> Among all the normal differential equations defined on a single Riemann surface S, there exists a *unique* [uniformizing] one the inverses of

> whose solutions are single-valued in a disc D. This yields a parametrization of S by D. Just as an elliptic curve is the quotient of $\mathbb{C}$ by a lattice acting as group via translations, the Riemann surface S is the quotient of the disc by a discrete group of holomorphic automorphisms.

Interestingly, textbook introductions to the theory of automorphic functions follow Poincaré's approach as the natural one.[1]

Great advances in analysis stemming from the theory of elliptic functions had long stimulated efforts to find new transcendentals beyond the elliptic function and the modular function, functions that are single-valued and periodic. Historically, Euler's arcsine function had prompted Gauss, via a slightly modified integrand, to explore boldly with complex numbers, arriving at a function periodic in two linear independent periods. Legendre's three volume *Treatise* on elliptic functions of a single real variable catalogued examples. Abel and Jacobi went far beyond Gauss by 1827, investigating so-called "Abelian integrals" of a rational function of x and y, where y is an algebraic function of x, making great advances by studying integrals of a particular form containing a parameter k (called the modulus) that did not vary within the integral. The theory of modular equations grew from their studies of "transformations" relating the value of the integral for different values of k. Jacobi created theta-functions as tools for constructing elliptic functions as ratios of holomorphic functions.

It was Poincaré's mentor, Charles Hermite, who had encouraged Fuchs in 1876 to consider the analogy with elliptic integrals, and who encouraged Poincaré to read Fuchs' paper of 1880 as soon as it appeared. Hermite provides the crucial conceptual link between the efforts in the 1870s in Germany (within the Riemannian program) to elaborate the theory of modular functions independent of the theory of elliptic functions, and Poincaré's 1880 advances (with no knowledge of Riemann's works and principles) via the works of Fuchs seeking solutions to differential equations. Voluminous conceptual advances on these questions during the 1860s and 1870s crossed the borders of mathematical fields and national boundaries, in particular through the works of Riemann, Hermite, Fuchs, Schwarz, Jordan, Kronecker, Dedekind, and Klein, among many others. Gray [Gra00, pp. 101–115] and Bottazzini and Gray [BG13] assess this intricate creative path: *from the functions introduced in* 1858 *by Hermite* (who noted their invariance under certain transformations) in order to obtain a solution of the quintic equation by modular functions; *through Kronecker's* "exceptionally beautiful results" that emphasized the fundamental importance of Hermite's observation regarding this invariance; *to Dedekind's* impulse to elaborate the theory of modular functions as functions on a lattice and thus independent of the theory of elliptic functions; *to Klein's* inspired researches regarding this new "geometric" interpretation of modular functions (ac-

[1] [Sti85, pp. 18–19] In his May 1880 prize essay submission (unedited mémoire), Poincaré's investigation "of the special case [of the non-overlapping property] was enough, however, to show him the right place to begin a theory of automorphic functions — in non-Euclidean tessellations and their groups. In the three long *Acta* papers which followed in 1882 and 1883, Poincaré developed the theory from this starting point, and the main lines of his exposition have been followed in textbooks down to the present day, notably Fricke and Klein [1897, 1912], Ford [1929], and Lehner [1964]."

knowledging Dedekind) that led Klein to link his results (via Jordan) with the Galois theory of groups in search of a better understanding of the moduli and to produce thereby a unified theory of modular functions. Dedekind meanwhile continued his independent work, in response to a paper of Fuchs studying elliptic integrals as a function of a parameter, with the aim of developing all these ideas algebraically.

In this conceptual chain, the works of Hermite and of Fuchs were crucial in advancing both the German researches within the Riemannian program and the researches directed by Hermite, linked to his independently-established research program building upon Gauss's *Disquisitiones Arithmeticae* [GSS10]. This puts in stark view the uniqueness of Poincaré's path to automorphic functions and uniformization — independent of the Riemannian program and ignorant of its great riches.

Poincaré knew only Hermite's work and those of Fuchs that Hermite had given him to read. Poincaré's "historical" review in his first *Acta* paper in 1882 shows how he viewed these matters after the German writings and the Riemannian perspective had been made known to him by Klein in late June 1881.

> The first example of a discontinuous group consisting of linear transformations is the one encountered in studying the modulus k of an elliptic function (usual notation) or the modulus J (notation of Klein) as a function of the ratio of the periods. Hermite has made a profound study of this sort of transcendant and, in showing that it is uniform, has shown at the same time that the corresponding group is discontinuous. The functions k and J, and hence the corresponding discontinuous group, have been studied by Dedekind, Fuchs and Klein, and more recently by Hurwitz. We mention in particular the important works of Klein found in the *Mathematische Annalen*, and a remarkable memoir of Fuchs in Volume 83 of Crelle's Journal. It is evident that any group G contains infinitely many others which are all discontinuous if G is; thus knowledge of a single discontinuous group easily permits the formation of infinitely many others. It is this remark which is the point of departure of the beautiful researches of Klein on the transformation of elliptic functions, and on modular functions in general [PS85, pp. 126–127].

Poincaré continues these historical comments with a focus on Schwarz's work on the hypergeometric equation (see Section 7), linking Riemann's perspective with that of Weierstrass, a perspective shared by Fuchs and Hermite.

We will see that Poincaré in 1880 introduced something new: from his study of the "uniformizing" differential equation, Poincaré derived the Riemann surface naturally and established its nature via the hyperbolic metric.

3 What is at stake: The "fundamental dialectic of mathematics", and what Poincaré added to the theory of Riemann surfaces

This paper features a perspective articulated by the renowned philosopher of mathematics Jean Cavaillès [Cav46] that goes to the heart of what actually concerned Klein, what it was that he had difficulty accepting based upon his own mathematical experience, and what Poincaré realized could not be resolved even if he were to accept Klein's demand that he change the name of his "Fuchsian functions". Cavaillès found within mathematical practices what he called "the fundamental dialectic of mathematics" — in effect "an alliance between the necessary and the unforeseeable", as so aptly expressed by the philosopher of mathematics Emily Groscholz in her much broader assessment of "representation and productive ambiguity in mathematics and the sciences" [Gro07, pp. 189–192]. In "La pensée mathématique", written in collaboration with the philosopher of mathematics Albert Lautman, Cavaillès claims that a mathematical result "exists" only in, and as, the link which it establishes between two contexts — the context from which the result issues, and the context which the result in turn produces; and that link of its very nature exists historically as "both a rupture and a continuity" [Cav46].

Reinhold Remmert, one of the founders of the theory of several complex variables, wrote a historical review of the development of the theory of Riemann surfaces in complex spaces to which we now turn. From Remmert's statement, we see that the Fuchsian groups and functions that issued (unforeseen) from Poincaré's path to the uniformizing differential equation "existed" as a rupture in Klein's Riemannian research until Klein came to understand their profound generalizability.

> Much has been said about the genesis of the theory of uniformization for algebraic Riemann surfaces and the competition between Klein and Poincaré. However there was never any real competition. Poincaré, in 1881, had the Theta-series and hence was far ahead of Klein; as late as May 7, 1882, Klein asks Poincaré how he proves the convergence of his series. It is true that Klein, unlike Poincaré, was aware of most papers on special discontinuous groups, in particular those by Riemann, Schwarz, Fuchs, Dedekind and Schottky. ... [In 1879, Klein constructed] a beautiful symmetric 14-gon as a fundamental domain. But Klein restricted himself to the consideration of fundamental domains which can be generated by reflection according to the principle of symmetry. Of course he was aware of the connections between fundamental domains and non-Euclidean geometry, but it seems that he never thought of attaching a fundamental domain to an arbitrarily given discontinuous group. According to Dieudonné [1975], Klein set out to prove the Grenzkreistheorem only after realizing that Poincaré was looking for a theorem that would give a parametric representation by meromorphic functions of all algebraic curves. Klein [1882] succeeded in sketching a proof independently of Poincaré. He used similar

> methods (suffering from the same lack of rigor) [Rem98, pp. 214–215].

Poincaré, in turn, learned that his new results exist as a natural (necessary) addition to Riemann's program in a movement toward generalized uniformization [Gra82, p. 221].

4 Poincaré's creative thrust of 1880: The Three Supplements to his prize submission to the French Academy of Sciences

Poincaré was well-prepared for the challenge of the Academy's 1880 prize question: To improve in a significant way the theory of linear differential equations of one independent variable. His thesis (August 1879) focused globally on properties of functions defined by partial differential equations, their singularities and critical points. He would publish his revolutionary qualitative theory of differential equations in three parts during the same years that he completed his papers for *Acta* on his theory of Fuchsian and Kleinian functions. Moreover, Poincaré's creative advances on number-theoretic issues included papers on properties of quadratic forms in 1879 and 1880 — preparing him for an astonishing realization.

Together, the 1880 documents[2] — Poincaré's prize submission (28 May) [Poi23] [PS85, pp. 305–356], his correspondence with Fuchs (29 May to 30 July) [Poi21a], and the Three Supplements [Poi97] — record Poincaré's unique path to creation of his theory of Fuchsian functions. These hand-written documents fully support the details Poincaré recalled in "L'invention mathématique" (23 May 1908) [Poi08] more than a quarter-century later. None of the 1880 documents were accessible until *Acta Mathematica* published the correspondence (in 1921) and the prize submission (in 1923). The Three Supplements of 1880 remained unknown until 1980, a century later.

4.1 June 1880 First Supplement

Poincaré developed the theory of generalized transcendental automorphic functions based on two profound advances made here — the explicit theta series he established, and his subsequent discovery that he could establish the precise conditions for non-overlapping (single-valuedness) by anchoring the theory in hyperbolic tessellations and their groups.

As recalled by Poincaré in "L'invention mathématique" in 1908:

> It is time to penetrate further [into mathematical creativity], and to see what happens in the very soul of the mathematician . . . relating how I wrote my first treatise on Fuchsian functions.
>
> For two weeks [late May-early June 1880], I sought to prove that no function analogous to what I have since called Fuchsian functions

[2] [Gra00, pp. 173–184] translates and assesses the letters in conjunction with the Supplements [Poi97].

> could exist.. . . One evening I could not sleep: the ideas crowded around me . . . as though beating against me, until two of them clung together, that is to say, they formed a stable combination. In the morning, I had established the existence of a class of Fuchsian functions, those which are derived from hypergeometric series.. . .
>
> I wanted next to represent these functions as the quotient of two series; . . . the analogy with elliptic functions guided me. I wondered what properties these series should have, if they existed, and I arrived without difficulty at forming the series that I called theta-Fuchsian.
>
> At that time [mid-June 1880], I left Caen, where I was living, to take part in a geological course undertaken by the École des Mines. The events of this trip made me forget my mathematical work; once we arrived at Coutances, we boarded an omnibus for some excursion or another; the moment that I set foot on the footboard, the idea came to me, apparently without anything in my previous thoughts having prepared me for it, that the transformations I had used to define the Fuchsian functions were identical to those of non-Euclidean geometry.

Poincaré's letter to Fuchs (12 June 1880) relates remarkable geometric properties of the functions, and asks for "permission to give them the name of Fuchsian functions." In his letter of 19 June 1880, Poincaré announced that he has further obtained "a much greater class of equations than you have studied, but to which your conclusions apply . . . These functions I call Fuchsian, they solve differential equations with two singular points whenever [the exponent differences] are commensurable with each other. Fuchsian functions are very like elliptic functions, they are defined in a certain circle and are meromorphic inside it."

Poincaré fully documents his reasoning with great energy in the First Supplement (80 pages) submitted to the Academy 28 June 1880:

> There are close connections with the above considerations [tessellation of the disc by "mixtiligne" quadrilaterals that results from successively operating on one of the quadrilaterals by two transformations, which form a group] and the non-Euclidean geometry of Lobachevskii. In fact, what is a geometry? It is the study of a group of operations formed by the displacements one can apply to a figure without deforming it. In Euclidean geometry the group reduces to *rotations* and *translations*. In the pseudogeomtery of Lobachevskii it is more complicated.. . . I intend to call this function a *Fuchsian function . . . The Fuchsian functions are to the geometry of Lobachevskii what the doubly periodic functions are to that of Euclid*.

A month later, he reports details of his theory to Fuchs (30 July 1880):

> [The new functions] present the greatest analogy with elliptic functions, and can be represented as the quotient of two infinite series in infinitely many ways. Amongst those series are those which are entire series playing the role of Theta functions. These converge in a certain circle and do not exist outside it, as thus does the Fuchsian function itself.

> Besides these functions there are others which play the same role as the zeta functions in the theory of elliptic functions, and by means of which I solve linear differential equations of arbitrary orders with rational coefficients whenever there are only two finite singular points and the roots of the three determinantal equations are commensurable. I have also thought of functions which are to Fuchsian functions as abelian functions are to elliptic functions and by means of which I hope to solve all linear equations when the roots of the determinantal equations are commensurable. Finally functions precisely analogous to Fuchsian functions will give me, I think, the solutions to a great number of differential equations with irrational coefficients.

4.2 August 1880 Second Supplement

In July, while engaging with his research on arithmetic groups, Poincaré realized that his functions apply much more widely. He conceptualized a new model of hyperbolic geometry, mapping tessellations of one sheet of a hyperboloid onto the hyperbolic disc.

As Poincaré recalled in "L'invention mathématique" in 1908:

> I then put myself to the task of studying arithmetic questions without any great apparent result and without suspecting that this could have the least relation with my previous research. Disgusted with my lack of success, I went to spend a few days on the seaside, and I thought about other things. One day, while I was walking along the sea cliff, the idea came to me, again with the same characteristics of brevity, suddenness, and immediate certitude, that the arithmetic transformations of indefinite ternary quadratic forms were identical to those of non-Euclidean geometry. Having returned to Caen, I reflected on this result and derived its consequences.
>
> The example of quadratic forms showed me that there were Fuchsian groups other than those corresponding to the hypergeometric series; I saw that I could apply to them the theory of theta-Fuchsian series and that, consequently, there existed Fuchsian functions other than those derived from the hypergeometric series, which were the only ones I knew up until then.

Poincaré explains his (August 1880) linked hyperboloid and conformal disk model of hyperbolic geometry (presented on 16 April 1881 at the Association française pour l'avancement des sciences in Algiers) in [Poi84] [Poi86] [Sti96, p. 121, translation]:

> One of the most important problems in the subject of indefinite ternary quadratic forms is the study of the discontinuous groups consisting of similarity substitutions, that is, substitutions which preserve the form. Let $F(x, y, z)$ be an indefinite quadratic form.
>
> We can choose the constant K so that $F(x, y, z) = K$ represents a hyperboloid of two sheets. The similarity substitutions then map a

> point on the hyperboloid to another point on the same sheet and, since the group is discontinuous, the hyperboloid becomes partitioned into infinitely many curvilinear polygons whose sides are diametric sections of the surface. [Intersections of the surface with planes through the origin.] A similarity substitution changes each polygon into another. We now take a perspective view by placing the eye at an umbilic of the surface and taking the plane of projection to be a circular section. One sheet of the hyperboloid is projected inside a circle, and the polygons drawn on this sheet project to polygons bounded by circular arcs of the kind we have discussed in the theory of Fuchsian groups. Thus the study of similarity substitutions of quadratic forms reduces to that of Fuchsian groups, which is an unexpected rapprochement between two very different theories, and a new application of non-Euclidean geometry.

Recorded in the Second Supplement (23 pages) submitted 6 September 1880:

> Definitions. [Poincaré reviews the hyperbolic geometry intrinsic to his theory of Fuchsian functions in the first 11 pages.] Relations with the theory of Quadratic Forms [2 pages]. *All the points z^*K are the vertices of a polygonal network obtained by decomposing the pseudogeometric plane pseudogeometrically equal to each other. The K substitutions are those that transform these polygons into each other, or even as we discuss below, those that reproduce the functions that we are going to define.* Having brought out the intimate and unexpected relations between the theories so different in appearance, I return to my principal subject. Generalization to thetafuchsienne functions [2 pages]. Generalization to Fuchsian functions [3 pages]. *To every decomposition of the pseudogeometrical plane into mutually congruent pseudogeometrical polygons there corresponds a function, analogous to the Fuchsian functions, and which enables us to integrate a second-order linear differential equation with algebraic, but irrational, coefficients.* One sees that there are functions, of which the Fuchsian function is only a particular case, which enable us to integrate linear algebraic differential equations. However, in order to determine whether a given equation is integrable in this way, a long discussion would be required which I do not wish to enter into for the moment, but reserve for later.

Poincaré had established Fuchsian groups in the context of the hypergeometric equation, not expecting them to apply more broadly, but found that he had a much wider theory of general transcendental automorphic functions to pursue at length.

4.3 December 1880 Third Supplement

Poincaré pushed forward to his theorem on the tiling of the hyperbolic plane, and extended his results to encompass the Legendre equation for the periods of an elliptic integral as a function of the modulus, moving towards uniformization.

As recalled by Poincaré in "L'invention mathématique" in 1908:

> Naturally I posed myself the problem of forming all of these functions; I made upon it a systematic siege, and one after the other I pulled off all of the leading fortifications. There was however one that still held and whose fall would lead to that of the whole guard. But all my efforts at first only served to improve my knowledge of the difficulty, which was already something. All of this work was perfectly conscious. Upon that, I left for Mont Valérien, where I had to do my military service; I therefore had very different preoccupations. One day, as I was crossing the boulevard, the solution to the difficulty that had blocked me appeared all of a sudden. I did not seek to immediately deepen the idea, and it was only after my service that I again took up the question. I had all of the elements; I had only to assemble and organize them. I therefore composed my definitive memoir at one stroke and without trouble.

As documented in the Third Supplement (12 pages) submitted 20 December 1880:

> Review. [Poincaré reviews the hyperbolic geometry he developed for the theory, 2.5 pages] Theta-Fuchsian functions [8 pages] ... *The transcendental that expresses the square of the modulus according to the ratio of the periods is thus the particular case of Fuchsian functions* ... Résumé [1.5 pages] ... I do not doubt, then, that the numerous equations envisaged by M. Fuchs in his memoir inserted in Volume 71 of Crelle's journal ... is but a particular case furnishing an infinity of transcendentals ... and that *these new functions will permit the integration of all linear differential equations with algebraic coefficients.*

5 Poincaré's stream of articles on his theory of Fuchsian functions (1 January 1881 — 11 June 1881): Mittag-Leffler corresponds

Poincaré had created a theory of general transcendental automorphic functions. He released his theory and findings in a dozen papers published between 14 February and 6 June 1881; he also delivered two papers at the Algiers meeting of the French Academy of Sciences on 16 April 1881. All of the papers (except those of April 16) were published in Volume 92 of the *Comptes rendus de l'Académie des sciences*. On 20 March 1881 he updated Fuchs about his results [Poi21a, p. 184].

Mittag-Leffler initiated a correspondence with Poincaré on 11 April 1881, requesting that Poincaré send a copy of his 1879 thesis [Nab99, pp. 51–53]. In a letter of 22 May 1881, Mittag-Leffler corrects Poincaré's citing of Hermite as the first to observe the existence of functions of "lacunary spaces", since Weierstrass had already long before described them in his work *Zur Functionenlehre* [Nab99, pp. 54–59]. Poincaré responded 1 June 1881, thanking Mittag-Leffler for his letter and reassuring him that far from blaming Mittag-Leffler for his interventions he was “delighted by the way you provide me to correct a historical error”, for indeed Poincaré had never read Weierstrass's account. Poincaré then addresses Mittag-Leffler's other questions:

> ...As regards the series ...I cannot say that I am ranked first as it so nearly resembles the series considered by Jacobi in the theory of elliptic functions; but I cannot say either that another is ranked before me; because both series are *nearly* the same without being *fully* the same. ...Now here is how I see the relationship between the s-series and the series of Jacobi. The modular function is a *Fuchsian function*; among the Fuchsian functions there is another that I call *arithmetic function* that is rationally expressed by the modular function. Any rational function of the modular function is expressed by the quotient of two functions similar to theta functions and which I call *theta-Fuchsian modular*; in the same way any rational function of the arithmetic f[unction] is expressed by the quotient of two *Theta-Fuchsian arithmetic* functions. Hence, the series of Jacobi are theta-Fuchsian modular functions, the s-series are theta-Fuchsian arithmetic functions. You asked me for an example of Fuchsian functions presenting a lacunary space; nearly all of those I have studied so far exhibit such a space. I will give you only as an example the modular function which is well known to you; or even the function defined in the following way: Consider the hypergeometric equation of Gauss and I suppose that the difference of the roots of the defining equations are aliquot parts of I. If we consider the variable as a function of the ratio of integrals, it will be a Fuchsian function presenting a lacunary space. I do not know when I will publish in detail my research on these types of Fuchsian functions; but I can give you some brief details. ... [Poi21b, pp. 147–149] [Nab99, pp. 60–69]

6 Poincaré learns Riemannian principles as Klein learns how Poincaré advanced them (12 June 1881 — December 1881)

On 11 June 1881, Klein read three papers by Poincaré entitled “On Fuchsian functions” and began a correspondence with him the next day.[3] Klein informed Poincaré of his own closely-related “reflections and endeavors” of the past several years in his work on elliptic functions and modular elliptic functions; Poincaré agreed that Klein had anticipated some of the results he obtained in his theory of Fuchsian functions. Klein then delineated all that he and Schwarz had done in related works and on special cases, and noted Schottky-type examples “for which

[3] In this paper we use the English translation of the letters found in “The correspondence between Klein and Poincaré” [dSG16, pp. 385–414].

there is no determined fundamental circle, so that the analogy with non-Euclidean geometry . . . does not hold." Poincaré responded (22 June 1881) that he had no doubt that the results of Klein and Schwarz would relate to his concerns in his paper (his first uniformization theorem) of 23 May 1881. He also asked for permission to cite a passage from Klein's letter in a communication Poincaré would give to the Académie in which he would "try to generalize" Klein's result — in effect, by disproving Klein's claim that Poincaré's non-Euclidean geometry analogy would not work. Poincaré had conceptualized the three-dimensional geometric (hyperbolic ball) interpretation of what he would call "Kleinian" groups. Taking Klein's silence as acquiescence, Poincaré published his hyperbolic ball generalization on 27 June 1881, informing Klein on 5 July.

Meanwhile Klein vigorously opposed the name Fuchsian functions as unwarranted, and also proposed a general uniformization theorem of his own on 2 July 1881 "if one is prepared to use Riemann's principles." On 9 July, 1881, Klein provided the source for Schwarz's results that supply "more rigorous methods of proof" in support of Riemannian principles. And he vigorously opposed the name "Kleinian functions" as well; he would "stay with [his] functions invariant under linear transformations". In December, Klein acknowledged that Poincaré had "definitely proved (as of August 8): that every linear differential equation with algebraic coefficients is integrable by means of zeta Fuchsian functions of an auxiliary variable", a uniformization theorem. He invited Poincaré to prepare an article for the next issue of the *Annalen*, which appeared in March 1882 [Poi2a] with Klein's comments adjoined to it:

> The investigations that Mr. Schwarz and I published a long time ago in the field under consideration deal with Fuchsian functions, about which Mr. Fuchs has not published anything . . . [Also] all research that is discussed here, both what is geometrical in reasoning and the more analytic work that is connected with the solutions of linear differential equations is based on Riemann's ideas. The coherence is even greater because one can state that in the research of Mr. Poincaré, what really counts is the further continuation of the general complex function program formulated by Riemann in his dissertation.

Poincaré responded saying that he could not leave readers with the impression that he had committed an injustice. He strongly agreed that the genius of Riemann has been imprinted on everything that has developed or will follow in mathematical analysis. Poincaré's justification for why Schwarz's name would not have been more appropriate than Fuchs's [Poi2b] was extensively supported in his *Acta* papers.

7 Mittag-Leffler's invitation: *Acta Mathematica* is born

On 29 March 1882, Mittag-Leffler invited Poincaré to publish his paper on his "remarkable" Fuchsian functions in the first volume of a new international journal for mathematical research that the Scandinavian mathematical community wished to establish. Poincaré's path to uniformization led also to *Acta Mathematica*.

> I think I do not deceive myself when I assure you that your discoveries will rank with those of Abel and that your functions are the most remarkable to have been found since the elliptic functions. But certainly M. Klein is right that you are wrong to name your functions Fuchsian and Kleinian functions. They ought to carry the name of Poincaré's functions. It is the only name that is fair and reasonable. ... And now I have a proposition I would like to make to you. We, the mathematicians in the Scandinavian countries, have a project to publish a new mathematical journal. ... [We] have thought that you, a Frenchman, would perhaps be generous enough to wish us success with our new journal. Would you wish to send us your memoir "Sur les groupes fuschiens" to be published as the first memoir of the journal? [Nab99, pp. 87–93]

Mittag-Leffler asks Poincaré for discretion about the new journal: "Please do not say anything to anyone yet about our project because the realization of this project depends on you" just as the success of Crelle's journal (in Germany) earlier in the century had depended on its publication of the groundbreaking works of Abel. He adds that if Poincaré refuses, the editors will have to delay establishment of the journal several years. He had nothing to fear. By the end of the week Poincaré mailed his paper to Mittag-Leffler for publication in the *Acta*. Poincaré's accompanying letter is lost, but Mittag-Leffler's reply of 10 April 1882 thanked Poincaré warmly for his "kindness": "I was touched by your good and kind letter and blessed by your splendid gift that you gave us by your memoir." [Nab99, pp. 93–95] Mittag-Leffler states that he is writing to Hermite today to tell him about the project and ask that the French mathematicians Picard and Appell also become collaborators on the journal.

> It is necessary however that no one in Germany know anything thereupon before the end of the month of July when I will visit Germany myself. The Germans ... have not forgotten [Sweden's] sympathies for France during the last war. I have good relationships in Germany however and by employing a little skill I do not doubt that I will win many of their most distinguished authors.

Mittag-Leffler's July trip to Germany convinced many German authors to publish in his new journal [Nab99, pp. 93–95].[4] But as he told Poincaré on 18 July 1882:

> I found he [Schwarz] was indignant with you. He believes he was the first to give an example of the groups you call Fuchsian that were

[4]Nabonnand (p. 91) notes that French authors made up 40.1 percent of the journal's authors, followed by German authors at 30.7 percent, and trailed by authors from other nations (the next largest being Swedish authors at 12.8 percent). Nabonnand (p. 95) also observes that Klein in his 1926 lessons on the history of mathematics recognized the entrepreneurial and diplomatic qualities of Mittag-Leffler.

> not encountered in the theory of elliptic functions. ... For your part, you must find proof of the importance of your discoveries in all the arguments with the German mathematicians about your new terms. M. Schwarz makes no secret at all of the fact that it is the Fuchsian functions above all that make him suffocate with fury.

On 27 July, Poincaré reported to Mittag-Leffler the results of his careful examination of Schwarz's works to confirm that there were no issues of priority. Poincaré also pointed to the relevant results from Schwarz's papers he would note in his *Acta* publications. He concluded that he did not see reason to change his terminology.

> I reread the memoirs of M. Schwarz to see if I had something to modify in the redaction of my history. These memoirs form a sort of series and one can see in it the development of the ideas of M. Schwarz.... In this series of memoirs, there is nothing but a few lines that relate to the question that occupies us and I will textually cite them soon.... So in summary, M. Schwarz obtained the two following results. The first of these results has no relation with the discontinuity of groups. I will cite it perhaps in my second memoir ["On Fuchsian Functions"] where it is a question of functions and no longer of groups. I would not have to speak about it since the important point, the uniformity, is not touched on, but I prefer to do so. In my first memoir ["On Fuchsian Groups"], this citation would not be appropriate. As to the second result, it is this that I cited and I do not see what I could change in my citation. You will see however in the page proofs that I'm sending you, that I have added there a phrase destined to bring out the importance of this result.

The historical section in his first *Acta* paper [Poi82b] [PS85, p. 127] reads:

> Apart from the groups contained in the modular group, whose discontinuity is evident, there is another group whose discontinuity has been noted by Schwarz in a memoir in Volume 75 of Crelle's Journal; this is Example I of Section VII. This is the first time we arrive at such a result without starting from the theory of elliptic functions. Finally, Fuchs takes up an analogous question in the work in Volume 89 of Crelle's Journal and in the proceedings of the Göttingen Society. Although the groups studied in this latter work all reduce to previously known groups, it was a reading of this remarkable memoir which guided me in my first researches and which enabled me to find the law for the generation of Fuchsian groups, and to give a rigorous proof of it.
>
> I first published them in a memoir I had the honour of submitting to the judgement of the Académie des Sciences in the concourse for the Grand Prix of mathematical sciences on 1st July 1880 [*sic* 28 June 1880, the First Supplement], and I have pursued the study of the groups in a series of works in *Comptes rendus* for the year 1881.

In the historical section at the end of his second *Acta* paper [Poi82a] [PS85, p. 254], Poincaré wrote, after a detailed summary of Schwarz's results, that:

> I could have made use of these results, but I have preferred to follow another path:
>
> 1. Because I could otherwise have proved the existence of Fuchsian functions only when the polygon R was symmetric.
>
> 2. Because the series expansions I have used not only give a proof of existence of the function, but also its analytic expression.

Poincaré concluded his letter to Mittag-Leffler with the frequently quoted statement that he is not able to resolve that which was actually the cause of Schwarz's anger:

> I don't hope to calm M. Schwarz down. In fact, what is the cause of his anger? First of all he is angry for having had an important result in his hands and not having profited from it. I can do nothing about that. Then he is unhappy that the name Fuchsian has been preferred to Schwarzian. On that I can add nothing to what I have already said.

Acta Mathematica gave Poincaré an outlet for immediate publication in 1882 of the full details of his theory of Fuchsian groups and Fuchsian functions. By the time Klein published Poincaré's response to Klein's *Annalen* comments — namely, in the next *Annalen* issue which appeared in December 1882 [Poi2b] — Poincaré's response was backed by his two fully developed articles published in *Acta Mathematica* 1 (1882).

Poincaré's "Memoir on Kleinian Groups" (*Acta* 3, 1883) concludes with a brief historical section [PS85, p. 304] where he refers to his creation of the hyperbolic ball approach in June 1881, when he also had labeled such groups Kleinian:

> It was Schottky who first noted the discontinuity of certain Kleinian groups ... namely the symmetrical groups of the third family. Later, Klein went deeply into the theory of these groups in various notes inserted in the *Mathematische Annalen* (Vols. 19, 20, and 21) and in a memoir also in Volume 21 of the same annals, entitled *Über Riemannsche Functionentheorie*. I have myself, in two notes I had the honor of presenting to the Académie des Sciences de Paris on 27 June and 11 July 1881 (see *Comptes rendus*, Vols. 92 and 93), succinctly announced the greater part of the results given in the present memoir.

Poincaré would publish 22 more articles in *Acta*, including his proof of Hilbert's #22.

8 Poincaré's generalization to analytic relations (1883), Hilbert's #22 (1900), and the Klein Seminars at Göttingen (1904—1907)

In his 1884 self-analysis of his work (submitted for his election to the Academy) [Poi84], Poincaré reviewed his path to uniformization.

> Is it always possible to make this choice so as to satisfy all these conditions? Such is the question that naturally arises. This comes down moreover to wondering if, among linear equations satisfying certain conditions that it is pointless to state here, there is always a Fuchsian equation. I have managed to prove that one can answer this question affirmatively. I cannot explain here in what consists the method we, M. Klein and I, have employed in studying diverse particular examples; how M. Klein has sought to apply the method in the general case; nor how I filled in the gaps which persisted in the proof of the German geometer in introducing a theory having the most profound analogies with that of the reduction of quadratic forms. . . . Thus it is possible to express the integrals of linear equations with algebraic coefficients in terms of new transcendentals, in the same way as one expresses, in terms of Abelian functions, the integrals of algebraic differentials. Furthermore the latter integrals are themselves susceptible of being obtained by means of Fuchsian functions and one then arrives at a new expression, entirely different from that involving theta-series in several variables [dSG16, p. 209].

Poincaré cites four of his articles in the *Comptes rendus* (8 August 1881, 17 October 1881, 10 April 1882, and 24 April 1882) and his article "Sur les groupes des équations linéaires" in *Acta Mathematica* 4 (1884, dated 20 October 1883). After Poincaré's 1883 generalization to the uniformization of any analytic function [Poi83], which was published concurrently in a leading French journal, Poincaré in effect "left the field" of automorphic functions, but continued to have enormous impact [Gra99b, pp. 11–12].

Schottky's early papers had built on the work of Weierstrass and Schwarz, but in 1887 Schottky [Sch87] adopted a new approach "modeled on the approach initiated by Poincaré" [BG13, p. 611], an approach Klein adopted and modified. In his research Seminars, Klein engaged students such as Ernst Ritter to bring Poincaré's ideas and methods into Klein's Riemannian program [Gra99b, pp. 11–12]. In [FK12, p. VII], Fricke wrote that Poincaré's 1887 article on "Fuchsian functions and arithmetic" [Poi87] inspired his whole research program.

At the 1900 ICM in Paris, Hilbert focused the attention of a new generation of mathematicians on Poincaré's general uniformization theorem, i.e., problem #22. (It was included as one of the 10 most important problems in the original spoken lecture.) Hilbert's #22 "Analytic relations expressed in a uniform manner by means of automorphic functions" [Hil00, pp. 105–106][5] reads as follows:

[5] This and all other English translations in this paper are by C.D., unless otherwise noted.

> We know that Poincaré was the first to prove that it is always possible to express any *algebraic* relation between two variables in a uniform manner by means of automorphic functions of one variable. In other words, given an algebraic equation between two variables, there can always be found for these variables uniform automorphic functions of a single variable which, substituted in the algebraic equation, renders it identically. The generalization of this fundamental theorem to any analytic non-algebraic relations between two variables has been done with great success by Poincaré [Poi83], though by a way entirely different from that which served him in the previous question of the algebraic case.
>
> But the proof by which M. Poincaré shows that it is possible to express in a uniform manner any analytic relation between two variables does not yet show us whether we can choose uniform functions of the new variable of the sort by which, while this variable traverses the *regular* domain of these functions, we obtain an effective representation of the set of *all* regular points of the given analytic function.
>
> On the contrary, it would seem, according to the researches of M. Poincaré, that besides certain branch points, it is still necessary in general to set aside an infinity of isolated points of the given analytic function, which is obtained only when the new variable approaches certain limit points of the functions. *Clarification and a solution of these difficulties seems to me extremely desirable in view of the fundamental importance of the question treated by M. Poincaré.*

We push forward to the Klein-Hilbert-Minkowski Mathematics Seminar for the Winter Semester of 1905—1906 [Klea]. Klein could not contain his enthusiasm:

> It is the fact that we are at the prospect of standing at the edge of developments in the area of linear differential equations and automorphic functions that motivated me to give you in the coming weeks a coherent overview of my old investigations on this subject, restricting myself to the most elementary case of the so-called hypergeometric functions (with only three singular points)....Not only did Riemann develop the foundations on which we descendants all build, he was in command of a variety of results that we later found independently....

Publication of long-lost notes from Riemann's 1859 lectures on hypergeometric functions, Klein continued, "show that Riemann had progressed even to the uniformization theorem." Riemann not only had already understood ideas that Schwarz later established but also "illuminated the elliptic modular functions in the manner later developed by Dedekind and Klein." [BG13, p. 302] Klein recalled the address by Wilhelm Wirtinger at the 1904 ICM. Wirtinger stated that Riemann "approached for the first time the problem of unique parametrization, which later Poincaré did in so general a way and just because of Riemannian principles." [Wir05, p. 134]

For Klein, the Seminar series aimed for a proof of general uniformization based on Riemannian principles. In the closing pages of his *Works III*, Klein proudly highlights the 1905—1907 Seminars with Koebe in attendance. On 13 June 1906, Poincaré submitted his proof of general uniformization to Mittag-Leffler for publication. Klein's Seminars continued for about a year before Klein, on 11 May 1907, presented a proof of general uniformizaton by Koebe to the Göttingen Mathematical Society.

9 Hilbert's #22 proved: Poincaré (1906) and Koebe (1907) — the evidence in the Mittag-Leffler archives

Beyond settling the issue of datation, evidence in the Mittlag-Leffler Archives throws interesting light on the history surrounding Poincaré's proof of Hilbert's #22. First, regarding the question of datation, archival letters establish unambiguously that Poincaré had *completed his proof prior to* 13 *June* 1906, not sometime in 1907, which was long assumed as best estimate. An exchange of letters in June 1906 gives *precise dates* for both the *submission* and the *receipt* that month of Poincaré's manuscript on the Uniformization of analytic functions for publication in *Acta Mathematica*. In a letter of 17 May 1906, Mittag-Leffler had written to Poincaré of his impatience to receive the memoir Poincaré had promised to send him ("J'attends avec impatience le mémoire que vous voulez bien m'envoyer") [Nab99, Doc. 1-1-231]. The exchange of letters shows that Poincaré fulfilled this promise within less than a month.

With a letter to "my dear friend" dated 13 June 1906, Poincaré sent "by registered mail" his proof ("mon manuscrit sur l'Uniformisation des fonctions analytiques") to Mittag-Leffler (the editor of *Acta Mathematica*), requesting that Mittag-Leffler send him two copies of the printed page-proofs so that he may keep one copy for himself.

> Poincaré to Mittag-Leffler, 13 June 1906
> My dear friend,
> I have the honor to send you by registered mail my manuscript on the Uniformization of analytic functions.
> When you send me the page-proofs, could you send them to me in double so that I could keep one.
> Please, I beg you to convey my respectful homage to Madam Mittag-Leffler and believe in my sincere friendship.
> Poincaré[6]

Mittag-Leffler replied a week later, on 20 June 1906, thanking Poincaré for this "magnificent gift" to the *Acta* and stating that it would be published in Volume 31, a new series to open with writings of Weierstrass on the three-body problem.

> Mittag-Leffler to Poincaré, 20 June 1906

[6]Poincaré to Mittag-Leffler, 13 June 1906 [Nab99, Doc. 1-1-232, p. 345]. ALS 2p. IML 139, Mittag-Leffler Archives, Djursholm.

My dear friend,

Thank you for the magnificent gift that you have made of your latest work for the *Acta*. We will publish it in Volume 31 which will open so-to-speak a new series and which will begin with the writings of Weierstrass on the three-body problem.

I will always send you two copies of the page-proofs.

Your devoted friend.
Mittag-Leffler[7]

We know that the journal printed Poincaré's article on 19 March 1907 — nine months after receipt of Poincaré's manuscript. It is not evident why the printing was delayed for so long. But the archives also contain a letter from Poincaré on 4 March 1907 that appears to be a clever "nudge" reminding Mittag-Leffler to print the memoir.

With the printer's page-proofs not yet received after those nine months, Poincaré had reason to inquire about the delay in the printing of his manuscript, and it seems that he did so by means of an indirect and amusing letter dated 4 March 1907 (received in Stockholm three days later) where Poincaré wonders:

Poincaré to Mittag-Leffler, 4 March 1907
My dear friend,

I ask myself what is the nature of the supply of electrical current in Stockholm, continuous or alternating? Voltage? Etc., etc. Could you inform me about this point. I take this opportunity to recall myself to your memory [pour me rappeler à votre bon souvenir] and to beg you to kindly send my respects to Madam Mittag-Leffler.

Poincaré[8]

The first page of Poincaré's published article states that it was printed on 19 March 1907 ("Imprimé le 19 Mars 1907"), eleven days after receipt of Poincaré's letter, a point of fact whether or not Poincaré's letter is the clever nudge as suggested here.

When was Poincaré's article published? Although Poincaré's proof of the Uniformization Theorem was *officially listed as published* in Volume 31 which is *dated* December 1908, there is indubitable evidence that it *appeared for scholarly use over a year earlier* (by November 1907), *it was printed nearly two years earlier* (on 19 March 1907), and *it had been submitted to the journal a further nine months earlier* (on 13 June 1906).

The evidence of Poincaré's November 1907 *publication* date [Poi07] comes from Koebe's publication of that month. Koebe published two different proofs of uniformization in 1907. Koebe's first proof (by a very different method from Poincaré's) was presented by Klein at a meeting in Göttingen on 11 May 1907 [Koe7a]. Koebe's November 1907 proof [Koe7b] appeared *after reading* Poincaré's

[7]Mittag-Leffler to Poincaré, 20 June 1906 [Nab99, Doc. 1-1-233, p. 345]. TLX 1p. Mittag-Leffler Archives, Djursholm.

[8]Poincaré to Mittag-Leffler, 4 March 1907 [Nab99, Doc. 1-1-234, p. 346]. ALS 1p. IML 141, Mittag-Leffler Archives, Djursholm.

proof in his *Acta* paper, directly addressed details of Poincaré's proof, and referenced Poincaré's paper in Issue 1 of *Acta* 31. Koebe states that reading Poincaré's treatise led him to return to "a thought which I had already considered, which led me to a new proof ... related to Poincaré's". In a footnote Koebe observes that "This [Poincaré's] treatise was already printed in March 1907, and is completely independent of [Koe7a]." [Koe7b, p. 634]

Archival documents thus establish that Poincaré submitted his proof of Hilbert's #22 nine months earlier than previously recognized. More important is the understanding gained regarding why, in 1906, Poincaré would create the "balayage" method of proof with its anchor in an electrostatic analogy. During 1906, Poincaré was deeply engaged with the question of electric telegraphy, and such work in physics and engineering often served as a rich resource for his thinking within mathematics.

10 Concluding comments

More than a quarter century after Klein's discovery of Poincaré's papers on Fuchsian functions, and shortly after the publication of the independent proofs of Hilbert's #22 by Poincaré and Koebe in 1907, Klein reacted to the publication of Poincaré's 1908 lecture "L'invention mathématique" by holding a seminar at Göttingen on the "Psychological Foundations of Mathematics". In a series of 16 sessions spanning 27 October 1909 to 2 March 1910, as Eugene Chislenko observes,

> one conspicuously absent topic is the writings of Poincaré. In the decade preceding the seminar, Henri Poincaré, the leading mathematician in France, had published a series of books that are among the major modern classics on almost all the topics in Klein's seminar. Poincaré's *La science et l'hypothèse* (1902), *La valeur de la science* (1905), and *Science et méthode* (1908) range across the connections between mathematics and the sciences, psychology, philosophy, and education, including reports of his own discoveries very much in the spirit Klein was looking for. Klein's notes even reiterate plans to discuss these works: "Analogous in Poincaré in ... (in general writings of Poincaré)"; "Who [will cover] Poincaré?"; "Poincaré. Science et méthode"; "Poincaré's self-report on the genesis of his 'fonctions fuchsiennes"'; "Diverse abilities. Can types be distinguished? My account in the Evanston colloquium. Poincaré's account." ... It is something of a mystery why, despite so many recognized connections, the seminar's protocols contain only one brief mention of Poincaré. Even that mention is a criticism by Klein, and it may be that tensions had not yet cooled since their competition in the 1880s.

Chislenko adds that the explanation to the "mystery" may in part be that "Klein simply ran out of time." [Kleb] [Chi, p. 14]

The mystery may also relate to Klein's not yet at that date having come to understand Poincaré's creative path to his theory — his representation of the

Riemann surface naturally through engagement with the work of Fuchs and without awareness of Riemann's work. Klein's understanding of Poincaré's path via Fuchs's work came only in the early 1920s when this "enigma" was solved by the publication in *Acta* 38 of Poincaré's correspondence with Fuchs and, finally, in *Acta* 39 of Poincaré's prize submission based on Fuchs's 1880 paper.

It may also in part be explained in terms of the dialectic between the necessary and the unforeseeable in mathematics. Klein insisted on the mathematical necessity of the Riemannian perspective. The discovery and publication of Riemann's 1859 lectures on hypergeometric functions served to confirm Klein's argument (to quote from Wirtinger) that Poincaré was able to create his theory "in so general a way ... just because of Riemannian principles". So the necessity Klein identified is precisely that unforeseen necessity independently created by Poincaré in 1880. In a lecture in 1916, Klein recalled his "sleepless night" of inspiration on 22–23 March 1882 when he came to his Grenzkreis theorem based on Riemannian principles; this was after Klein had grasped "the full generality of Poincaré's ideas". [Gra99a, p. 129]

The mathematics of uniformization advanced through the works of many mathematicians (from various nations and institutions), across diverse mathematical fields, utilizing a variety of mathematical approaches, but always through the "fundamental dialectic of mathematics", which recognizes that a mathematical advance exists historically as both a rupture and a continuity. Mittag-Leffler's publication of Poincaré's groundbreaking theory launched a new international mathematical research journal, *Acta Mathematica*. Poincaré had, through his unique path to a uniformizing differential equation, added something new and epochal to the understanding of the Riemann surface and Riemannian principles. Poincaré had recognized hyperbolic geometry — the hyperbolic tessellation — as key to establishing the theory of general transcendental automorphic functions and, ultimately, as reflecting the essential nature of Riemann surfaces. As with all epochal advances, it was only the beginning.

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