Poincaré Replies to Hilbert: On the Future of Mathematics ca. 1908

JEREMY GRAY

Years Ago features essays by historians and mathematicians that take us back in time. Whether addressing special topics or general trends, individual mathematicians or "schools" (as in schools of fish), the idea is always the same: to shed new light on the mathematics of the past. Submissions are welcome.

➢ Send submissions to **David E. Rowe,**
Fachbereich 08, Institut für Mathematik,
Johannes Gutenberg University,
D-55099 Mainz, Germany.
e-mail: rowe@mathematik.uni-mainz.de

In April 1908, Henri Poincaré boarded a train for Rome, where he planned to deliver a plenary lecture at the International Congress of Mathematicians (ICM) on the present state of mathematics and its prospects for the future. This was his first opportunity to reply to David Hilbert's address, delivered at the ICM in Paris in 1900 (Hilbert 1900), in which Hilbert had presented a by now famous list of problems for the new century. Poincaré could not have replied at the intervening ICM held in Heidelberg in 1904, because he had chosen to go to the International Congress of Arts and Sciences in St. Louis instead, where he spoke on the past, present, and future of physics. However, by 1908 he knew that Hilbert's problems were beginning to draw attention, and like many French mathematicians, he was well aware that the Germans did things differently – and rather well. Hilbert was by now attracting many talented students to Göttingen, including several foreigners. Poincaré, as the universally recognized leader of the French mathematical community, clearly felt prompted to respond to Hilbert's challenge.

Personal illness intervened, however, preventing Poincaré from presenting his lecture at the Rome Congress. Instead, the paper was presented on his behalf by Gaston Darboux, a long-standing supporter. Poincaré was able to keep his promise to the readers of *Le Temps* by writing a report on the Congress after talking to other guests in his hotel,[1] but the illness was, in fact, serious and was still troubling Poincaré a year later when he went to Göttingen to deliver the first series of Wolfskehl lectures he too, was not in the best of health at this time. Since he was recovering from a bout of depression, he chose not to attend the ICM in Rome. However, his friend Hermann Minkowski was present and passed along news to him about what had transpired. Remarking on the text that Darboux read, Minkowski called it an "entirely weak remake of your Paris lecture" ("ein ganz schwählicher Abklatsch Deines Pariser Vortrages") (Minkowski to Hilbert, 4 May 1908).

Poincaré's paper at the Rome ICM, "L'avenir des Mathématiques," was promptly published in the *Revue générale des sciences pures et appliquées* and then in the *Atti del IV congresso internazionale dei matematici*, 1909. As was Poincaré's practice, it was afterward published in a modified form in a book of essays, *Science et Méthode*, from which two English translations were made, one in the American journal the *Monist*, volume 20, 1910, pages 76–92, and another in the English edition of the book: *Science and Method*. What is not apparent from these latter publications is that they differ substantially from the original in that almost all the detailed mathematical comments have been removed; whole sections have, in fact, been deleted. Because these books are

[1] *Le Temps*, 21 April 1908, nr. 17102.

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DOI 10.1007/s00283-012-9299-7

usually the only sources cited today, this has contributed to a misleading picture of Poincaré's famous lecture. The purpose of this essay is to restore the fuller picture of what he had to say about the future of mathematics in 1908 by first sketching the main themes that surface in Poincaré's lecture, followed by a presentation of the entire text in a new English translation. At the same time, I will suggest the reason that this speech can be seen as Poincaré's reply to Hilbert's even more famous lecture, "Mathematische Probleme," delivered eight years earlier in Paris.

The first people to realise the existence of the two very different texts in recent times seem to have been Philip J. Davis and David Mumford in their article of 2008 entitled "Henri's crystal ball", which used the translation of the full text available at http://portail.mathdoc.fr/BIBLIOS/PDF/Poincare.pdf. Their article takes what might be called a telescopic view, looking at developments after 1908 with a view toward seeing what Poincaré got right and what he missed. This article aims instead at gaining a closer understanding of how he saw the future of mathematics in the year 1908. It thus takes what might be called a microscopic view of the full text and its context.

The quality of the Portail translation, which seems to be anonymous, leaves much to be desired. Far from capturing Poincaré's elegant prose, it is written in garbled, substandard English, as is clear from its very beginning:

> The true method of forecasting the future of mathematics lies in the study of its history and its present state. And have we not here for us mathematicians, a task in some sort professional? We are accustomed to extrapolation, that process which serves to deduce the future from the past and the present and I 'so well know its limitations that we run no risk of being deluded with its forecasts.

The well-known but much abbreviated translation is at least free of a strong hint of Hercule Poirot, but in view of the defects of both translations, it seems worthwhile to publish a fresh and complete translation. What follows makes only a small number of changes to the familiar English version and restores the missing passages. A commentary on the text follows the translation.



JEREMY GRAY is a Professor of the History of Mathematics at the Open University, and is an Honorary Professor at the University of Warwick, where he lectures on the history of mathematics. His most recent book is *Plato's Ghost: The Modernist Transformation of Mathematics*, Princeton University Press (2008), and he is presently finishing a scientific biography of Henri Poincaré.

Faculty of Mathematics, Computing and Technology
The Open University
Walton Hall, Milton Keynes MK7 6AA
UK
e-mail: j.j.gray@open.ac.uk

The Future of Mathematics by Henri Poincaré

[I thank Claude Baesens for help with some passages in the new translations; any mistakes in them are, however, mine.]

If we wish to foresee the future of mathematics, our proper course is to study the history and present condition of the science.

For us mathematicians, is not this procedure to some extent professional? We are accustomed to extrapolation, which is a method of deducing the future from the past and the present; and since we are well aware of its limitations, we run no risk of deluding ourselves as to the scope of the results it gives us.

In the past there have been prophets of ill. They took pleasure in repeating that all problems susceptible of being solved had already been solved, and that after them there would be nothing left but gleanings. Happily we are reassured by the example of the past. Many times already men have thought that they had solved all the problems, or at least that they had made an inventory of all that admit of solution. And then the meaning of the word solution has been extended; the insoluble problems became the most interesting of all, and other problems *were proposed that one had not dreamt of*.[2] For the Greeks, a good solution was one that employed only ruler and compass; later it became one obtained by the extraction of radicals, then one in which only *algebraic functions and logarithms appeared*. Thus the pessimists found themselves continually passed over, continually forced to retreat, so that at present I really believe there are none left.

My intention, therefore, is not to refute them, since they are dead. We know very well that mathematics will continue to develop, but we have to find out in what direction. I shall be told "in all directions", and that is partly true; but if it were altogether true, it would become somewhat alarming. Our riches would soon become embarrassing, and their accumulation would soon produce a mass just as impenetrable as the unknown truth was to the ignorant.

The historian, the physicist himself, must make a selection of facts. The scientist's brain, which is only a corner of the universe, will never be able to contain the whole universe; whence it follows that, of the innumerable facts offered by nature, we shall leave some aside and retain others. The same is true, a fortiori, in mathematics. The mathematician similarly cannot retain pell-mell all the facts that are presented to him, the more so in that it is he himself – I was going to say his own caprice – that creates *them*. It is he who constructs a new combination of the pieces by assembling the elements and from top to bottom; it is generally not brought to him ready-made by nature.

No doubt it is sometimes the case that a mathematician attacks a problem to satisfy some requirement of physics, that the physicist or the engineer asks him to make a calculation in view of some particular application. Will it be said that we

[2]Text amended or inserted in this translation is shown in italics.

geometricians are to confine ourselves to waiting for orders, and, instead of cultivating our science for our own pleasure, to have no other care but that of accommodating ourselves to our clients' tastes? If the only object of mathematics is to come to the help of those who make a study of nature, it is to them we must look for the word of command. Is this the correct view of the matter? Certainly not; for if we had not cultivated the exact sciences for themselves, we should never have created the mathematical instrument, and when the word of command came from the physicist we should have been found without arms.

Similarly, physicists do not wait to study a phenomenon until some pressing need of material life makes it an absolute necessity, and they are quite right. If the scientists of the eighteenth century had disregarded electricity, because it appeared to them merely a curiosity having no practical interest, we should not have, in the twentieth century, either telegraphy or electro-chemistry or electro-traction. Physicists forced to select are not guided in their selection solely by utility. What method, then, do they pursue in making a selection between the different natural facts? [I have explained this in the preceding chapter.][3] *We can reply easily:* the facts that interest them are those that may lead to the discovery of a law, those that have an analogy with many other facts and do not appear to us as isolated, but as closely grouped with others. The isolated fact attracts the attention of all, of the layman as well as the scientist. But what the true scientist alone can see is the link that unites several facts which have a deep but hidden analogy. The anecdote of Newton's apple is probably not true, but it is symbolical, so we will treat it as if it were true. Well, we must suppose that before Newton's day many men had seen apples fall, but none had been able to draw any conclusion. Facts would be barren if there were not minds capable of selecting between them and distinguishing those which have something hidden behind them and recognizing what is hidden – minds which, behind the bare fact, can detect the soul of the fact.

In mathematics we do exactly the same thing. Of the various elements at our disposal we can form millions of different combinations, but any one of these combinations, so long as it is isolated, is absolutely without value; often we have taken great trouble to construct it, but it is of absolutely no use, unless it be, perhaps, to supply a subject for an exercise in secondary schools. It will be quite different as soon as this combination takes its place in a class of analogous combinations whose analogy we have recognized; we shall then be no longer in the presence of a fact, but of a law. And then the true discoverer will not be the workman who has patiently built up some of these combinations, but the man who has brought out their relation. The former has only seen the bare fact, the latter alone has detected the soul of the fact. The invention of a new word will often be sufficient to bring out the relation, and the word will be creative. The history of science furnishes us with a host of examples that are familiar to all.

The celebrated Viennese philosopher Mach has said that the *role* of science is to effect economy of thought, just as a machine effects economy of effort, and this is very true. The savage calculates on his fingers, or by putting together pebbles. By teaching children the multiplication table we save them later on countless operations with pebbles. Someone once recognized, whether by pebbles or otherwise, that 6 times 7 are 42, and had the idea of recording the result, and that is the reason why we do not need to repeat the operation. His time was not wasted even if he was only calculating for his own amusement. His operation only took him two minutes, but it would have taken two million, if a million people had had to repeat it after him.

Thus the importance of a fact is measured by the return it gives – that is, by the amount of thought it enables us to economise.

In physics, the facts that give a large return are those that take their place in a very general law, because they enable us to foresee a very large number of others, and it is exactly the same in mathematics. Suppose I apply myself to a complicated calculation and with much difficulty arrive at a result, I shall have gained nothing by my trouble if it has not enabled me to foresee the results of other analogous calculations, and to direct them with certainty, avoiding the blind groping with which I had to be contented the first time. On the contrary, my time will not have been lost if this very groping has succeeded in revealing to me the profound analogy between the problem just dealt with and a much more extensive class of other problems; if it has shown me at once their resemblances and their differences; if, in a word, it has enabled me to perceive the possibility of a generalization, then it will not be merely a new result that I have acquired, but a new force.

An algebraic formula that gives us the solution of a type of numerical problem, if we finally replace the letters by numbers, is the simple example that occurs to one's mind at once. Thanks to the formula, a single algebraic calculation saves us the trouble of a constant repetition of numerical calculations. But this is only a rough example: everyone feels that there are analogies that cannot be expressed by a formula, and that they are the most valuable.

If a new result is to have any value, it must unite elements long since known, but till then scattered and seemingly foreign to each other, and suddenly introduce order where the appearance of disorder reigned. Then it enables us to see at a glance each of these elements in the place it occupies in the whole. Not only is the new fact valuable on its own account, but it alone gives a value to the old facts it unites. Our mind is as frail as our senses are; it would lose itself in the complexity of the world if that complexity were not harmonious; like the short-sighted, it would only see the details, and would be obliged to forget each of these details before examining the next, because it would be incapable of taking in the whole. The only facts worthy of our attention are those that introduce order into this complexity and so make it accessible to us.

Mathematicians attach a great importance to the elegance of their methods and of their results, and this is not mere dilettantism. What is it that gives us the feeling of

[3]Sentences such as this one which were added in the book versions are set apart in brackets.

elegance in a solution or a demonstration? It is the harmony of the different parts, their symmetry, and their happy adjustment; it is, in a word, all that introduces order, all that gives them unity, that enables us to obtain a clear comprehension of the whole as well as of the parts. But that is also precisely what causes it to give a large return; and in fact the more we see this whole clearly and at a single glance, the better we shall perceive the analogies with other neighbouring objects, and consequently the better chance we shall have of guessing the possible generalizations. Elegance may result from the feeling of surprise caused by the unlooked-for occurrence together of objects not habitually associated. In this, again, it is fruitful, since it thus discloses relations until then unrecognized. It is also fruitful even when it only results from the contrast between the simplicity of the means and the complexity of the problem presented, for it then causes us to reflect on the reason for this contrast, and generally shows us that this reason is not chance, but is to be found in some unsuspected law. Briefly stated, the sentiment of mathematical elegance is nothing but the satisfaction due to some conformity between the solution we wish to discover and the necessities of our mind, and it is on account of this very conformity that the solution can be an instrument for us. This aesthetic satisfaction is consequently connected with the economy of thought. *It is in this way that the caryatids of the Erechtheum, for example, seem elegant to us because they bear a heavy load with suppleness and, one might say say, nimbly, and so they give us the feeling of economy of effort.* [Again the comparison with the Erechtheum occurs to me, but I do not wish to serve it up too often.]

It is for the same reason that, when a somewhat lengthy calculation has conducted us to some simple and striking result, we are not satisfied until we have shown that we might have foreseen, if not the whole result, at least its most characteristic features. Why is this? What is it that prevents our being contented with a calculation that has taught us apparently all that we wished to know? The reason is that, in analogous cases, the lengthy calculation might not be able to be used again, whereas this is not true of the reasoning, often semi-intuitive, which might have enabled us to foresee the result. This reasoning being short, we can see all the parts at a single glance, so that we perceive immediately what must be changed to adapt it to all the problems of a similar nature that may be presented. And since it enables us to foresee whether the solution of these problems will be simple, it shows us at least whether the calculation is worth undertaking.

What I have just said is sufficient to show how vain it would be to attempt to replace the mathematician's free initiative by a mechanical process of any kind. In order to obtain a result having any real value, it is not enough to grind out calculations, or to have a machine for putting things in order: it is not order only, but unexpected order, that has a value. A machine can take hold of the bare fact, but the soul of the fact will always escape it.

Since the middle of the last century, mathematicians have become more and more anxious to attain to absolute exactness. They are quite right, and this tendency will become more and more marked. In mathematics, exactness is not everything, but without it there is nothing: a demonstration that lacks exactness is nothing at all. This is a truth that I think no one will dispute, but if it is taken too literally it leads us to the conclusion that before 1820, for instance, there was no such thing as mathematics, and this is clearly an exaggeration. The geometricians of that day were willing to assume what we explain by prolix dissertations. This does not mean that they did not see it at all, but they passed it over too hastily, and, in order to see it clearly, they would have had to take the trouble to state it.

Only, is it always necessary to state it so many times? Those who were the first to pay special attention to exactness have given us reasonings that we may attempt to imitate; but if the demonstrations of the future are to be constructed on this model, mathematical works will become exceedingly long, and if I dread length, it is not only because I am afraid of the congestion of our libraries, but because I fear that as they grow in length, our demonstrations will lose that appearance of harmony that plays such a useful part, as I have just explained.

It is economy of thought that we should aim at, and therefore it is not sufficient to give models to be copied. We must enable those that come after us to do without the models, and not to repeat a previous reasoning, but summarize it in a few lines. And this has already been done successfully in certain cases. For instance, there was a whole class of reasonings that resembled each other, and were found everywhere; they were perfectly exact, but they were long. One day someone thought of the term "uniformity of convergence", and this term alone made them useless; it was no longer necessary to repeat them, since they could now be assumed. Thus the hair-splitters can render us a double service, first by teaching us to do as they do if necessary, but more especially, by enabling us as often as possible not to do as they do, and yet make no sacrifice of exactness.

One example has just shown us the importance of terms in mathematics; but I could quote many others. It is hardly possible to believe what economy of thought, as Mach used to say, can be effected by a well-chosen term. I think I have already said somewhere that mathematics is the art of giving the same name to different things. It is enough that these things, though differing in matter, should be similar in form, to permit of their being, so to speak, run in the same mould. When language has been well chosen, one is astonished to find that all demonstrations made for a known object apply immediately to many new objects: nothing is required to be changed, not even the terms, since the names have become the same.

One example presents itself to the mind before all others, and that is quaternions, upon which I need not insist. A well-chosen term is very often sufficient to remove the exceptions permitted by the rules as stated in the old phraseology. This accounts for the invention of negative quantities, imaginary quantities, decimals to infinity, and I know not what else. And we must never forget that exceptions are pernicious, because they conceal laws.

This is one of the characteristics by which we recognize facts that give a great return: they are the facts that permit these happy innovations of language. The bare fact, then, has sometimes no great interest: it may have been noted many times without rendering any great service to science;

it only acquires a value when some more careful thinker perceives the connection it brings out, and symbolizes it by a term.

The physicists also proceed in exactly the same way. They have invented the term "energy", and the term has been enormously fruitful, because it also creates a law by eliminating exceptions; because it gives the same name to things that differ in matter, but are similar in form.

Among the terms that have exercised the most happy influence I would note "group" and *"invariant"*. They have enabled us to perceive the essence of many mathematical reasonings, and have shown us in how many cases the old mathematicians were dealing with groups without knowing it, and how, believing themselves far removed from each other, they suddenly found themselves close together without understanding why.

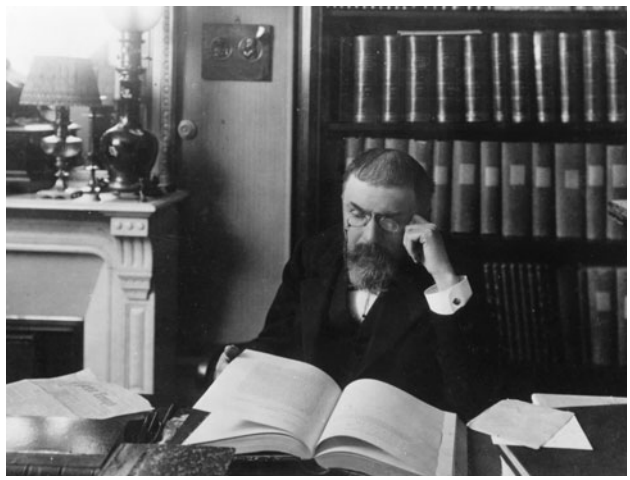
Today we should say that they had been examining isomorphic groups. We now know that, in a group, the matter is of little interest, that the form only is of importance, and that when we are well acquainted with one group, we know by that very fact all the isomorphic groups. Thanks to the terms "group" and "isomorphism", which sum up this subtle rule in a few syllables, and make it readily familiar to all minds, the passage is immediate, and can be made without expending any effort of thinking. The idea of group is, moreover, connected with that of transformation. Why do we attach so much value to the discovery of a new transformation? It is because, from a single theorem, it enables us to draw ten or twenty others. It has the same value as a zero added to the right of a whole number.

This is what has determined the direction of the movement of mathematical science up to the present, and it is also most certainly what will determine it in the future. But the nature of the problems that present themselves contribute to it in an equal degree. We cannot forget what our aim should be, and in my opinion this aim is a double one. Our science borders on both philosophy and physics, and it is for these two neighbours that we must work. And so we have always seen, and we shall still see, mathematicians advancing in two opposite directions.

On the one side, mathematical science must reflect upon itself, and this is useful because reflecting upon itself is reflecting upon the human mind that has created it; the more so because, of all its creations, mathematics is the one for which it has borrowed least from outside. This is the reason for the utility of certain mathematical speculations, such as those that have in view the study of postulates, of unusual geometries, of functions with strange behaviour. The more these speculations depart from the most ordinary conceptions, and, consequently, from nature and applications to natural problems, the better will they show us what the human mind can do when it is more and more withdrawn from the tyranny of the exterior world; the better, consequently, will they enable us to know this mind itself.

But it is to the opposite side, to the side of nature, that we must direct our main forces.

There we meet the physicist or the engineer, who says, "Will you integrate this differential equation for me?; I shall need it within a week for a piece of construction work that has to be completed by a certain date". "This equation", we



Jules Henri Poincaré (1854 - 1912). AIP Emilio Segre Visual Archives.

answer, "is not included in one of the types that can be integrated, of which you know there are not very many." "Yes, I know; but, then, what good are you?" More often than not, a mutual understanding is sufficient. The engineer does not really require the integral in finite terms, he only requires knowledge of the general behaviour of the integral function, or he merely wants a certain figure that would be easily deduced from this integral if we knew it. Ordinarily we do not know it, but we could calculate the figure without it if we knew just what figure and what degree of exactness the engineer required.

Formerly an equation was not considered to have been solved until the solution had been expressed by means of a finite number of known functions. But this is impossible in about ninety-nine cases out of a hundred. What we can always do, or rather what we should always try to do, is to solve the problem qualitatively so to speak – that is, to try to know approximately the general form of the curve that represents the unknown function.

It then remains to find the *quantitative* solution of the problem. But if the unknown cannot be determined by a finite calculation, we can always represent it by an infinite converging series that enables us to calculate it. Can this be regarded as a true solution? The story goes that Newton once communicated to Leibnitz an anagram somewhat like the following: *aaaaabbbeeeeii*, etc. Naturally, Leibnitz did not understand it at all, but we who have the key know that the anagram, translated into modern phraseology, means, "I know how to integrate all differential equations" and we are tempted to make the comment that Newton was either exceedingly fortunate or that he had very singular illusions. What he meant to say was simply that he could form (by means of indeterminate coefficients) a series of powers formally satisfying the equation presented.

Today a similar solution would no longer satisfy us for two reasons – because the convergence is too slow, and because the terms succeed one another without obeying any law. On the other hand, the Θ series appears to us to leave nothing to be desired, first, because it converges very rapidly (this is for the practical man who wants his number

as quickly as possible), and second, because we perceive at a glance the law of the terms, which satisfies the aesthetic requirements of the theorist.

There are, therefore, no longer some problems solved and others unsolved, there are only problems more or less solved, according as this is accomplished by a series of more or less rapid convergence or regulated by a more or less harmonious law. Nevertheless an imperfect solution may happen to lead us toward a better one.

Sometimes the series is of such slow convergence that the calculation is impracticable, and we have only succeeded in demonstrating the possibility of the problem. The engineer considers this absurd, and he is right, since it will not help him to complete his construction within the time allowed. He doesn't trouble himself with the question of whether it will be of use to the engineers of the twenty-second century. We think differently, and we are sometimes more pleased at having economized a day's work for our grandchildren than an hour for our contemporaries.

Sometimes by groping, so to speak, empirically, we arrive at a formula that is sufficiently convergent. "What more would you have?" says the engineer; and yet, in spite of everything, we are not satisfied, for we should have liked to be able to predict the convergence. And why? Because if we had known how to predict it in the one case, we should know how to predict it in another. We have been successful, it is true, but that is little in our eyes if we have no real hope of repeating our success.

In proportion as the science develops, it becomes more difficult to take it in its entirety. Then an attempt is made to cut it in pieces and to be satisfied with one of these pieces – in a word, to specialize. Too great a movement in this direction would constitute a serious obstacle to the progress of science. As I have said, it is by unexpected concurrences between its different parts that it can make progress. Too much specializing would prohibit these concurrences. Let us hope that congresses, such as those of Heidelberg and Rome, by putting us in touch with each other, will open up a view of our neighbours' territory and force us to compare it with our own, and so escape in a measure from our own little village. In this way they will be the best remedy against the danger I have just noted.

But I have delayed too long over generalities; it is time to enter into details.

Let us review the different particular sciences that go to make up mathematics; let us see what each of them has done, in what direction it is tending, and what we may expect of it. If the preceding views are correct, we should see that the great progress of the past has been made when two of these sciences have been brought into conjunction, when men have become aware of the similarity of their form in spite of the dissimilarity of their matter, when they have modelled themselves upon each other in such a way that each could profit by the triumphs of the other. At the same time we should look to concurrences of a similar nature for progress in the future.

I. Arithmetic

The progress of arithmetic has been much slower than that of algebra and analysis, and it is easy to understand the reason. The feeling of continuity is a precious guide that fails the arithmetician. Every whole number is separated from the rest, and has, so to speak, its own individuality; each of them is a sort of exception, and that is the reason why general theorems will always be less common in the theory of numbers, and also why those that do exist will be more hidden and will escape detection longer.

If arithmetic is backward as compared with algebra and analysis, the best thing for it to do is to try to model itself on these sciences, in order to profit by their advance. The arithmetician then should be guided by the analogies with algebra. These analogies are numerous, and if in many cases they have not yet been studied sufficiently closely to become serviceable, they have at least been long foreshadowed, and the very language of the two sciences shows that they have been perceived. Thus we speak of transcendental numbers, and so become aware of the fact that the future classification of these numbers has already a model in the classification of transcendental functions. However, it is not yet very clear how we are to pass from one classification to the other; but if it were clear, it would be already done and would no longer be the work of the future.

The first example that comes to mind is the theory of *congruences*, in which we find a perfect parallelism with that of algebraic equations. We shall certainly succeed in completing this parallelism, which must exist, for instance, between the theory of algebraic curves and that of *congruences* with two variables. When the problems relating to *congruences* with several variables have been solved, we shall have made the first step toward the solution of many questions of indeterminate analysis.

Another example where, however, the analogy was only seen later, is provided for us by the theory of fields and ideals. To have a counterpart to this, consider curves traced on a surface: to existing numbers correspond complete intersections, to ideals incomplete intersections, to prime ideals indecomposable curves; the various classes of ideals also have their analogues.

There is no doubt that this analogy can explain the theory of ideals, or that of surfaces, or perhaps both at once.

The theory of forms, in particular that of quadratic forms, is intimately linked to that of ideals. If, among arithmetical theories, it has been one of the first to take shape, it was when one was led to introduce unity into it by considering groups of linear transformations.

These transformations have allowed a classification and, consequently, the introduction of order. Perhaps one has harvested all the fruit one can hope to; but, if these transformations are the parents of perspectives in geometry, analytical geometry provides us with many other transformations (as, for example, the birational transformations of an algebraic curve) of which it will be advantageous to seek the arithmetical analogues. Without any doubt they will form discontinuous groups, of which one must first seek the fundamental domain, which is the key to everything. In this study, I do not doubt that one will have to make use of Minkowski's Geometrie der Zahlen.

One idea from which we have not drawn all that it contains is Hermite's introduction of continuous variables into the theory of numbers. We know now what it signifies.

Let us take as our point of departure two forms F and F', the second quadratic definite, and apply the same transformation to them. If the transform of F' is reduced, we say that the transformation is reduced and also that the transform of F is reduced. It follows that if the form F can be transformed into itself, it can have several reduced forms; but this inconvenience is essential and cannot be avoided in any way; and besides it does not prevent these reduced forms from leading to a classification of forms. It is clear that this idea, which up to now has only been applied to very particular forms and transformations, can be extended to nonlinear groups of transformations, because it is of much greater import and has not been exhausted.

One domain of arithmetic in which unity seems to be entirely lacking is the theory of prime numbers; we have only found asymptotic laws, and we must hope for others; but these laws are isolated and one only comes upon them by different routes that do not seem to communicate with each other. I think I have glimpsed where one might draw the desired unity, but I have only glimpsed it vaguely; everything will come down without doubt to a family of transcendental functions that will permit, by the study of their singular points and an application of the method of Darboux, calculating certain functions of very large numbers asymptotically.

II. Algebra

The theory of algebraic equations will long continue to attract the attention of geometricians, because the sides by which the theory may be approached are so numerous and so different. *The most important is the theory of groups, to which we shall return. But there is also the question of the numerical calculation of the roots and that of the discussion of the number of real roots. Laguerre has shown that not everything was said on this point by Sturm. There is a place for studying a system of invariants that do not change sign when the number of real roots remains the same. One can also form power series representing functions that have as their singular points the various roots of an algebraic equation (for example, rational functions whose denominator is the first term of that equation); the coefficients of the higher-order terms provide us with an approximation to a greater or lesser degree of accuracy; there is the germ of a procedure there for numerical calculation that one could study systematically.*

Some forty years ago it was the study of the invariants of algebraic forms that seemed to absorb all of algebra; today it is abandoned, the matter, however, is not exhausted; only it is necessary to extend it and not restrict oneself, for example, to the invariants of linear transformations but to tackle those that belong to an arbitrary group. The theorems acquired formerly will thus suggest to us other more general ones that will group themselves around them like a crystal feeding itself in a solution. And as for Gordan's theorem that the number of distinct invariants is bounded, and which Hilbert has so happily simplified the proof of, it seems that this theorem leads us to ask a much more general question: if one has an infinity of polynomials that depend algebraically on a finite number of them, can one always derive them from a finite number of them by addition and multiplication?

It must not be supposed that algebra is finished because it furnishes rules for forming all possible combinations; finding interesting combinations still remains, that is, those that satisfy such-and-such conditions. Thus there will be built up a kind of indeterminate analysis, in which the unknown quantities will no longer be whole numbers but rather polynomials. So this time it is algebra that will model itself on arithmetic, being guided by the analogy of the whole number, either with the whole polynomial with indefinite coefficients, or with the whole polynomial with whole coefficients.

III. Differential Equations

We have already done a lot with linear differential equations, and it only remains to perfect what we have done. But as for what concerns nonlinear differential equations, we are much less advanced. The hope of integrating them by means of known functions was lost a long time ago; it is therefore necessary to study for themselves the functions defined by these differential equations and to attempt first a systematic classification of these functions; the study of the way they grow in the neighbourhood of a singular point will without doubt furnish the first elements of this classification, but we will only be satisfied when we have found a certain group of transformations (for example, the Cremona transformations), which plays with respect to differential equations, the same role that the group of birational transformations plays for algebraic curves. We will then be able to place in the same class all the transforms of the same equation. We will then have as our guide a theory already constructed: that of the birational transformations and of the genus of an algebraic curve.

One can propose to reduce the study of these functions to that of single-valued functions, and that in two ways: we know that if $y = f(x)$, one can, whatever the function $f(x)$ is, write y and x in terms of single-valued functions of an auxiliary variable t; but, if $f(x)$ is the solution of a differential equation, in what case do the auxiliary functions themselves satisfy a differential equation? We do not know; we do not even know in what case the general integral can be put in the form $F(x, y) =$ an arbitrary constant, $F(x, y)$ being single-valued.

I will insist on the qualitative discussion of curves defined by differential equations. In the simplest case, when the equation is of the first order and the first degree, this discussion leads to the determination of the number of limit cycles. It is very delicate, and what can enable us to do it is the analogy with the study of the number of real roots of an algebraic equation; when a fact makes the nature of this analogy evident, we can be certain in advance that it will be fertile.

IV. Partial Differential Equations

We have recently made considerable progress in our knowledge of partial differential equations, following the discoveries of M. Fredholm. Now, if one examines the essence of these discoveries closely, one sees that they consist in modelling a difficult theory on another much simpler one: that of determinants and systems of the first degree. In most of the problems in mathematical physics, the equations to be integrated are linear; they allow us to determine unknown functions of several variables, and those functions are continuous. Why?

Because we have written those equations while regarding matter as continuous. But matter is not continuous; it is made of atoms, and, if we were to have wanted to write the equations as they would be written by an observer sufficiently perceptive to see the atoms, we would not have a small number of differential equations serving to determine certain unknown functions, we would have a great number of algebraic equations serving to determine a great number of unknown constants. And these algebraic equations would be linear, so that one could, with infinite patience, directly apply the method of determinants.

But, as the shortness of our lives does not permit us the luxury of infinite patience, it is necessary to proceed differently: it is necessary to pass to the limit by supposing matter to be continuous. There are two ways of generalising the theory of first-degree equations by passing to the limit. One can consider an infinite number of discrete equations with another infinity, equally discrete, of unknowns. This, for example, is what Hill did in his theory of the Moon. One then has infinite determinants, which are to ordinary determinants as series are to finite sums.

One can take a partial differential equation, representing, so to speak, a continuous infinity of equations, and use it to determine an unknown function representing a continuous infinity of unknowns. One then has other infinite determinants, which are to ordinary determinants what integrals are to finite sums. That is what Fredholm did; his success also leads to the following fact: if in a determinant the elements on the principal diagonal are equal to 1, *and the other elements are considered as homogeneous of the first order, one can arrange the development of the determinant by collecting in a single group the terms that are homogeneous of the same degree. Fredholm's infinite determinant can be expanded in this fashion, and it happens that one obtains in this way a convergent series.*

Has this analogy, which certainly guided Fredholm, given all that it can give? Certainly not; if its success derives from the linear form of the equations, one must be able to apply ideas of the same kind to all problems that relate to equations of a linear form, and even to ordinary differential equations, because their solution can always be reduced to that of a first-order linear partial differential equation.

For some time, people have tackled the Dirichlet problem and other connected problems by another means, by going back to Dirichlet's original idea and seeking the minimum of a definite integral, but this time by rigorous procedures. I do not doubt that one can without great difficulty bring the two methods together, taking account of their mutual relationships, and I do not doubt that both have much to gain thereby. Thanks to M. Hilbert, who has been doubly the initiator, we will proceed on that path.

V. Abelian Functions

One knows the principal question that remains to be resolved in the theory of Abelian functions. The Abelian functions generated by a curve are not the most general; they are only a particular case that we can call the special Abelian functions. How are they related to the general functions, and how can we classify the latter? Not long ago, the solution seemed remote. Today I consider the problem as virtually solved since MM. Castelnuovo and Enriques have published their memoir on the integrals of total differentials on varieties of more than two dimensions. We now know that there are Abelian functions attached to a curve and others to a surface and that it will never be necessary to pass to varieties of more than two dimensions. By combining this result with those that follow from Wirtinger's work, we will get without doubt to the end of our difficulties.

VI. Theory of Functions

It is above all of the theory of functions of two or more variables that I wish to speak. The analogy with the theory of functions of a single variable is a precious guide but is insufficient. There is an essential difference between the two sorts of functions, and every time one attempts a generalization passing from one to the other, one comes on an unexpected difficulty that one sometimes overcomes by special tricks, but which has often remained unsurpassable until now. We must therefore study the facts, which are of the kind that will illuminate for us the essence of the difference between functions of one variable and those that contain several. It is first necessary to look closely at the tricks that have been used in particular cases and then to see what they have in common. Why is conformal representation usually impossible in a domain of four dimensions, and what is necessary to put in its place? Is not the true generalization of functions of one variable the harmonic functions of four variables, of which the real parts of functions of two variables are only particular cases? Can one include in the theory of the transcendental functions of several variables what one knows about algebraic and rational functions? Or, in other terms, in what sense can one say that transcendental functions of two variables are to transcendental functions of one variable, as rational functions of two variables are to rational functions of one variable?

Is it true that if $z = f(x, y)$ then one can write x, y, z as uniform functions of two auxiliary variables or, to use an expression that has begun to be consecrated by use, can one uniformise functions of two variables as one uniformises functions of one? I restrict myself to raising the question, to which the immediate future may perhaps provide the solution for us.

VII. Theory of Groups

The theory of groups is an extensive subject on which there is much to say. There are several sorts of groups, and whatever the classification adopted, one always finds new groups that have not been included. I want to restrict myself here and speak only of Lie's continuous groups and Galois's discontinuous groups, which one is accustomed to qualify in each case as groups of finite order, although "finite order" does not at all have the same sense in the one case and in the other.

In the theory of Lie groups, one is guided by a particular analogy: a finite transformation is the result of infinitely many infinitesimal transformations. The simplest case is the one in which the infinitesimal transformations reduce to multiplication by $1 + \varepsilon$, ε *being very small. The repetition of this function generates the exponential transformation; this is how Napier came to it. We know that the exponential function can be represented by a very simple and rapidly*

convergent series, and this analogy can show us the way to follow. This analogy can be expressed by a very special symbolism, and the reader will forgive me if I do not insist on it here. We have made great progress here, thanks to Lie, Killing, and Cartan. It only remains to simplify the proofs and to coordinate and classify the results.

The study of Galois groups is much less advanced, and that explains itself; it is for the same reason that arithmetic is less advanced than analysis, because continuity confers great advantages from which one has profited. But happily there is a manifest parallelism between the two theories, which one must insist on and must bring more and more into evidence. The analogy is quite similar to what we have noted between arithmetic and algebra and makes the same inclusion.

VIII. Geometry

It would seem that geometry can contain nothing that is not already contained in algebra or analysis, and that geometric facts are nothing but the facts of algebra or analysis expressed in another language. It might be supposed, then, that after the review that has just been made, there would be nothing left to say having any special bearing on geometry. But this would imply a failure to recognize the great importance of a well-formed language, or to understand what is added to things themselves by the method of expressing, and consequently of grouping, those things.

To begin, geometric considerations lead us to set ourselves new problems. These are certainly, if you will, analytical problems, but they are problems we should never have set ourselves on the score of analysis. Analysis, however, profits by them, as it profits by those it is obliged to solve in order to satisfy the requirements of physics.

One great advantage of geometry lies precisely in the fact that the senses can come to the assistance of the intellect and can help to determine the road to be followed, and many minds prefer to reduce the problems of analysis to geometric form. Unfortunately our senses cannot carry us very far, and they leave us in the lurch as soon as we wish to pass outside the three classical dimensions. Does this mean that, when we have left this restricted domain in which they would seem to wish to imprison us, we must no longer count on anything but pure analysis, and that all geometry of more than three dimensions is vain and without object? In the generation that preceded ours, the greatest masters would have answered "Yes". Today we are so familiar with this notion that we can speak of it, even in a university course, without exciting too much astonishment.

But of what use can it be? This is easy to see. In the first place it gives us a very convenient language, which expresses in very concise terms what the ordinary language of analysis would state in long-winded phrases. More than that, this language causes us to give the same name to things that resemble one another, and states analogies that it does not allow us to forget. It thus enables us still to find our way in that space that is too great for us, by calling to our mind continually the visible space, which is only an imperfect image of it, no doubt, but is still an image. Here again, as in all the preceding examples, it is the analogy with what is simple that enables us to understand what is complex.

This geometry of more than three dimensions is not a simple analytical geometry, it is not purely quantitative, but also qualitative, and it is principally on this ground that it becomes interesting. There is a science called Geometry of Position, which has for its object the study of the relations of position of the different elements of a figure, after eliminating their magnitudes. This geometry is purely qualitative; its theorems would remain true if the figures, instead of being exact, were rudely imitated by a child. We can also construct a Geometry of Position of more than three dimensions. The importance of Geometry of Position is immense, and I cannot insist upon it too much; what Riemann, one of its principal creators, has gained from it would be sufficient to demonstrate this. We must succeed in constructing it completely in the higher spaces, and we shall then have an instrument that will enable us really to see into hyperspace and to supplement our senses.

The problems of Geometry of Position would perhaps not have presented themselves if only the language of analysis had been used. Or rather I am wrong, for they would certainly have presented themselves, since their solution is necessary for a host of questions of analysis, but they would have presented themselves isolated, one after the other, and without our being able to perceive their common link.

What has contributed above all to recent progress in geometry is the introduction of the notion of transformations and groups. It is thanks to this that geometry is no longer a collection of more or less curious theorems that follow each other without any resemblance, but geometry has achieved a unity. And on the other side, the history of science must not forget that it is in geometry that we began to study continuous transformations systematically, so that pure geometry has played its part in the development of the idea of a group that is so useful in other branches of mathematics.

The study of groups of points on an algebraic curve in the manner of Brill and Noether still gives us useful results, either directly or by providing a model for analogous theories. It is in this way that we have developed a whole chapter in the theory of geometry where curves on a surface play a role similar to that of points on a curve. In this way we can hope today to clear up the final mysteries that belong to the theory of surfaces and that appear so persistent.

Geometers therefore have a vast field to explore, and I must be careful not to forget enumerative geometry and infinitesimal geometry, cultivated with such skill by M. Darboux and to which M. Bianchi has made such useful contributions. If I do not say anything on this subject, it is because I have nothing to tell you after M. Darboux's brilliant lecture.

IX. Cantorism

I have spoken previously of the need for returning continually to the first principles of our science, and of the advantage of this process to the study of the human mind. It is this need that has inspired two attempts that have each held a very great place in the most recent history of mathematics. The first is Cantorism, and one knows the services it has rendered to science. [Here Poincaré added some text for the later editions.] One of the characteristic features of Cantorism is that, instead of rising to the general by erecting constructions that are more

and more complicated, and defining by construction, Cantorism starts with the *genus supremum* and only defines, as the scholastics would have said, per *genus proximum et differentiam specificam*. Hence exists the horror that *it* has sometimes inspired in certain minds, such as Hermite's, whose favourite idea was to compare the mathematical with the natural sciences. For *most* of us, these prejudices have been dissipated, but it has come about that we have run against certain paradoxes and apparent contradictions, which would have rejoiced the heart of Zeno of Elea and the school of Megara. Then *everyone sought* a remedy, each man his own way. For my part I think, and I am not alone in so thinking, that the important thing is never to introduce any entities but such as can be completely defined in a finite number of words. Whatever be the remedy adopted, we can promise ourselves the joy of the doctor who is called in to follow a fine pathological case.

X. The Search for Postulates

Attempts have been made, from another point of view, to enumerate the axioms and postulates more or less concealed, which form the foundation of different mathematical theories, and in this direction Mr Hilbert has obtained the most brilliant results. It seems at first that this domain must be strictly limited, and that there will be nothing more to do when the inventory has been completed, which cannot be long. But when everything has been enumerated, there will be many ways of classifying it all. A good librarian always finds work to do, and each new classification will be instructive for the philosopher.

I here close this review, which I cannot dream of making complete *for a number of reasons, above all because I have already abused your attention too much*. I think that these examples will have been sufficient to show the mechanism by which the mathematical sciences have progressed in the past, and the direction in which they must advance in the future.

Remarks on ''L'avenir des Mathématiques''

The generalities with which Poincaré introduced his subject are well-known, but nonetheless still interesting. His opening words "To predict the future of mathematics the proper method is to study its history and its present state", allowed his audience to decide whether he agreed or disagreed with Hilbert's approach eight years earlier. But as Poincaré went on to say, he did not fear that mathematics was about to become exhausted, but rather that it would grow so much that it would produce "a mass just as impenetrable as the unknown truth was to the ignorant". Therefore, he deduced, we are forced to make a selection of the facts, especially if one is a mathematician who creates those facts. The grounds for selection should not be narrowly utilitarian – even physicists, he pointed out, built their theories in advance of the electrical technologies that could not have been discovered without them – nor should mathematicians take their instructions from natural scientists.

Poincaré's style was always to make points delicately, but these opening remarks were not mere commonplaces. Hilbert had opened his address with a long historical reach, going back to Fermat and his "last" theorem and Johann Bernoulli's derivation of the curve of quickest descent. Poincaré chose instead to put an accent on more recent developments in physics. Thus he pointed to Maxwell and Hertz, physicists on whose theories of electricity, magnetism, and optics Poincaré was an acknowledged authority. He noted the unexpected implications of these theories for wireless telegraphy, another topic with which he was intimately acquainted. One of his positions was professor of electrical theory in the École professionnelle supérieure des Postes et des Télégraphes in Paris, where he lectured on the propagation of electric current (published 1904), telephony (1907), and wireless telegraphy (1908, 1911). Whether this difference of emphasis was a merely personal question of taste, or a significant difference of opinion with Hilbert about the scope and nature of mathematics, Poincaré did not presume to say.

Poincaré was no great advocate of facts, whether in mathematics or physics, and instead advocated Mach's principle of economy of thought. "The importance of a fact", he said, "is measured by the return it gives – that is, by the amount of thought it enables us to economise". In line with his lifelong insistence on the importance of discovery in mathematics, he emphasised that what mathematicians call elegance in a good proof is a reflection of an underlying harmony, which in turn introduces order and unity. This "enables us to obtain a clear comprehension of the whole as well as its parts. But that is also precisely what causes it to give a large return." Far from being an aesthetic matter, he suggested that our emotional satisfaction derives from a conformity between the sought-for solution and the necessities of our mind, whereas a lengthy calculation, even if it leads to a striking result, is not satisfying until it is coupled with an explanation that, albeit retrospectively, allows us to predict at least the characteristic features of the result.

For Poincaré efficiency was the key, though it should also be accompanied by rigour. He noted that standards of rigour had risen steadily in mathematics and that this would surely continue, since "a demonstration that lacks rigour is nothing." Hilbert, too, had insisted on rigour and had argued strongly against the idea that rigour was the enemy of simplicity. He had insisted that arithmetic and analysis held no monopoly on rigorous arguments, contending that geometry and even parts of physics were capable of being placed on similarly firm foundations. In his Paris address and long afterward, axiomatics emerged as his watchword for upholding new standards of rigour in all of mathematics and beyond. New symbols and new signs would be invented, he said, to handle new concepts. Poincaré 's emphasis was quite different, though he, too, noted the importance of modern conceptualisation. Looking to the recent past, he put his trust in well-chosen terms that encapsulate progress, while preventing rigorous proofs from becoming so long as to be almost incomprehensible: these key concepts included "uniform convergence", "group", and "invariance" (which were among his lifelong themes), or the notion of "energy" in physics.

Poincaré made at least an indirect nod to Hilbert at the outset of his speech, in a passage in which he noted that mathematics is bordered by philosophy on one side and physics on the other:

> On the one side, mathematical science must reflect upon itself, and this is useful because reflecting upon itself is reflecting upon the human mind which has created it; the more so because, of all its creations, mathematics is the one for which it has borrowed least from outside. This is the reason for the utility of certain mathematical speculations, such as those which have in view the study of postulates, of unusual geometries, of functions with strange behaviour. The more these speculations depart from the most ordinary conceptions, and, consequently, from nature and applications to natural problems, the better will they show us what the human mind can do when it is more and more withdrawn from the tyranny of the exterior world; the better, consequently, will they make us know this mind itself.

All these matters had deeply preoccupied Hilbert, whose "unusual geometries" were also a source of some discomfort for Poincaré. He had reviewed Hilbert's *Grundlagen der Geometrie* in highly positive terms in 1902 and 1903, but his own view of geometry was intimately tied to a philosophy that explained how knowledge of the external world was possible at all, and Poincaré badly missed any consideration by Hilbert of the psychological origins of the axioms (1903, 23): "The axioms are postulated; we do not know where they come from . . .. His work is then incomplete; but this is not a criticism which I make against him. Incomplete one must indeed resign one's self to be." The mind, the knowing subject that constructs the world around it, was a concern of Poincaré's but not one of Hilbert's.

Turning from philosophy to physics, Poincaré next made his own priorities plain. "But it is to the opposite side, to the side of nature", he went on, "that we must direct our main forces." Here, he said, we find that ninety-nine percent of the problems we face cannot be solved exactly in terms of known functions; moreover, the convergence of power series solutions is often too slow to be of any use, so progress can only be made qualitatively. Our standards have changed over the decades: "There are, therefore, no longer some problems solved and others unsolved, there are only problems *more or less* solved . . .." Poincaré may not have known that Hilbert was actively lecturing in Göttingen on topics in physics, drawn in partly by the enthusiasm of his friend Minkowski (see Corry 1999, 2004). But Poincaré's public involvement with the subject was patent – formally, at least, he was professor of general astronomy at the École Polytechnique, and had been since 1904 – and his ideas regarding qualitative progress stood in sharp contrast to Hilbert's quite different message to his fellow mathematicians, namely that every well-posed problem has a solution. Hence, for Hilbert, the challenge was clear: we should either be able to solve any such problem or to prove it is unsolvable.

Following these generalities, Poincaré turned to specifics, and at this point, the text in *Science et Méthode* is much reduced from the original, obscuring the fact that Poincaré was alluding to his own early work.

He started with arithmetic, and here he was of the opinion that progress had been slow because one could not appeal to continuity. He therefore concluded that arithmetic should be guided by the numerous analogies with algebra. He offered some analogies between the theory of congruences and that of algebraic curves, predicting that the solution of problems about congruences in several variables would lead to considerable progress in indeterminate analysis. He also drew on an analogy between fields and ideals on the one hand, and curves on surfaces on the other, noting that both could be explored to their mutual advantage. Likewise the theory of quadratic forms was intimately connected with the theory of ideals. This was a subject Poincaré had explored in his article (1885), but neither then nor in 1908 did he choose to separate out his contributions from those of Dedekind, which had by then been swept up into the panorama of Hilbert's *Zahlbericht*. Poincaré's way of thinking about mathematics relied heavily on analogy as a means for suggesting proofs, but the explicit analogy with curves on surfaces is unexpected and reflects his interest in contemporary work, both in France and Italy, on this subject. Indeed, another reason Poincaré came to Rome was to award the first Guccia medal of the Circolo matematico di Palermo to Severi for his work on algebraic geometry.

It would seem likely that Poincaré had in mind the analogy between the ring of algebraic functions on a curve or surface and the rings of algebraic integers in a number field. This had been the motivating analogy in Dedekind and Weber's paper on Riemann surfaces (1882). The extension of these ideas to algebraic surfaces and higher-dimensional varieties is explicit in Kronecker's famous *Grundzüge* (1882), and the extension was amplified in the small literature devoted to explaining what Kronecker had not been able to say in a way people had found easy to follow (e.g., in König 1903). By 1908, the best that had been done in the context of ideals in polynomial rings in several variables was Lasker's paper (1905) where he established the theorem on the factorisation of any ideal into its primary components. This work was heavily algebraic, and the Italian geometers Castelnuovo and Enriques complemented the geometric side with their work on algebraic surfaces, which in turn fed into the prolonged attempts by Picard and Simart to produce a generalisation of Riemann's theory to complex functions of two complex variables.

It is likely that Poincaré knew of Picard's work and that of the Italians, but it is much less certain that he knew of the German work. He had a reputation of not being very well read, and his citations of German works are sparse. Whatever may be the modern implications of the analogy that Poincaré made, its vagueness on the day of the lexture may also reflect that he had not thought about it very deeply. In any case, he did not go on to exploit it himself.

What unity Poincaré could find in arithmetic had come about through the use of transformations; indeed, this had been the approach that guided his own study of forms of degrees 3 and 4 in his earliest work on number theory (Poincaré 1881, 1882). He noted that geometry offered examples of many types of transformations, not all of them linear, and he suggested how these could be applied in arithmetic. The groups that would arise would, without doubt, be discontinuous, and should be analysed by looking at their fundamental domains, where the most important contribution was to be found in Minkowski's *Geometrie der Zahlen*. It is amusing to see how Poincaré was quite far into his speech before he utters the name of a mathematician (and

that it is not that of Hilbert!). He then added some words about one of his own earliest interests, Hermite's theory of forms with real coefficients, before concluding with some rather more perfunctory remarks about the study of prime numbers, where he could find no unity in the few asymptotic laws that were known.

In algebra, he found that the subject had long been dominated by the theory of equations. This topic, he said, was best approached by way of the theory of groups, which he would discuss later, but there was also the question of the numerical solution of equations. Less trite were his remarks about the theory of invariants, which he observed had unfolded over some 40 years but had recently been abandoned, although the subject was far from exhausted. There was, for example, the problem of determining the invariants of an arbitrary group (this is Hilbert's 14th problem), but there were also still important problems to be solved in conventional invariant theory, despite Hilbert's happy simplification of Gordan's algorithmic work. Furthermore, Poincaré raised the prospect of a study of the algebra of polynomials modelled on arithmetic. This, although he did not say so, was already the hope and intention of several German mathematicians (Kronecker, Dedekind, and Hilbert), who looked for analogies between fields of functions and number fields.

Turning to ordinary differential equations, Poincaré remarked that although linear differential equations were by now well understood, nonlinear equations were not. A start had been made on the study of their singular points, but Poincaré held out hope that a fruitful analogy would be found with Cremona transformations, which are used to simplify the study of the singular points of algebraic curves. This idea was taken up, perhaps independently, by Henri Dulac, who documented some work with it in his paper (1908), and by Poincaré's nephew Pierre Boutroux in lectures he gave at the Collège de France in the same year. It is not possible here to survey what came of this idea, but an important recent development came in the 1980s when the Brazilian school took up the study of singular points of holomorphic differential equations (see Cerveau 2006, 2010, and the articles cited therein). True to his custom of mentioning very few names, Poincaré overlooked that Picard had discussed this topic in the 1890s in his *Traité d'analyse*, vol. 3, Ch. IV. Instead, Poincaré noted that there were important unsolved problems in the uniformisation of the solution functions. The qualitative study of the solutions, even in the case of first-order, first-degree equations, had led to delicate questions about limit cycles and included analogies with the study of the real roots of algebraic equations. That, in turn, was connected with the theme of Hilbert's 16th problem, which, however, Poincaré neglected to mention.

Taking up the study of partial differential equations, Poincaré expressed his admiration for the pioneering work of Fredholm. Mittag-Leffler had brought this to his attention some years earlier, and he offered some thoughts on the underlying reason for Fredholm's success. This was tied, he emphasized, to the interesting question as to why so many of the partial differential equations used in physics were linear. Poincaré claimed this derived from the modelling arguments that were used to construct the equations. These methods turned the behaviour of infinitely many discrete molecules into a few continuous processes, for example, via the theory of infinite determinants, which appeared in both Fredholm's work as well as in that of the American lunar theorist George William Hill (1877). The latter had provided inspiration to Poincaré for his work on celestial mechanics. But much remained to be done in this area, for example, on the Dirichlet problem and on convex domains. Here Poincaré pointed to recent progress that had been made thanks to Hilbert's initiatives.

Hilbert had already signalled his interest in such matters in his ICM address, where the closing problems 19, 20, and 23 deal with the calculus of variations and related boundary-value problems, including the Dirichlet problem. But by 1908 he was even more prominently associated with work on function spaces and functional analysis, so here it is interesting to note that Poincaré took every opportunity to promote Fredholm's merits. This was partly because he recognised that Fredholm had seen more deeply into the problems of partial differential equations and mathematical physics than he had, but also because he appreciated that Fredholm had come up with a simple and productive new formulation of the subject, one that conformed to his own Machian views about the economy of thought. He was to reiterate his high opinion of Fredholm's work in the first of his six Göttingen lectures in 1909 – it would be interesting to know how such an unorthodox opinion was accepted – and again when he gave the Bolyai Prize of the Hungarian Academy of Sciences to Hilbert in 1910. For Poincaré, Fredholm was the discoverer, however great the subsequent achievements of Hilbert and his school might have been.

There follow some brief, and it must be said, obscure remarks about Abelian functions in the context of the Schottky problem. An Abelian function in p variables is a function on $\mathbb{C}^p$ that is periodic with respect to a lattice of real dimension $2p$. The point at issue is that, as Riemann had shown in his paper on Abelian functions, the period relations for an algebraic curve of genus $p > 1$ gives rise to a $p \times p$ symmetric matrix that determines a lattice in p-dimensional complex space $\mathbb{C}^p$. The matrix reflects the complex structure of the curve, which is in turn determined by its $3p - 3$ moduli, so the space of all possible such lattices arising from algebraic curves in this fashion has dimension at most $3p - 3$. However, the space of all $p \times p$ symmetric matrices is of dimension $\frac{1}{2}p(p + 1)$, which is greater than $3p - 3$ as soon as $p \geq 4$. The Schottky problem, first raised in 1888, asks for a characterisation of the lattices that arise from curves. As Poincaré noted, the study of Abelian functions, and especially those that do not arise from algebraic curves, had been blocked for some time, but now he noted that it had been almost completely solved in the recent work of Castelnuovo and Enriques. This, together with Wirtinger's results (no mention of Picard or Painlevé!), resolved many of the outstanding difficulties.

Poincaré himself was to publish a major paper in this area in 1910, so one wonders if he already had such a contribution in mind 2 years earlier. The notion of the genus of an algebraic curve generalises in two ways to algebraic surfaces: the geometric genus p_g is the dimension of the space of holomorphic 2-forms, and the arithmetic genus p_a (which is too complicated to define here) captures some of the

singularities of the surface when it is given as an embedding in $\mathbb{C}^3$. Even taken together, these two genera do not suffice to characterise a surface up to birational equivalence; when these numbers are not the same, the surface is said to be irregular. In 1904 Severi had showed that a surface was irregular if it admitted nonzero holomorphic 1-forms, and Enriques had promptly established the converse. Almost at once, Severi then showed that the irregularity, the difference between the two genera, was $r - q$, where q and r were the dimensions of the spaces of holomorphic 1-forms and 1-forms with simple poles (this was among the papers for which he was awarded the first Guccia medal in 1908). Finally, Castelnuovo and then Severi presented different proofs that showed that $q = p_g - p_a$ and $r = 2(p_g - p_a)$, although their arguments hinged on a result of Enriques that was not wholly convincing. Picard and Simart gave considerable prominence to this result in the second volume of their *Théorie des fonctions algébriques de deux variables indépendants* in 1906, because the result made a connection between the geometrical theory of the Italians and the analytical or transcendental theory in France. As Castelnuovo and Enriques remarked in an appendix they contributed to that book (Picard, Simart 1906, 495): "This result is therefore the fruit of a long series of researches, to which the transcendental methods of M. Picard and the geometrical methods used in Italy contributed equally". It is this theorem that Poincaré proved analytically in his article (1910), when he showed that the irregularity is the maximum number of holomorphic 1-forms on the surface. However, all was not well with the Italian approach, specifically with Enriques's less than convincing result. This whole story has recently been told in Babbitt and Goodstein (2011) and Mumford (2011), but until Mumford's (1966) paper, the only accepted proof was the one Poincaré gave in 1910, an argument that Castelnuovo was to describe almost 30 years later as displaying "the indelible mark of [Poincaré's] universal genius".[4]

Poincaré's next topic was the general study of functions of two complex variables, which, as he knew very well, was different at every turn from functions of a single variable. Among the many disparities, Poincaré mentioned the lack of a uniformisation theorem in two or more variables. His remark that "conformal representation is usually impossible in a domain of four dimensions" probably does not refer to an old result of Liouville's, but rather alludes to his own paper (1907) on holomorphically inequivalent, but topologically equivalent domains in $\mathbb{C}^2$. This implies the failure of the Riemann mapping theorem in two or more complex dimensions.

At this stage, he took up the theory of groups, which interestingly appears as a topic separate from algebra. It was also much too large, so Poincaré restricted himself to Lie's continuous groups and Galois's discrete or finite ones. Even so, the result is curiously perfunctory for someone whose interest in groups and geometry was so profound. Poincaré had long admired Lie's work, although he only contributed to it after Lie's death in 1899 with his version of what came to be called the Campbell-Baker-Hausdorff theorem and the related Poincaré-Birkhoff-Witt theorem. Poincaré merely observed that much had been done by Lie, Killing, and Cartan that now needed to be simplified and coordinated. The study of finite groups lagged for the same reason that arithmetic lagged behind algebra: the lack of continuity; but Poincaré hoped that the analogy between the two kinds of groups would prove fruitful. This odd remark may reflect Poincaré's situation as a French mathematician for, despite what should have been the enormous boost to the subject given by Jordan in his *Traité des Substitutions et des Équations Algébriques* of 1870 and other works, it was Serret's older and less forward-looking account of algebra that had prevailed in French higher education. The study of finite groups fared much better in Britain with Burnside, in Germany with Frobenius, and in the United States with Dickson, whereas Galois theory served as a metaphor and guiding theme in Hilbert's *Zahlbericht* (1897).

Poincaré then turned to geometry, and the text of *Science et Méthode* carries all but the final remarks, where, indeed, Poincaré attributed much of the recent progress to the introduction of transformation groups. Indeed, Poincaré's only criticism of Hilbert's *Grundlagen der Geometrie* had been that he had not looked at the role groups play in this new geometry.

The original text and the later published versions now agree to the end, including the penultimate topic: Poincaré's famous critique of Cantorism, the mathematical study of infinity. The controversies initially caused by Cantor's theory, said Poincaré, stemmed from the fact that its starting point was one of complete generality. This had provoked a true sense of horror in Hermite's mind, but by now most mathematicians had come to terms with it. However, this was only the subjective side of the problem, for there were now:

> certain paradoxes and apparent contradictions, which would have brought joy to the heart of Zeno of Elea and the school of Megara. Then began the business of searching for a remedy, everyone in their own way. For my part I think, and I am not alone in so thinking, that the important thing is only to introduce such entities as can be completely defined in a finite number of words. Whatever be the remedy adopted, we can promise ourselves the joy of the doctor called in to follow a fine pathological case.

This passage is the supposed source of a famous remark that Poincaré did *not* make.[5] The only-too-familiar quote runs: "Later generations will regard *Mengenlehre* as a disease from which one has recovered." It implies that Poincaré was strongly opposed to the study of the theory of sets. As we can see, Poincaré's position was much more interesting, if less colourful.

Poincaré concluded his address with some remarks on what he called the search for postulates, or what we might call the axiomatic method, noting that "in this direction Mr. Hilbert has obtained the most brilliant results". He then offered the hope "that these examples will have been sufficient to show the mechanism by which the

[4]Castelnuovo, 1938, Bologna ICM, I, 196, quoted in Poincaré *Oeuvres* 6, 178 n.1

[5]For an account of how E. T. Bell came to make this particular error, see Gray (1991).

mathematical sciences have progressed in the past, and the direction in which they must advance in the future."

Hilbert and Poincaré : Contrasting Visions

Poincaré's complete address, unlike the much truncated version in *Science et Méthode*, offers considerable insight into his approach to mathematics now that he had become a senior statesman of science. Its central theme – that mathematical understanding is expressed in the productive organisation of facts and the recognition of analogies – indeed reflects how Poincaré had guided his own work for almost 30 years. In fact, his text follows rather closely the arc of his career, from his early work on number theory under the influence of Hermite to his clashes over foundations with Couturat, Russell, and Zermelo after 1900. He mentioned relatively few people by name, but even so those names make an interesting list: Minkowski, Hilbert of course, Lie and Killing, Castelnuovo and Enriques, Hermite and Darboux; but neither Picard nor Painlevé appear, and only Cartan among the next generation of French mathematicians.

There is a curious way in which Hilbert and Poincaré may be said to have danced around each other, and while doing so, they reached unexpected positions. The French view of German mathematics, expressed forcefully by Hermite, and shared, for example, by Picard, was that it was needlessly abstract; it did not grow out of interesting problems but imposed a way of thinking from above that might not accomplish anything worth doing. This was Hermite's view of Cantor's transfinite sets: the mathematics was not wrong; it was worse than wrong, it was an affront to the sensibilities of a mathematician who deeply admired work in the tradition of Jacobi and Weierstrass. Cantor's theory was perhaps an extreme example, but in their different ways many German mathematicians were open to the charge of excessive abstraction: Dedekind's structural approach to number theory would be another example. Yet this was not how his German contemporaries saw Hilbert. They saw him as a man of problems, someone who could bring formidable skill to the resolution of difficult challenges. To be sure, his ICM address in Paris was animated by a vision of the unity of mathematics, by the interplay he saw between theory and problems, but it was, first and foremost, a presentation of problems. Whereas some were general, others were quite specific (the continuum hypothesis, the Riemann hypothesis), and most drew attention to a specific topic in a way that would enable an ambitious mathematician to say that he or she had eventually solved a Hilbert problem.

That was surely not the case with Poincaré's address in 1908. He might suggest an analogy between two fields, he might even challenge others to find one, but it would be much more difficult for anyone to say he or she had solved a "Poincaré problem". The breakthrough Poincaré obviously found most profound was Fredholm's: here was a way of posing problems as integral equations that would surely be fruitful in many settings. This was Poincaré's most cherished belief, often asserted in his writings: the right place to stand was the most important thing a mathematician can possess. Once attained, all manner of facts and problems can be seen in an orderly and productive way. This is what he asked of himself and, according to his nephew Pierre Boutroux (1914, 1921), this is what he asked of others. Clearly, Henri Poincaré moved on a high plane of generality. In his address, he invited his listeners and later readers to contemplate surfaces and the curves on them, equations algebraic or differential and their singular points, transformations such as those that had been so productive in geometry, various analogies that might be invoked when continuity is not available. In his own work across many fields there is little by way of specifics: a theory of Fuchsian groups, for example, and of their different types, but never a detailed study of a particular Fuchsian group; theories of various types of differential equations, but very seldom any illustrative examples. Even in his work on physics, Poincaré disdained examples and was only brought to ground by specific failures to match theory with experiment.

Comparing these two lectures, we can discern what were surely temperamental differences, but also striking differences when it came to priorities. Hilbert began his survey of key problems with foundational questions, whereas Poincaré ended his with reflection about such issues. Other differences were also at play. Hilbert was comfortable with people around him and was stimulated by the competitive atmosphere in Göttingen. Poincaré preferred his own company, or that of a select few. As a result, Hilbert could spin off ideas that others would then take up afterward. Poincaré clearly left much for others to do as well, but Paris was not the place for ambitious students who sought the kind of intense guidance several universities in Germany could offer to them. This was changing with the presence of Émile Borel and the people around him, but they were working on topics in real and complex analysis that held little interest for Poincaré.

There is still another way in which these two addresses differ. No one could mistake the seriousness of Hilbert's intentions, one measure of which was the scope of his 23 Paris problems. With few exceptions they were well chosen: central to their field, undoubtedly difficult, and well worth tackling. Poincaré's address could more easily be taken for polite remarks, although it was certainly much more than that. What it did not do, unlike Hilbert's address, was offer a clear view of what mathematics is: certain theories in which certain key problems are presently central. Hilbert's was an open-ended wish list of problems whose solutions would mark breakthroughs in the subject. Poincaré's address, by contrast, offered a view of what it meant to do mathematics, how the mind can be organised most productively. It tells us about the best ways to think so that good mathematics can be done. In the main, Hilbert selected problems that others found important and mathematics did advance when they were solved. Poincaré offered advice, however elusive, that whenever it can be taken will always enable mathematics to advance.

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