JEREMY GRAY

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Poincaré in the Archives — two Examples

Jeremy Gray
Faculty of Mathematics and Computing
Open University

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Abstract. By way of indicating the riches of the archival material on Poincaré, of which only a part is known to me, and its importance in changing and enriching our understanding of his work, I describe two examples. One is the discovery of three extended essays written in 1880 on differential equations in the complex domain and recently published by Scott Walter and myself through ACERHP. The other is the discovery, made by June Barrow-Green, of the original version of his prize-winning memoir of 1888 on celestial mechanics. Each manuscript illuminates the path of his thought to some of his most influential discoveries.

Résumé. A titre d'illustration de la richesse du matériel d'archives de Poincaré, dont une partie seulement m'est connue, et de son importance pour l'évolution et l'enrichissement de notre compréhension de son œuvre, je développe deux exemples. Le premier est la découverte de trois longs essais, écrits en 1880, sur le problème des équations différentielles dans le domaine complexe, et publiés récemment par Scott Walter et moi-même par l'intermédiaire des ACERHP. Le second est la découverte, faite par June Barrow-Green, de la version originale de son mémoire couronné de 1888 sur la mécanique céleste. Chaque manuscrit met en lumière le cheminement de sa pensée vers certaines de ses découvertes les plus influentes.

Introduction

This paper is a version of a paper I presented at the ACERHP in February 1996 when the Centre was officially opened. It was an honour for me to be invited, and I took the occasion to describe some of the archival riches known to me, in anticipation of the greater riches which the Centre will discover. The documents I described have been well-known for some time, and therefore much of what I said at the meeting was familiar material. For that reason this paper is fairly short. I give two examples: the three *suppléments* Poincaré wrote to an essay for a prize competition of 1880, which document his path to the discovery of automorphic functions; and the original essay which won a prize competition in 1889, which permit us to document Poincaré's discovery of homoclinic points and chaos.

The competition of 1880

By good luck, and coincidentally in their centennial year, in 1980 I discovered three *suppléments* by Poincaré, which he appended to his essay for a prize competition of the *Académie des Sciences* in Paris. The *suppléments*, written in 1880, document his discovery of automorphic functions and of the important role non-Euclidean geometry can play in complex function theory. They are briefly described in Gray [1981; 1986] and have now been published under the auspices of the ACERHP in an edition prepared by Scott Walter and myself [Poincaré 1997]. They precede his published papers of 1881 on the subject, and they show in detail how he made and exploited a succession of insights into what was to become his first major contribution to mathematics.

The prize competition had been announced in 1878. The question set was "To improve in some important way the theory of linear differential equations in a single independent variable" («Perfectionner en quelque point important la théorie des équations différentielles linéaires à une seule variable indépendante»). It was announced with a closing date in 1880; the panel of judges comprised Bertrand, Bonnet, Puiseux, Bouquet with Hermite as rapporteur, It is quite likely that the topic, which was chosen by Hermite, was set with the work of his German friend L.I. Fuchs in mind, and indeed that he may well have thought would catch the interest of Poincaré, whom he knew well.

On 22 March 1880 Poincaré submitted a memoir on the real theory, which he withdrew on 14 June before the examiners could report on it. It would seem that his imagination had been captured by the very different complex case, which he wrote up and submitted on 28 May 1880. This essay was only to be published in the first volume of his Oeuvres, [1912, 578-613]. In it he considers special cases of the hypergeometric equation, a linear ordinary differential equation with 3 singular points, which may be taken to be the points $z = 0, 1$, and ∞. Let the functions w_1 and w_2 be two linearly independent solutions of the equation. Their quotient

$$\eta(z) := \frac{w_1(z)}{w_2(z)}$$

maps the upper and lower half-planes onto two circular-arc triangles, with vertex angles determined by the coefficients of the differential equation. If these are chosen suitably, the angles will be of the form π/k, for integers k (what are called 'aliquot' fractions of π). The triangles then fit together, say $2k$ at one vertex, $2m$ at the next vertex, and $2n$ at the third. Analytic continuation then produces a net of triangles, and Poincaré was able to show, by straightening out the sides of the triangles, that they never partially overlap but always fit together along edges, no matter how the analytic continuation is performed.

The next day he wrote the first of several letters to Fuchs (the letters are collected in *Oeuvres* 11, [1956, 13-25] but see also page 275 in the second pagination of volume 11 for a further letter from Fuchs to Poincaré). Shortly afterwards he had the first breakthrough into the topic of automorphic functions, and began work on the first of the three *suppléments*. It is, of course, this connection through Hermite to Fuchs, and Poincaré's patchy reading, that explains why

Poincaré was to call a large class of automorphic functions 'Fuchsian'. Poincaré proposed the name in a letter to Fuchs of 12 June 1880. The first *supplément* was then submitted to the Académie on 28 June 1880.

In this essay of 80 pages, Poincaré put forward a raft of new ideas. He now observed that the process of analytic continuation moved the triangles around 'crab-wise' by rotations through angles of the form $2\pi/k$. He called a rotation through $2\pi/m$ M and a rotation through $2\pi/n$ N, and observed that any position of a triangle could be reached from the starting position by a succession of motions of the form

$$M^{L_1}N^{K_1}M^{L_2}N^{K_2}\ldots$$

and that the collection of all these motions forms a group. Indeed, there is a precise connection to geometry, to be precise to non-Euclidean geometry. He now knew that the triangles filled out a disc, and he described the motions as non-Euclidean isometries. It is this idea of geometry that I wish to analyse, so I shall deal briefly first with the rest of the suppléments.

Poincaré considered the function which is defined on the net of triangles and which at a point P takes the value $\eta^{-1}(P)$. This is a complex function, whose properties depend on the nature of the triangular net, but it is clear that if there is an element of the group of motions, say g, and another point of the net, P' say, such that $gP' = P$, then $\eta^{-1}(P') = \eta^{-1}(P)$. So the function h is a generalisation of the doubly periodic or elliptic functions that were such a central part of 19th Century complex analysis. Poincaré called these new functions Fuchsian.

To solve a differential equation is to produce a function which is the solution. Poincaré did this by generalising the familiar theta functions that underpin the theory of elliptic functions. He wrote down power series that were not invariant under the action of the group of motions but changed in such a straight-forward way that quotients of them were — exactly mimicking the behaviour of the theta functions. He therefore called his power series theta-Fuchsian series. He showed that the power series converged by a clever juggling of estimates using both the Euclidean geometry and the non-Euclidean geometry of the disc. He had therefore constructed analytically the functions that had the right invariance properties to be the inverse of the quotient of the solutions of the original differential equation, and a simple formal argument showed that indeed they could be used to express the solutions themselves.

On September 6 1880 Poincaré sent a second *supplément* to the Académie, in which he described non-Euclidean geometry more carefully and extended his analysis to differential equations of the

Fuchsian type but with more than two singular points. On December 20 1880 he sent in a short third supplément, extending his analysis to triangles with vertices on the boundary of the disc, an important special case that includes Legendre's equation. Even so he did not win the prize, which went to a long essay by Halphen in differential equations and invariant theory.

In 1908 Poincaré addressed the *Société de Psychologie* in Paris on the subject of the psychology of mathematical invention (see his [1909]). The essay has become deservedly famous, and figures prominently in Hadamard's investigation of that subject. The inventions Poincaré chose to describe were precisely those involving his discovery of the Fuchsian and theta-Fuchsian functions. There are several famous incidents, and the *suppléments* permit us to date them rather precisely.

In the first of these, Poincaré was kept awake at night by a cup of coffee, which he had drunk contrary to his usual custom. After two weeks of vain attempts to prove that Fuchsian functions cannot exist, he related that

> Les idées surgissaient en foule ; je les sentais comme se heurter, jusqu'à ce que deux d'entre elles s'accrochassent, pour ainsi dire, pour former une combinaison stable. Le matin, j'avais établit l'existence d'une classe de fonctions fuchsiennes, celles qui dérivent de la série hypergéométrique.

He was then able to verify that the results thus suggested were correct in a matter of hours. A similar matter of conscious analogy led him to the theory of the theta-Fuchsian analogues of the familiar theta functions.

He then left Caen on a field trip, and changed buses at Coutances.

> Au moment où je mettais le pied sur le marche-pied, l'idée me vint, sans que rien dans mes pensées antérieures parût m'y avoir préparé, que les transformations dont j'avais usage pour définir les fonctions fuchsiennes étaient identiques à celles de la géométrie non-euclidienne.

The original circular-arc triangles had been straightened out in the writing of the memoir. Presumably Poincaré now realised that in that form their net filled out the disc with the Beltrami metric, in other words that they are triangles in two-dimensional non-Euclidean space. The original net of circular-arc triangles also filled out a disc, and in this version, as Poincaré saw, the angles of the triangles are represented correctly. The conformal quality of this net meant that he had obtained a picture of non-Euclidean space with more attractive metrical properties than the Beltrami description. To judge by the suppléments, this discovery came after the first supplément had been begun, and led to the eruption of enthusiasm on p. 129.

Finally, the generalisation of the triangular case to arbitrary polygons, which came while walking on the cliffs near Caen plainly comes between the first and second suppléments.

If this chronology is correct, and it agrees with the train of thought in the *suppléments*, it suggests that Poincaré first had the idea of generalising elliptic functions to Fuchsian functions and then discovered the role of non-Euclidean geometry. This requires that he appreciated the point of the triangles moving around crab-wise, and used that insight to write down the sum that replaces the usual representation of an elliptic function by a power series. To put the point clearly and briefly, but not rigorously, an elliptic function may be defined by a sum of the form

$$\frac{1}{z^2}+\sum\left((\frac{1}{z-w})^2-\frac{1}{w^2}\right)$$

where the sum is taken over every member w of the period lattice. Whatever else, this function is clearly doubly periodic. A Fuchsian function, by analogy, is defined by a sum of the form $\Sigma f(\gamma z)$, where the function f is suitably chosen and the summation is taken over every element of the corresponding Fuchsian group. Whatever else, this function is clearly automorphic with respect to that group. Convergence questions remain to be sorted out. Whether or not Poincaré solved that problem quickly, he certainly did when he realised that the motions of the triangles were rigid-body motions in non-Euclidean geometry.

It is worth quoting in full what Poincaré says about geometry in the first supplément. On p 129 he wrote

> Il existe des lines étroits entre les considérations qui précèdent et la géométrie non-euclidienne de Lobatchewski. Qu'est-ce en effet qu'une Géométrie ? C'est l'étude du *groupe d'opérations* formé par les déplacements que l'on peut faire subir à une figure sans le déformer.

The question naturally arises as to where Poincaré could have learned of this definition of geometry, and the question arises with some force because there seems to be a simple answer: Felix Klein's Erlangen Program [Klein 1872]. I argue that tracing it back to Klein is mistaken. It is unlikely on internal grounds (the detailed accounts of geometry are very different), on grounds of attribution (Poincaré never said that Klein was the source of these ideas) and it on grounds of availability (the Erlangen Program was only available as a booklet distributed from Erlangen). Let us take each of these points in turn.

Klein's Erlangen Program defines a geometry as a group acting on a space (to use somewhat more modern terms), and that isomorphic group actions give rise to equivalent geometries. Then it seeks to establish that most well-known geometries are special cases of projective geometry, and in particular that non-Euclidean geometry is a geometry whose space is the set of points inside a conic and whose group is the projective transformations mapping the interior of the conic to itself. In papers published at the time Klein showed in more detail how the projective invariant of cross-ratio (which involves four points) can be made to yield a two-point metrical invariant. In the Erlangen Program, however, the emphasis is strongly projective, and metrical geometry is not much discussed. But in Poincaré's work the emphasis is entirely metrical, and there is no suggestion of a hierarchy of geometries; indeed, Euclidean and non-Euclidean geometries are the only ones invoked. It is true that Poincaré first defines the non-Euclidean metric in the disc in a way that involves cross-ratio, but this arises from the fact that his group elements arose naturally as Möbius transformations. There is none of the richness of context that would indicate a direct influence.

Poincaré does not call his view of geometry the Kleinian one, and he was as scrupulous with attributions as his patchy reading and remarkable imagination would allow. The names he mentions are Beltrami and Houël. Only in 1881 does he say that Klein is of course the expert in non-Euclidean geometry, which is consistent with one of his better-read French colleagues having pointed this put to him. This remark is in a letter to Klein, which makes it all the more likely that Poincaré had only found out recently.

Finally, the Erlangen Program was only distributed at Erlangen on the occasion of Klein's appointment as a professor there in 1872. It was not the subject of his inaugural address. It is not cited in the literature of the 1870s, and it is even more unlikely that Poincaré, who was not a voracious reader, would have known of it. It did not become well-known until the early 1890s, when later developments, including Poincaré's own subsequent work and that of Sophus Lie made it seem prescient, and when Klein, as the editor of *Mathematische Annalen*, was able to orchestrate its re-distribution.

For all these reasons it is very unlikely that the Erlangen Program is the unacknowledged source of Poincaré's philosophy of geometry. That leaves us with the question of what, if anything, was. When I talked at the ACERHP, various suggestions were made

(Helmholtz, Houël, Darboux[1]) and it is now possible for me to reply properly to them. Because the Erlangen Program was deliberately programmatic, it is useful to look at it in a little more detail first in order to make our ideas more precise.

The role played by group theory in the Erlangen Program ends with the recognition (novel though it is) that an isomorphism of group actions is what is meant by an equivalence of geometries. Indeed, as Klein himself noted when the Program was reprinted, the very definition of a group that he gave in 1872 was inadequate. This is a clue to us to ask about the degree to which group theory enters various formulations of geometry. In the case of Helmholtz's papers, the answer is not at all. Helmholtz does discuss rigid-body motions as the source of our knowledge of geometry, but there is no notice taken of the fact that the motions of bodies may be thought of as the action of a group. The same is true of Beltrami's almost Euclidean talk of superposition. In Klein's case, the concepts of subgroup and isomorphism are brought in to the story. To go to the other extreme, in Lie's case, there is a much more profound analysis, yielding a classification theorem for at least the low-dimensional geometries.

I think it would be in the spirit of the Erlangen Program to describe a group action, indicate the appropriate invariants, establish an isomorphism. It is not in the spirit to fail to mention groups altogether. It goes beyond the spirit to investigate a group in any detail, or and well beyond to seek to analyse all of them. So when in 1880, in the still-unpublished *Suppléments* to his essay on linear differential equations, Poincaré simply says that a geometry is a group of operations formed by the displacements of a body that do not deform it, we can see various influences at work. The motion of rigid bodies is an idea vividly presented by both Helmholtz and Beltrami. Even Houël in his book on Euclidean geometry wrote in those terms. It is more metrical, and more narrow than Klein's.

What then? The sources available to Poincaré included not only work by Houël (a friend of Darboux) on Euclidean geometry [Houël 1863], but his translations of Beltrami's Saggio [Beltrami 1869] and Lobachevskii's Geometrische Untersuchungen [Lobachevskii 1866]. It is not certain that the work of Helmholtz was known to him, but nor is it clear that it would have added anything to what was readily available. With or without Helmholtz's papers, Poincaré could have known from his teachers that geometry is the

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1 The Darboux influence is of another kind: the maps Poincaré used to pass between the Beltrami description of non-Euclidean geometry and his own conformal one were already written up in Darboux's [1869], and Darboux was an early influence on Poincaré. There is no more to it than that.

study of figures in a space that can be moved around rigidly, so that exact superposition is possible and there is a notion of congruence. This idea, which is easier to think through in the metrical than the projective case, works for both Euclidean and non-Euclidean geometry. To anyone aware that thinking group-theoretically is advantageous, it was then natural to observe that the rigid-body motions form a group. This idea could have been had by Jordan, Darboux, Hermite, or Poincaré himself; it could even have been a common-place among the better French mathematicians of the 1870s. There is no need to attribute to the influence of Klein. Poincaré's subsequent work on automorphic functions has often been described (see [Gray 1986] and the references there). I shall turn instead to the other unpublished Poincaré documents known to me.

The competition of 1889

This archival material relates to his work on real differential equations, in particular those of celestial mechanics. Since they were discovered and described by my colleague June Barrow-Green (see her [1994]) they have been discovered again and described from a different stand-point by Andersen [1994]. The context is the prize competition of King Oscar II, the King of Sweden, which Poincaré won in 1889. This work led to his long-essay in *Acta Mathematica* [Poincaré 1890] and in due course to his three-volume *Mécanique Céleste*. This part of the story is also described by Goroff in his re-edition of the English translation of that work [Poincaré 1993]. For a full account of all this material and much more, based on thorough archival work see the book Barrow-Green [1996]. What concerns us here is the difference between the published paper of Poincaré's and the essay that actually won the prize.

The King of Sweden was advised throughout by Gösta Mittag-Leffler, a young, fairly rich, and very ambitious Swedish mathematician. He had founded a journal, Acta Mathematica, and assiduously cultivated mathematicians around Europe. He appointed Hermite and Weierstrass as judges of the competition, and they put together a list of four questions, of which the first was to solve the n-body problem by power series converging uniformly for any range of time. In fact, three of the problems connect to Poincaré, who had been working since 1883 on a new approach to the theory of differential equations ultimately aimed at the differential equations that apply to the study of the solar system. The announcement was published in several journals and in various languages in mid-1885. The closing date was 1 June 1888, and the winner would be announced on 21 January 1889, the King's 60th birthday.

In his prize essay, Poincaré alternated extensive general passages with pages devoted to the three body problem. Poincaré concentrated on the special case where one body, sometimes called J, has mass μ, another, S, has mass $1 - \mu$, and the third, E, mass 0. This describes a two body system and as such is completely understood, while the third body is attracted by the first two but is too small to affect them (the system consisting of Jupiter, the Sun, and the Earth is an example). What Poincaré christened the restricted three body problem is the further assumption that first two bodies travel in circles while the third body is constrained to lie in the plane of the first two.

For the restricted three body problem, Poincaré concentrated on a case which Hill, a distinguished American mathematical astronomer had recently shown to be realistic, when there is a periodic orbit with $\mu = 0$, and asked: what happens when μ becomes very small but positive? He showed that in general there would still be periodic orbits, and that some orbits were attracted to the periodic orbit ($\mu \neq 0$) while some moved away. To follow them, Poincaré took a small region of a plane, Π, transverse to the periodic orbit at a point P of it. Points of Π close to P flow round and meet Π again, giving a map of Π to itself. Poincaré showed that there are two (or, in a sense four) curves through P in Π with simple properties. Along two of these points move inwards; along the other two, outwards. They are the inward and outward curves of the opening example. To investigate the global shape of these curves Poincaré first obtained a formal power series solution, then showed that the power series converges. What he wanted was a power series whose coefficients depend in a manageable way on the parameter μ (in particular, they remain convergent as μ varies). For a technical reason Poincaré first worked to first order in $\sqrt{\mu}$ and showed that in the special case of the restricted three body problem the outward curve turns into the inward one. Then he worked to arbitrary but finite order in $\sqrt{\mu}$, and he showed that the curve just perturbs slightly. Finally he allowed the dependence on $\sqrt{\mu}$ to be given by power series in $\sqrt{\mu}$, and showed that the series still converge.

First Poincaré submitted a paper of some 150 pages. Then he produced 100 pages of additions. All in all, his manuscript was of extraordinary originality. It may not have solved the actual problem, but it created a wealth of new ideas that could be pursued. But, as so often with Poincaré the paper was difficult. It was not always clear what was true, or what was proved. Even so, the jury was unanimous that his essay deserved the prize, and the printers of Acta Mathematica were instructed to set the manuscript into type. The job of seeing the manuscript through the press was entrusted to a junior editor of Acta, Phragmén. Since it was a very long paper, a dummy run was done (there is a bound copy in the Mittag-Leffler Institute) and then 32-page runs were made. Halfway through, Phragmén came to something he did not understand. Neither, it emerged, did Mittag-Leffler, so in July they wrote to Poincaré.

Only in December did a reply come, but it was one that must have made Mittag-Leffler's heart sink. Poincaré confirmed that there was a mistake in the paper. Not, it would seem, the one Phragmén had spotted, but a much more serious one. Mittag-Leffler immediately ordered all the printed copies of the paper withdrawn - some half-dozen had gone out to editors of Acta. June Barrow-Green found written inside one surviving copy a note in Swedish saying "All other copies destroyed". He ordered the printing of Acta to stop until a correct version was written, and charged Poincaré with the bill (it came to more than his original prize money).

Poincaré's argument that his power series converged as a power series in $\sqrt{\mu}$ was false. He had hardly thought about it, and was probably driven by his pictures to conclude convergence (for a detailed discussion see [Barrow-Green 1994; 1996]). Phragmén's questions may have reminded Poincaré that a series might represent a function asymptotically, and between July and December Poincaré not only showed by an example that this could happen, but that when it did, the inward and outward curves no longer joined up but crossed. But, he showed, if they cross once they must cross infinitely often, so the picture is appalling to draw. As Poincaré wrote (*Mécanique Céleste* 3, 389),

> One is struck by the complexity of this figure, which I shall not even attempt to draw. Nothing gives a better idea of the complication of the three body problem and in general all the problems of dynamics where there is no single-valued solution and the [usual] series are divergent.

This discovery of the homoclinic tangle, as it became called after Poincaré called the crossing point a homoclinic point, caused many pages of alterations to the original paper (see [Barrow-Green 1996] for a full description). The usual method for tackling the three body problem was now known to be suspect. One cannot solve a simple problem (the one with $\mu = 0$) and then readily deduce results about a 'nearby' problem (with $\mu > 0$ but small). The dependence on the parameter is really delicate, which goes a long way to explain why Poincaré could only substantiate his general claims in a very special case. It is oddly comforting to see even a great mathematician go into print with a mistake, a mistake moreover that has been past the best trained eyes of the mathematical profession. And it is even more satisfying to see such wonderful mathematics reward the person who made the mistake simply when he realises it and tries to put it right.

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