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Forgotten and neglected theories of Poincaré

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Abstract. This paper describes a number of published and unpublished works of Henri Poincaré that await continuation by the next generations of mathematicians: works on celestial mechanics, on topology, on the theory of chaos and dynamical systems, and on homology, intersections and links. Also discussed are the history of the theory of relativity and the theory of generalized functions (distributions) and the connection between the Poincaré conjecture and the theory of knot invariants.

Contents

The list of creators of modern mathematics starts from the names of Newton, Euler and Poincaré.

Poincaré's point of view on mathematics was very different from the formalist ideas of Hilbert or Hardy: mathematical science was for Poincaré an important part of physics and the natural sciences, rather than the art of permutations of symbols.

Describing mathematical problems (some years before Hilbert's celebrated list), Poincaré divided them into two parts: the *binary problems* (similar to the Fermat problem, where the answer is a choice between the two possibilities: yes or no), and the *interesting problems*, where the progress is continuous, studying first of all the possibility of variations of the problem (such as variations of the boundary conditions for a differential equation) and investigating then the influences of these variations on the properties of the solutions (which would be hidden, if the problem were formulated as a binary one).

Poincaré followed rather the ideas of Francis Bacon (who claimed that to start scientific investigations from general axioms and principles is a dangerous and damned method, leading to unavoidable mistakes) than the Cartesian theory (saying that conformity to any reality is unrelated to science, which is the art of deducing corollaries of arbitrary axioms).

Poincaré's prediction of the most important problems for the coming 20th century was: "to study the mathematics needed for the future development of quantum physics and the theory of relativity."

Comparing today the influence of Poincaré's and Hilbert's problems, one observes that the mathematics of the 20th century has followed rather Poincaré's suggestions, be it in the development of topology, created by Poincaré (and being the main achievement of 20th century mathematics), or of mathematical physics (where we should mention first and foremost H. Weyl, a student of Hilbert whose contributions to quantum theory, especially to the discovery of the Schrödinger equation, have remained unknown to most modern mathematicians), or of the ergodic theory of chaos and of dynamical systems (originating in Poincaré's works on celestial mechanics and on ordinary differential equations).

These contributions of Poincaré have been mostly unnoticed by the historians of science, and I shall describe only a few particular cases.

1. Bifurcation theory. Poincaré's thesis contained the versal deformation theorem (called by him "Lemma 4") for holomorphic complete intersections. In modern mathematics this basic statement of bifurcation theory (developed by Poincaré for his study of the bifurcations of periodic orbits in the 3-body problem of celestial mechanics) is usually attributed to Grothendieck, Malgrange and Thom.

Grothendieck and Thom studied this problem for many years in their neighbouring offices at IHES, Bures-sur-Yvette. They never discussed this with each other, and the relation between their results remained unknown to both of them.

The important difference was that Thom wished to extend the results of the analytic case to the smooth case. While he did not succeed, he persuaded Malgrange (who for several years had not believed in this possibility) that the versal deformation theorem should be true in the smooth case too.

After several years of hard work Malgrange proved Thom's conjecture, which is now the celebrated *Malgrange theorem*, basic for the whole of singularity theory.

However, neither Thom nor Malgrange have ever observed the relations of the versal deformation theory to Poincaré's thesis (and to his studies on the bifurcations of periodic orbits, based on it).

While I was doing a simultaneous translation of Malgrange's talk on his results at a session of the International Congress of Mathematicians in Moscow (1966), I was suddenly stopped by Malgrange, who observed (although he was unable to understand my Russian words): "you are already translating some sentences which I have not yet uttered in my talk."

There was no written text to translate, but he had guessed correctly: the phrases I used in my "simultaneous" translation did indeed emerge in his talk a few minutes later.

Anyway, Poincaré's bifurcation theory was elaborated by the Russian mathematicians Pontryagin and Andronov already in the 1920s and 30s (due to the need to apply these bifurcations to radiophysics).

Andronov published (with complete proofs) the theory of the birth of a periodic motion of a dynamical system under a generic loss of stability of an equilibrium position, in the case when two eigenvalues of the linearised system cross the imaginary axis, moving from the stable to the unstable complex half-plane.

Andronov's theorem claims that (depending on the sign of some higher term of the Taylor series) exactly two generic cases may occur: either the stability of the equilibrium position is inherited by the new-born limit cycle (whose radius grows like the square root of the difference between the new value of the parameter and the value at the stability loss), or else the radius of the domain of attraction, which decreases like the square root of the difference between the growing parameter value and the future value at which stability will be destroyed, goes to zero at the moment of loss of stability.

The first case is called mild stability loss; the new-born periodic motion-attractor describes small oscillations near the old stationary regime. The second case is called hard stability loss; the behaviour of the system after this stability loss is very far from the equilibrium that loses its stability.

The proofs of these results of Andronov on bifurcations of phase portraits were based on Pontryagin's extension of Poincaré's results in the holomorphic case to the case of smooth systems of differential equations.

Poincaré's versal deformation Lemma 4 provided an estimate of the degree of a polynomial form to which the bifurcating system might be reduced by a change of variables.

The degrees of these polynomials depend on the holomorphic branching of the analytic implicit functions (in terms of the degree of degeneration of the principal part). Their estimates form a part of the Newton polyhedron theory, known in "modern mathematics" under the name of Puiseux, and depending on the complex continuations of the real functions to which one applies the "Puiseux series" (which Newton considered as his main contribution to mathematics).

Pontryagin had observed that one can eliminate all the complex variables theory from these bifurcation studies, proving the corresponding theorems on the birth of periodic motions for smooth dynamical systems, and Andronov used his results.

In the Poincaré–Pontryagin theory the practical problem of estimating the number of periodic orbits remains unsolved even in the simple case of perturbations of the Lotka–Volterra integrable system (in the so-called 16th problem of Hilbert).

In this problem the unperturbed system of differential equations has the form

$$\frac{dx}{dt} = x(a + bx + cy), \qquad \frac{dy}{dt} = y(p + qx + ry). \tag{1}$$

It has a first integral if the coefficients of the right-hand side satisfy a certain algebraic equation (a particular example is the Lotka–Volterra case, $b = r = 0$).

This first integral has the form

$$H(x, y) = x^{\alpha} y^{\beta} z^{\gamma},$$

where $z = 1-x-y$, for some suitable numbers α, β, γ and suitable linear coordinates x, y depending on the initial system (1).

To obtain the new-born cycles, the general Poincaré–Pontryagin–Andronov theory suggests studying the first integral variation, produced by variation of the dynamical system:

$$\frac{dx}{dt} = x(a + bx + cy) + \varepsilon f(x, y), \qquad \frac{dy}{dt} = y(p + qx + ry) + \varepsilon g(x, y). \tag{2}$$

This variation of the integral H is (in their approximation)

$$\delta H(h) = \varepsilon \int_{H(x,y)=h} \left(\frac{\partial H}{\partial x} f + \frac{\partial H}{\partial y} g \right) dt \tag{3}$$

(integrating along one period of the periodic motion $(x(t), y(t))$ of the unperturbed system (1) for which $H(x, y) = h$).

The difficult part of the theory is to understand the number of zeroes h of the equation $\delta H(h) = 0$: is it bounded for the generic perturbations εf and εg? The case where the perturbing functions f and g are second-degree polynomials is needed for Hilbert's 16th problem on the number of limit cycles of vector fields whose components are second-degree polynomials.

The answer is still unknown, in spite of the nice theorem (by Khovanskii and Varchenko): the number of solutions h of the corresponding equation $\delta H(h) = 0$ is bounded in the case of the Hamiltonian unperturbed equations (instead of (1))

$$\frac{dx}{dt} = -\frac{\partial H}{\partial y}, \qquad \frac{dy}{dt} = \frac{\partial H}{\partial x},$$

where H is a polynomial of known degree in x and y.

The Lotka–Volterra system (1) being non-Hamiltonian, its integral H is generically a transcendental function, and even in the case of rational numbers α, β and γ the degree of the corresponding polynomial (and the genus of the corresponding Abelian integral) are not bounded uniformly, and therefore the versal deformation theory for these systems of bifurcations of periodic orbits is unknown both in the smooth and in the holomorphic category, in spite of the fact that the right-hand sides of the Lotka–Volterra system consist of polynomials of degree 2 (1).

2. Cohomology theory. One other typical example of the discoveries of Poincaré neglected by the next generations is the invention of cohomology theory.

Kolmogorov, in inventing general cohomology theory in his four short notes in *C. R. Acad. Sci. Paris* in 1935, stated that his main inspiration came from Gunther's "theory of functions of domains" (which term was used by Gunther to describe his version of the "theory of distributions" or "theory of generalised functions" used by him before the twenties to obtain existence and uniqueness theorems for the differential equations of hydrodynamics).

Kolmogorov explained in his papers that his cohomology theory, a combinatorial algebraic theory, is a mathematical version of the general physical ideas of incompressible fluid flows and of magnetic field potentials and Gaussian linking numbers. He told me that all these ideas (including Dirac's δ-function and its higher-dimensional versions) were explicitly known to Poincaré, but that the few pages of Poincaré's exposition of these ideas were understood only by E. Cartan (whose explanation of these remarks of Poincaré later inspired de Rham's theorems).

It is possible that the formal level of rigour of what Poincaré published on cohomology theory is not exactly what "modern mathematicians" would like. He claimed, for instance, that the only two methods for teaching fractions is to cut, at least theoretically, into equal parts either an apple or a round pie; any other methods lead most students to rules like

$$\frac{1}{2} + \frac{1}{3} = \frac{2}{5}$$

(which is a simpler axiom than Dedekind's theory of pairs of integers and than the Grothendieck ring definition).

3. Sobolev equations. The modern attributions of mathematical discoveries usually follow the name of the last person to find them (America is never called Columbia).

L. Schwartz told me that Sobolev made a serious mistake, publishing his great discoveries (on generalised solutions of partial differential equations) in a provincial journal, using a little-known language, and that the main contribution of Schwartz himself to this theory was a translation of Sobolev's results (published by Sobolev in French in *C. R. Acad. Sci. Paris*) into English in a widely read journal. Sobolev told me that Schwartz did more, but I would like to repeat Kolmogorov's words that the contributions of Poincaré and Gunther (whose student was Sobolev) should not be forgotten.

At the end of the 1950s Sobolev explained to me his classified results on the oscillations of the fluid contents of rotating missiles, where he created the theory of "Sobolev equations" (declassified in 1960).

Today I know that the "Sobolev equation" had been published and studied by Poincaré in 1910 as the equation of hydrodynamics on rotating planets, in meteorology. At present this Poincaré–Sobolev theory is applied mostly to the theories of the atmospheres of Jupiter and Venus (in the case of the Earth it is also important, providing cyclonic activity waves due to the rotation of the Earth).

Sobolev's theory of Poincaré's equation was based on a new class of function spaces, discovered by him: the difference from the standard Hilbert space is the Lorentzian signature of the quadratic form defining the (Finslerian) metric of this Sobolev space.

Today these generalised Hilbert spaces are mostly called P-spaces (for Pontryagin, who extended Sobolev's Lorentzian metrics to the case of any finite number of negative squares). Pontryagin had some difficulty publishing his results, since Sobolev's original paper was classified.

In the last few years the Poincaré–Sobolev theory has been combined (by Babin, Makhalov and Nikolaenko) with the so-called KAM-theory of quasi-periodic motions, providing new averaging results in the meteorology of a rotating planet.

4. Principle of relativity. Perhaps the most celebrated rediscovery of Poincaré's theories is Einstein's principle of relativity.

Poincaré had published 10 years earlier, in 1895, a paper "on measurement of time" in a philosophical journal. There he had clearly explained that the Galilean or Newtonian notions of absolute space and absolute time have no empirical definitions, simultaneity being explicitly dependent on the way the clocks are synchronised.

According to Poincaré, the only scientific way to avoid this theoretical inconvenience is to postulate the complete independence of the true laws of nature from the arbitrariness of the coordinate systems used to describe experiments.

In his paper Poincaré avoided the mathematical formulae (well known to him), in order not to intimidate the philosophers with no mathematical background.

Minkowski, being a teacher of Einstein and a friend of Poincaré, had early suggested to Einstein that he should study Poincaré's theory, and Einstein did (though never referring to this until a 1945 article).

The mathematical part of the "special theory of relativity" was also published by Poincaré earlier than by Einstein (including the famous formula $E = mc^2$). However, Poincaré never claimed any priority, meeting Einstein at Solvay conferences and being extremely friendly to him and willing to help him.

5. Lorentz transformations. It is interesting to know that the famous "Lorentz transformations" of the special theory of relativity were also invented by Poincaré.

This discovery stemmed from Poincaré's Sorbonne lectures on the theory of electromagnetic fields and on Maxwell's equations. In his lectures Poincaré mentioned that Lorentz had studied the symmetry group of Maxwell's system of equations. Trying to include Lorentz's answer in his lecture, Poincaré started the proof, but was unsuccessful.

Some days later he observed that even the simpler theory of infinitesimally small transformations is strange: the infinitesimal symmetries should form a Lie algebra and it was not the case in this example.

Calculating further, Poincaré proved that the expected "symmetries" did not preserve Maxwell's equations. In the end he decided to solve the symmetries problem himself. The resulting Lie group (and the Lie algebra of the infinitesimal symmetries) were included by Poincaré in his course.

When he published his lectures, Poincaré chose for these newly-discovered transformations the name "Lorentz transformations", as they are known to everybody today.

It is similarly interesting to know that the "Stokes Lemma", basic both for cohomology theory and for Maxwell's theory of electromagnetic fields, was never invented nor proved by Stokes. Namely, its discoverer was Sir Thompson, Lord Kelvin (Stokes had transmitted Thompson's result to the Cambridge Tripos Committee, and Maxwell, a student there, therefore called it the "Stokes Lemma").

Strangely enough, I plagiarised Poincaré's attribution of his result to Lorentz, inventing the terms "Maslov index" and "Gudkov's conjecture" in symplectic topology and real algebraic geometry.

Maslov told me that the integer I called the "Maslov index" in my report on his thesis should not be attributed to him, because only its residue modulo 4 had physical importance in the quasi-classical theory, while my integer was useless.

Gudkov objected that the conjecture (on the divisibility by 16 of a certain topological invariant of real plane algebraic curves) which I had attributed to him in my review of his thesis was not conjectured by him, since he was aware of some counterexamples.

Insisting on the relation of this conjecture to both the differential topology of 4-manifolds and topological quantum field theory, I persuaded Gudkov that his counterexamples were wrong, and at present Gudkov's conjecture, proved by Rokhlin, is one of the main results of real algebraic geometry. This science started from Hilbert's 16th problem on the possible disposition of the 11 ovals of a real projective plane algebraic curve of degree 6. Hilbert claimed that there are only two possible arrangements of the 11 ovals. Gudkov proved that Hilbert's statement was wrong, that there are 3 arrangements (and Gudkov's conjecture implied the absence of any other).

It is interesting to note that only two problems (the 13th and the 16th) of Hilbert's list, regarded by him as a testament of the 19th century to the 20th century, are related to topology, which was the most active part of mathematics in the 20th century.

And, while for plane curves of degree 6 Hilbert proposed a (wrong) answer, claiming that he had proved it, the possible arrangements of the 22 ovals of a real projective plane algebraic curve of degree 8 are still unknown even today.

In this problem there are 268,282,855 topologically possible arrangements. Gudkov's conjecture and other restrictions known today reduce this number to about 90 cases. The number of known examples today exceeds 70. I don't know the latest upper and lower bounds, but this non-binary problem (in the sense of Poincaré) is still unsolved (in spite of the fact that the general problem of the possible topological configurations of the algebraic curves of a given degree is one of the most fundamental problems of mathematics, similar to the theory of ellipses and hyperbolas and much more important than, say, the binary Fermat problem).

6. Publishing Poincaré in Russian. The relativistic theory of Poincaré was used by the Moscow mathematician Bogolyubov in a very unusual way.

About 1970 I proposed to the Moscow Academy of Sciences editorial board of the "Classics" series to translate the main works of Poincaré into Russian. Unfortunately, the answer I got from Academician Logunov, a former student of Bogolyubov — and at the time chief editor of the "Classics" series — was negative. Logunov wrote: "As you ought to know, the idealistic and Machist ideas of the weak philosopher Poincaré were criticised in the 1909 book *Materialism and Empiriocriticism* (by V. I. Lenin). Therefore a Russian edition of any work of Poincaré is impossible."

My friends suggested a way to overcome Logunov. They explained to me that the head of the Mathematics Department of the Academy, Bogolyubov, had a very positive opinion of the works of Poincaré (which he extended himself in his papers on averaging theory). He also had a very positive opinion of Arnol'd (having published a book extending a result of Arnol'd). Therefore he might help to persuade Logunov to publish the collected works of Poincaré in Russian.

I phoned to Nikolai Nikolaevich Bogolyubov, and he immediately invited me to visit him at his apartment in the Moscow State University building at the Vorob'evy Gory. There, after reading Logunov's letter, he said the following clever words.

All three of us, he told me, — Poincaré, myself and you — are not just mathematicians, we are also physicists and even natural scientists.

The approach of a natural scientist to all phenomena, even to such dangerous phenomena as earthquakes and volcanic eruptions, is pragmatic: he tries to use even the worse things as a source of new scientific progress (measuring, for example, parameters of the interior structure of the planet).

I shall show you now, he continued, how to use for the progress of science even such a disagreeable phenomenon as the anti-Einsteinism and antisemitism of certain individuals.

With these words he took a sheet of white paper, headed with all his distinctions: senior member of the Academy of Sciences, director of the Joint Institute of Nuclear Research, and so on. And he wrote:
"Dear Anatolii Alexeevich,

Together with professors Arnol'd and Oleinik, I am proposing to publish in the "Classics" series of the Academy a project of selected works of Poincaré in three large volumes, including the relativity papers which he published before Einstein's... ."

A few weeks later I got from Anatolii Alexeevich Logunov the needed agreement, and the three volumes appeared in 1972 (including, for example, his *New methods of celestial mechanics*, his *Analysis situs*, topology books and articles, his works on automorphic functions (remembering that H. Poincaré had been quoted in the Larousse dictionary of about 1925 as "the author of Fuchsian functions"), and also his relativity papers).

This edition is accompanied by many comments on the present developments of Poincaré's ideas (written by the best modern experts), but no criticism of Einstein (perhaps expected by Logunov).

7. Averaging theory. It is interesting to know the relation between Bogolyubov's averaging theory and that of Poincaré. Nikolai Nikolaevich told me (and had published in his books) that while Poincaré had developed the averaging theory for the Hamilton differential equations (of celestial mechanics), Bogolyubov's goal was to extend this theory of Poincaré to general, non-Hamiltonian, dynamical systems.

In preparing the Russian edition of Poincaré's works, I discovered in his letters his own description of his averaging theory. He claimed that this theory had been developed earlier by the Swedish mathematician and astronomer Lindstedt, but that, upon trying to apply Lindstedt's general theory to the Hamilton differential equations of celestial mechanics, he observed some specific (symplectic in modern terms) properties of the Hamiltonian systems, and therefore he described the averaging theory for Hamiltonian systems as a specific theory, having its own goals and techniques.

I must say that the final version of Bogolyubov is clearer and easier in practical applications than Lindstedt's original general theory, whose Hamiltonian generalisation was published by Poincaré and was dis-Hamiltonised later by Bogolyubov (unaware, of course, of the works of Lindstedt).

The present theory of averaging of Hamiltonian systems is an enormous development of Poincaré's investigations (of what he had christened "the main problem of dynamics"). Kolmogorov's theorem on the persistence of invariant tori under small perturbations of integrable Hamiltonian systems (1954) is a very important example.

The last discoveries by M. Herman (a few months before he died) of the differences between the non-planar celestial mechanics of more than 3 bodies and the 3-body problem, which is the simplest non-integrable case studied by Poincaré, should be also mentioned, as well as Sevryuk's theorem (preceding Herman's discoveries) on applications of the same theory of Diophantine approximations on generic varieties, though Sevryuk did not use it in celestial mechanics applications, of which he was unaware.

The theory of Diophantine approximations appears in these problems because of the crucial influence of the resonances between the frequencies of the unperturbed problems on the evolution of the perturbations.

One of the first observed manifestations of these resonances is the approximate commensurability of the years of Saturn and Jupiter, the ratio of whose periods is approximately $5:2$ (Jupiter's angular motion is about $299''$ per day, and that of Saturn about $120''$).

The Poincaré averaging in the case of such a resonance leads to a large "secular perturbation", whose period is of order 10^3 years, but which is still periodic (like the oscillation of a pendulum) near the unperturbed motion. It leads to the evolution of the orbit in one direction during several centuries, which would destroy the solar system if continued forever. Fortunately, it goes in the opposite direction for the next several centuries, and the system remains planetary.

This interaction between the theory of dynamical systems and the statistics of Diophantine approximations was discovered by Poincaré, who used it as a basic tool in his works on celestial mechanics.

8. Kovalevskaya and Poincaré's non-integrability theorem. I have read recently in the *Encyclopedia of mathematical physics* that Poincaré was the author of the celebrated results of S. Kovalevskaya on the new integrable case of the problem of rotation of a heavy rigid body, also formulated by him. Both Poincaré's contribution and Kovalevskaya's discovery (for which Poincaré gave her an important prize of the Paris Academy of Sciences) are important, but I prefer to explain correctly what happened.

The problem had been formulated by Weierstrass, who suggested that his student Kovalevskaya should apply Poincaré's bifurcation theory of periodic orbits in celestial mechanics to prove the absence of any new analytic first integral in the problem of rotation of a heavy rigid body (where the previous integrable cases had been discovered and studied by Lagrange and Euler).

Kovalevskaya was completely unsuccessful: she discovered the impossibility of applying Poincaré's method to her problem. In trying to understand the reasons for her failure, she discovered that it is impossible to prove the conjecture of her teacher for the following reason: the conjecture is wrong, there exist more integrable cases.

Her success was much greater than if she had confirmed Weierstrass' conjecture: Kovalevskaya's case of integrability for the motion of a heavy rigid body is today the turning point of a large new important "complete integrability" theory of Hamiltonian systems, including such well-known models in mathematical physics as the Korteweg–de Vries, Schrödinger, and sine-Gordon equations, the Fermi–Pasta–Ulam numerical study of non-linear wave equations, and so on.

Poincaré never worked on these problems, at least he never mentioned his previous result in this direction when he evaluated the prize paper of Kovalevskaya.

What Poincaré had discovered in his works on non-integrability in celestial mechanics is an extremely important general theory, which he never published, as far as I know.

The main idea of Poincaré's non-integrability theorem is his description of the influence of resonances on the bifurcations of periodic orbits for small generic perturbations of integrable systems.

Namely, the peculiar property of the periodic orbits of the integrable Hamiltonian systems discovered by Poincaré is their appearance in continuous families together with neighbouring periodic orbits.

For the generic non-integrable systems the periodic orbits are isolated closed curves (on a constancy level of the Hamilton function). If one finds sufficiently many such isolated closed curves for some system, then non-integrability would follow.

In spite of this wonderful discovery of the topological difference between the integrable and non-integrable cases, Poincaré avoided proving it completely: he observed that a certain similar approximate property is already sufficient to prove the impossibility of new analytic first integrals, and therefore published only a detailed (and long) proof of this weaker result, rather than his great qualitative topological discovery, from which this weaker result originated.

Modern Russian mathematicians (especially V. V. Kozlov) have recently published a Poincaré-style proof of the fact that Kovalevskaya's case is the only case where Poincaré's method does not provide a proof of non-integrability. In this sense the suggestion of Weierstrass that Kovalevskaya should try to apply Poincaré's method was a good idea. However, it was done neither by Kovalevskaya nor by Poincaré (the influence of whose ideas on this whole area is still crucial).

Many proofs of impossibility in mathematics involve a deeper understanding of matters than a negative result on impossibility.

The Taylor series of the $\arctan x$ function diverges for $|x| > 1$, and one might prove this by evaluating the coefficients. However the *real reason* for divergence of the series is different: it is the singularity at the imaginary point $x = i$ of the derivative $1/(1 + x^2)$ of the arctangent function.

Similarly, Abel's theorem on the impossibility of solving algebraic equations of degree 5 by radicals is a topological fact. These equations are *topologically unsolvable*: no complex function of the same topological ramification as the root $x(a)$ of the algebraic equation $x^5 + ax + 1 = 0$, can be represented as a finite combination of radicals and univalent functions.

When proving that some simple behaviour is impossible, one should rather formulate topological qualitative reasons for the impossibility, as a positive statement about a complexity property of the behaviour of the object of study which makes it different from any representation whose impossibility one wishes to prove.

Knowing many examples of such "topological impossibility" results, I must mention that even Poincaré did not always formulate his results in this way, knowing (at least intuitively) much more than he stated explicitly.

As a sad example of this general situation I shall mention results on the topological impossibility of computing Abelian integrals of positive genus in elementary functions (say, of the elliptic integral

$$t(X) = \int_0^X \frac{dx}{\sqrt{x^3 + ax + b}}$$

or of the elliptic function $X(t)$: topological non-representability in terms of finite combinations of elementary functions).

In 1963 I attributed to Abel the proof of the fact that no complex function, topologically equivalent to $t(X)$ or to $X(t)$, is elementary. Unfortunately, Abel did not publish the proof (nor even the exact formulation) of this impossibility statement.

I hope that similar topological impossibility theorems will be published soon also for the integration of differential equations "in quadratures".

9. The statistics of continued fractions. Returning to the resonance studies in the works of Poincaré on bifurcations of periodic orbits, I shall mention also some of his non-mathematical results of great importance.

The statistics of the approximate commensurability of the periods of the motions in celestial mechanics provides serious difficulties in the study of the longtime behaviour of a perturbed system: will the Moon collide with the Earth? Will Jupiter cross the Earth's orbit?

The arithmetic statistics of random real numbers have been studied in Diophantine approximation theory. The simplest case is the description of continued fraction approximations

$$x = a_0 + \frac{1}{a_1 + \frac{1}{a_2 + \frac{1}{a_3 + \cdots}}}. \tag{4}$$

Here the question is what approximation $x \approx p/q$ of an irrational number x by a rational fraction p/q is possible if q is not too large? For example, the classical approximation

$$\pi \approx \frac{355}{113}$$

provides 6 digits of $\pi \approx 3.1415929\ldots$, and it is known that the continued fraction approximation (stopping at some a_k) provides the best approximation.

But to understand how good it is, one should know how large the "continued fraction elements" a_k are. Stopping before a large a_k, one obtains an excellent approximation

$$x \approx a_0 + \frac{1}{a_1 + \frac{1}{\cdots + \frac{1}{a_{k-1}}}}.$$

But are there large numbers a_k in the infinite continued fraction (4)?

For the golden ratio $x = \frac{\sqrt{5}+1}{2} = 1.6\ldots$ all the elements a_k are equal to 1.

The statistics of the values of the elements a_k for random x is known: the frequency p_n of the element n equals

$$p_n = \frac{1}{\ln 2} \ln\left(1 + \frac{1}{n(n+2)}\right) \tag{5}$$

(so more than 1/3 of the elements a_k are equal to 1).

Poincaré asked whether the observed statistics for the ratios of the measured periods of motions in celestial mechanics would be similar to these theoretical predictions (5).

The story of this problem is complicated. The above formula for p_n was known to Gauss, but he never published its proof. The astronomers, following Poincaré, have confirmed the similarity of the empirical observations to this prediction (H. Gyldén, "Quelques remarques relativement à la représentation des nombres irrationnels par des fractions continues", *C. R. Acad. Sci. Paris* **107** (1888), 1584–1587).

Later, the Swedish mathematician Wiman published a 250-page article on this problem (A. Wiman, "Über eine Warscheinlichkeitsaufgabe bei Kettenbruchentwickelungen", *Acad. Förh. Stockholm* **57** (1900), 589–841), but I was unable to understand whether he proved (5), so long is his paper.

The first known proof of formula (5) was published by R. O. Kuz'min in 1928. It is well explained in the nice book by Khinchin (A. Khinchin, *Continued fractions*, Nauka, Moscow 1978). Khinchin's version is based on the Birkhoff ergodic theorem. But Birkhoff's proof of this theorem (suggested already by Boltzmann and Poincaré) appeared later than Kuz'min's article.

Therefore, one should reconsider the papers of Wiman (1900) and Kuz'min (1928) — do they contain a proof of the ergodic theorem? They needed it for the system $x \mapsto$ (fractional part of $1/x$).

Gauss had observed the invariant measure $\int \frac{dx}{1+x}$ for this map $A\colon (0,1) \to (0,1)$. Invariance of a measure under the map A is the identity $\mathrm{meas}(A^{-1}M) = \mathrm{meas}(M)$ for any measurable set M. Formula (5) corresponds to the set $\dfrac{1}{n+1} \leqslant x < \dfrac{1}{n}$, where the integer part of $1/x$ is n.

It is well known that Birkhoff's proof of the ergodic theorem was a byproduct of his examination of von Neumann's weaker version of it; it would be interesting to understand its relations to the works of Poincaré, Wiman and Kuz'min on the statistics of continued fractions.

10. Poincaré's last geometric theorem. Among many other interesting ideas of Poincaré, I shall mention his "last geometric theorem". The modern formulation of this basic result of symplectic topology was not formally published by Poincaré, whose paper contains instead the main ideas of a Morse-theoretic proof of it. Understanding well that these ideas were insufficient for a rigorous proof, he claimed only that he had verified the result in several hundreds of particular cases, and that he was leaving the search for a full proof to the coming generations of mathematicians.

The simplest conjecture of Poincaré in this area was proved a few years after his death by G. D. Birkhoff: an area-preserving map of a plane annulus onto itself that rotates the boundary circles in opposite directions has at least 2 fixed points.

The general form (still unproved, as far as I know, in its full generality, but verified for many hundreds of examples) replaces the annulus by a compact closed symplectic manifold (the symplectic structure being a closed non-degenerate exterior differential 2-form on a $2n$-dimensional smooth manifold).

The area preservation condition and the boundary rotation condition are replaced in the general case by the following description of the map of the symplectic manifold M^{2n} onto itself. It should be the time-one diffeomorphism of the phase flow $g^t\colon M^{2n} \to M^{2n}$ defined by a Hamiltonian vector field v on M^{2n}:

$$\frac{dg^t(x)}{dt} = v(g^t(x)), \qquad g^0(x) = x.$$

I recall that a Hamiltonian vector field v on a symplectic manifold M^{2n} (with the symplectic structure ω) is determined by a smooth Hamilton function $H\colon M^{2n} \to \mathbb{R}$ by the formula

$$\omega(v(x), w) = -dH(w)$$

for any tangent vector w of the manifold M^{2n} at the same point x:

$$v(x) \in T_x M^{2n}, \qquad w \in T_x M^{2n}.$$

In the classical case (of the "Darboux coordinates" p and q) the symplectic structure is $\omega = dp \wedge dq$ and the Hamiltonian vector field defines the Hamilton differential equations:

$$\frac{dp}{dt} = -\frac{\partial H}{\partial q}\,, \qquad \frac{dq}{dt} = \frac{\partial H}{\partial p}\,.$$

Poincaré's rotation condition is represented in the above general case by the condition that the Hamilton function $H(p,q;t)$ (depending on the time variable t as a parameter) is a univalued function, rather than a differential form.

The torus translation corresponding to $H(p,q) = p$ is a counterexample to the existence of the fixed points (for $g^t(p,q) = (p, q+t)$).

The generalised "Poincaré's Last Geometric Theorem" claims the existence of at least m fixed points of the map $g^t\colon M^{2n} \to M^{2n}$ determined by a univalued Hamilton function $H(p,q;t)$, where $m(M^{2n})$ is the Morse number (being the minimal number of critical points of a smooth function on M^{2n}).

One might consider here either generic (non-degenerate) Morse functions and maps g, or one might admit arbitrary degeneration (both of the functions in the definition of the Morse number m and of the fixed points of the symplectomorphism g). The conjecture is probably true in both cases (while the two statements do not follow one from the other).

Poincaré's original case of an annulus is very close to the case of the 2-torus $M^2 = S^1 \times S^1$, for which $m = 4$ (the torus surface representing both sides of the two-sided annulus).

In this case Poincaré's conjecture was proved by Birkhoff. Later Arnol'd proved it for maps g which are not too far from the identity map (on any closed symplectic manifold).

Conley and Zehnder then proved the conjecture for the $2n$-torus $M^{2n} = (S^1)^{2n}$ (where the Morse number is $m = 2^{2n}$, counting the critical points and fixed points with their natural multiplicities).

Floer extended these results to many Kählerian manifolds (including the genus g surfaces, where the Morse number is $m = 2g + 2$).

There are many useful particular cases (including products of the preceding manifolds) where the conjecture has been recently proved (mostly using some Floer-type versions of quantum field theory).

In spite of announcements of proofs of the general conjecture, I am unaware of any successful proof of it. By the way, in most papers the upper estimate of the number of fixed points by the Morse number is replaced by the inequality

$$\text{(number of the fixed points)} \leqslant \Big(b_* = \sum b_i\Big)$$

in terms of the Betti numbers b_i.

The Morse theory inequality $m \geqslant b_*$ is well known, and $m = b_*$ in many examples (like tori and the surfaces mentioned above). However the general "Poincaré's Last Geometric Theorem" conjecture

$$\text{(number of fixed points)} \leqslant \text{(Morse number } m\text{)}$$

is stronger than the above upper estimate in terms of Betti numbers and should not be mixed up with it.

11. Perturbation theory and symplectic topology. Returning to Poincaré's perturbation theory, which led to all these results in symplectic topology, I shall mention one more forgotten corollary of his approach.

The influence of a simple resonance on the first approximation of the perturbation theory leads, according to the Poincaré averaging, to a generalised pendulum "equation of slow and small oscillations near the resonance". It is a Lagrangian natural mechanical system whose configuration space is a circle (of the slowly varying resonant phase), the potential energy being a smooth function on this circle and the kinetic energy having the standard form ap^2 (for some constant a).

This "generalised pendulum" equation can be easily integrated, providing a nice description of the resonant events (at time scales of order at least $\sqrt{1/\varepsilon}$ for small perturbations of order ε).

A similar Poincaré-type problem for the intersection of two resonant zones is far from being investigated, in spite of its extreme importance for understanding the influence of resonances on the slow evolution of perturbed systems (with more than 2 degrees of freedom).

Namely, the "pendulum" equation is replaced in the 2-resonance case by a Lagrangian dynamical system whose configuration space is the 2-torus $T^2 = S^1 \times S^1$. The potential energy is a smooth function $U\colon T^2 \to \mathbb{R}$.

The kinetic energy is a translation-invariant quadratic form of the tangent vectors of the torus. It can be written, for suitable coordinates q_1, q_2 on the torus, in the form $a_1\dot{q}_1^2 + a_2\dot{q}_2^2$, which may have an arbitrary signature, depending on the system which we are perturbing.

The above coordinates q_1 and q_2 are not, in general, the standard angular coordinates on the torus: the torus is described in these terms as

$$T^2 = \mathbb{R}^2/(\omega_1\mathbb{Z} + \omega_2\mathbb{Z}),$$

for some linearly independent vectors ω_1, ω_2 in the plane $\mathbb{R}^2$ with coordinates q_1, q_2.

In some cases the kinetic energy is positive definite. In such cases one understands many geometric properties of the generalised pendulum equation (even when it is not integrable), using the topological methods of global variational calculus.

For instance, there exist closed orbits in any homotopy class of closed curves on the torus (parallel, meridian and so on), namely, the shortest curve for the Jacobi–Maupertuis metric is such a closed orbit. One is also able to find "homoclinic and heteroclinic" orbits of Poincaré (approaching asymptotically a periodic orbit or two periodic orbits as $t \to +\infty$ and as $t \to -\infty$).

These topological results are very useful for studying the evolution due to the resonances, but they are unfortunately missing in the case of the Lorentzian metrics, where $a_1 a_2 < 0$.

One might formulate the first questions in this direction as follows.

1) Does there exist (generically) a periodic orbit in any homotopy class of closed curves on the torus (or at least in most classes, taking into account the possible variants of the Diophantine approximation properties of the two light-directions where the kinetic energy vanishes, with respect to the lattice generated by ω_1 and ω_2)?

2) Does there exist (generically) a heteroclinic connecting orbit between two given homotopy classes of asymptotic closed orbits for $t \to +\infty$ and $t \to -\infty$?

Both the theoreticians and the authors with practical experience have claimed many times that there should be more instability (and faster "Arnol'd diffusion") in the Lorentzian (hyperbolic) case than in the Riemannian (elliptic) case $a_1 a_2 > 0$.

However, this (natural) conjecture has never been proved, and one is still waiting for Floer-type homologies and periodic-orbit theorems in the Lorentzian metric cases.

12. Fruitful mistakes. In preparing the Russian edition of Poincaré's collected works, I was obliged to discuss also his mistakes (and the resulting development in several branches of mathematics).

I shall include in this survey only the two well-known cases: the non-integrability question in the 3-body problem and the Poincaré conjecture on S^3.

The Swedish king Oscar II formulated a crucial problem of celestial mechanics: knowing or supposing the divergence of the series of perturbation theory, prove the non-existence of converging approximations in the 3-body problem (for the infinite time interval where the motion should be approximated).

Poincaré got the prize for his proof of the non-existence of a new analytic first integral (in the domain of the phase space corresponding to the perturbed Keplerian elliptic motions).

But these results of Poincaré contradicted the Sundman theory of regularisation of collisions. Namely, this theory implies the analytic dependence of the solutions on the initial conditions in some domain of the complex time t axis containing the whole real time axis (with a neighbourhood of diminishing radius for large $|t|$).

By the Riemann theorem this neighbourhood is a complex diffeomorphic image of the (open) unit disc. Parametrising it by this disc, one obtains a representation of the solutions by functions holomorphic in the disc. Their Taylor series converge inside the disc, providing a convergent series approximation of the initial solution along the whole real time axis.

The contradiction does not invalidate any of Poincaré's non-integrability results or proofs. Simply the absence of new analytic integrals does not imply the answer to the prize problem: it invalidates **some** perturbation theories, rather than the existence of **any** convergent series representation of the solution.

Understanding this, Poincaré spent his prize to buy all the copies of his article in *Acta Mathematika* containing the prize result, so that he could rewrite it, have it printed again, and send the new copies to all the subscribers and libraries.

The resulting new version later became the celebrated *New methods of celestial mechanics*. Seeing the story today, I understand that the non-integrability results of Poincaré (and especially his ideas leading to these results, published by him only partially) were far more important than the formal "binary problem" of the Swedish king.

Fortunately, all these famous problems, degrees and prizes (including even the Hilbert problems, the Nobel Prizes and the Fields medals) have had little influence on the development of the science, and the works of, say, Weyl and Morse, Leray and Whitney, Kolmogorov and Pontryagin, Petrovskii and Turing, and Shannon and Moser represent the best of 20th century mathematics, in spite of the absence of these names on the Fields list.

The "Poincaré conjecture" was proved by Poincaré as a theorem. However, later he observed that some of his lemmas were wrong. His mistake lay in confusing homology and homotopy (for curves).

The results of this serious mistake were wonderful. First, Poincaré created both homology and homotopy theories, carefully distinguishing them. For example, his descriptions of monodromy and the theory of automorphic functions are clearly homotopical, depending on the highly non-commutative properties of the fundamental group.

On the other hand, his studies of the ramifications of multiple integrals (known today as the "Picard–Lefschetz theory" and as the "Gauss–Manin connection"), especially in his works on the asymptotic expansions of perturbing functions in celestial mechanics, are purely homological (or even cohomological) works, as well as his transformation of the "Kronecker characteristic" (generalising the "Sturm characteristics") into the notions of the index of a singular point of a vector field, the degree of a map, intersection rings and linking theory.

Poincaré himself constructed a counterexample to his wrong statement (claiming that a homology sphere is homeomorphic to the true sphere), which he associated with the dodecahedron and which might be written today in the form of the Brieskorn sphere E_8 in $\mathbb{C}^3$:

$$x^3 + y^5 + z^2 = 0, \qquad |x|^2 + |y|^2 + |z^2| = 1.$$

This exotic (homology) sphere of Poincaré is a predecessor of the 28 Milnor spheres, which are smooth manifolds homeomorphic to the usual 7-sphere S^7 but pairwise non-diffeomorphic (and hence 27 of them are not diffeomorphic to S^7).

Each Milnor sphere is defined in $\mathbb{C}^5$ by the system of 3 real equations,

$$x^{6k-1} + y^3 + u^2 + v^2 + w^2 = 0,$$
$$|x|^2 + |y|^2 + |u|^2 + |v|^2 + |w|^2 = 1.$$

To get the 28 exotic spheres one should take $k = 1, 2, \dots, 28$. Exactly one of the choices produces a manifold strangely diffeomorphic to the usual sphere S^7.

The corrected version of Poincaré's conjecture states that any closed simply connected 3-manifold is homeomorphic to the 3-sphere.

The corresponding characterisation of the 2-sphere follows from the classification of surfaces.

Starting from dimension 5 one should add to the "simply connected" condition $\pi_1(M^3) = 0$ the conditions that the higher homotopy groups vanish: $\pi_k(M^n) = 0$ for all $k < n$. In this case the manifold is homeomorphic to the sphere S^n ("Smale's theorem").

So the mild dimensions ($n = 3$ and 4) remain the most difficult cases of the Poincaré problem.

It was announced that the corrected Poincaré conjecture for the sphere S^3 was proved recently by G. Perel'man.

In a Russian official scientific newspaper his result was formulated in the following way: "Poincaré proved that any closed path on the two-sphere can be deformed to the trivial loop of a single point while remaining on the two-sphere. The celebrated Poincaré problem was to prove that this statement is still true for the three-dimensional sphere S^3. Our young mathematician G. Perel'man has recently proved it."

I think that we should write correct descriptions of what is happening, otherwise the image of mathematics and mathematicians in the eyes of the general public would be too negative.

One of the last corollaries of the Poincaré conjecture that I have seen in the library is the following theorem (M. Eisermann, "Vassiliev invariants and the Poincaré conjecture", *Topology* **43**:5 (2004), 1211–1230): the Poincaré conjecture would follow from the statement that the Vassiliev invariants of knots distinguish any two different knots.

I hope that the reader has seen many knots and does understand the difficult mathematical problem of the classification of knots.

In fact, this mathematical problem had been first formulated explicitly by a physicist, Sir Thompson, Lord Kelvin. His idea had been to explain the Mendeleev periodic table of chemical elements by some microscopic geometric structure inside the nuclei of atoms.

Trying to choose a convenient discrete structure, he suggested supposing that it is a small knot, whose geometric and topological properties are responsible for the chemical peculiarities of different atoms. So he started to classify knots (studying plane projections of them with few self-intersections of the projected closed curve).

Even to understand, upon looking at two projections, whether they might represent the same knot (that is, where one closed space curve can be transformed continuously into the other while remaining free of self-intersections during the deformation) is a difficult task: such combinatorial problems are close to the so-called *algorithmically unsolvable problems* (a celebrated example of an algorithmically unsolvable problem is the problem of recognising whether a given finite system of polynomial equations with integer coefficients has an integer solution).

For the distinction of knots people invented *knot invariants*: characteristics of the projections which are algorithmically computable and which take equal values on any two representations of the same knot.

The *Vassiliev invariants* are special knot invariants whose position in the space of arbitrary knot invariants is similar to the position of the polynomials in the space of arbitrary functions.

These invariants are closely related to such branches of mathematics as singularity theory, complex integration theory, graph theory, the theory of configuration spaces, Lie algebras and quantum field theory.

They represented a happy part of the almost uncomputable invariants theory of knots, but the general pessimistic opinion has been that, for just this reason, they form too small a part of the complicated world of invariants, insufficient for the goal of distinguishing different knots.

This new and highly unexpected application of the topological ideas of Poincaré is restoring the priority of the simplest things: in spite of their unsophisticated nature, the Vassiliev invariants (invented only 15 years ago) are universal. One hopes that they contain all the knot invariants (in the sense that any invariant is a function of the simplest Vassiliev invariants).

This result would never have been possible without the Poincaré conjecture, and thus without Poincaré's mistake (of confusing homotopy with homology) which produced, in the end, these wonderful corollaries.

I think, in general, that the mistakes form an extremely important part of scientific activity; their role is sometimes greater than that of the formal proofs and dull axioms. One should study the histories of the mistakes of previous generations of scientists, using their experiences as instructive examples and as sources of new discoveries. The mistakes of the greatest persons are the most useful.

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