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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This paper describes a number of published and unpublished works of Henri Poincaré that await continuation by the next generations of mathematicians: works on celestial mechanics, on topology, on the theory of chaos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 functions and the connection between the Poincaré conjecture and the theory of knot invariants.

MIRAMARE – TRIESTE

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The list of the 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 of natural sciences, rather than the art of symbols permutations.

Describing the mathematical problems (some years before the celebrated Hilbert's 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 the variations of the problem (like, say, the variation 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 the conformity to any reality is unrelated to the science, which is the art of the deductions of the corollaries of arbitrary axioms).

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

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

These contributions of Poincaré are mostly unnoticed by the historians of science and I shall describe only few particular cases.

Bifurcation theory

Poincaré's Thesis contained the versal deformation theorem (called by him "Lemma 4") for the 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 mostly attributed to Grothendieck, to Malgrange and to Thom.

Grothendieck and Thom studied this problem for many years in their neighbouring offices at IHES, Bures-sur-Yvette. They never communicated to each other and the relation of 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 had not succeeded, he persuaded B. 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 B. Malgrange proved Thom's conjecture, which is now the celebrated "Malgrange Theorem", basic for the whole 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 the simultaneous translation of Malgrange's talk on his results at the meeting of the International Congress of Mathematicians (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 have not yet been pronounced in my talk".

There was no written text to translate, but he guessed correctly: the words mentioned in my "simultaneous" translation appeared in his talk few minutes later.

Anyway, Poincaré's bifurcation theory was elaborated by the Russian mathematicians Pontryagin and Andronov already in the 20's and in the 30's (due to the need to apply these bifurcations to radiophysics).

Andronov published (with all the proofs) the theory of the birth of a periodic motion of a dynamical system under the 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 attraction domain, diminishing like the square root of the difference between the growing parameter value and the future value, at which the stability will be destroyed, disappears at the stability loss moment.

The first case is called the mild stability loss, the new-born periodic motion-attractor describes a small oscillation near the old stationary regime. The second case is called the hard stability loss, the behaviour of the system after this stability loss being very far from the equilibrium, losing its stability.

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

Poincaré's versal deformation Lemma 4 provided an estimation 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 the degeneration of the principal part). Their estimations 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 might eliminate all the complex variables theory from these

bifurcation studies, proving the corresponding theorems on the birth of periodic motions for the smooth dynamical systems and Andronov used his results.

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

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

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

It has a first integral, provided that the coefficients of the right hand side verify some algebraic equation (a particular example is the 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 to study the first integral variation, produced by the variation of the dynamical system

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

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

$$\delta H(h) = \int_{H(x,y)=h} ((\partial H/\partial x)\varepsilon f + (\partial H/\partial y)\varepsilon g) dt \quad (3)$$

(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 the zeroes h of the equation $\delta H(h) = 0$: Is it bounded for the generic perturbations $\varepsilon f, \varepsilon g$?

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

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

H being 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 the 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 degree 2 polynomials (1).

Cohomology theory

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

Kolmogorov, inventing the general cohomology theory in his 4 short Notes in the C.R.A.S., Paris, 1935, stated that his main inspiration came from Gunter's "theory of functions of domains" (which term was used by Gunter to describe his version of the "distributions theory" or of the "generalised functions theory", used by him before the twenties to obtain the existence and uniqueness theorems for the differential equations of hydrodynamics).

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

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

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

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

Sobolev equations

The modern attributions of the mathematical discoveries are usually following the name of the latest person (America is never called Columbia). L. Schwartz told me that Sobolev made a serious mistake, publishing his great discoveries (on the generalised solutions of the partial derivatives differential equations) in a provincial journal, using an infrequent language, and that the main contribution of Schwartz himself to this theory was the translation of Sobolev's results (published by Sobolev in French at C.R.A.Sci., Paris) to the English edition in a readable place. Sobolev told me that Schwartz made more, but I would like to repeat Kolmogorov's words that the contributions of Poincaré and of Gunter (whose student was Sobolev) should not be forgotten.

In the end of 1950 Sobolev explained me his classified results on the oscillations of the fluid content 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 the rotating planets, in meteorology. At present this Poincaré-Sobolev theory is applied mostly to the theories of Jupiter and Venus atmospheres (in the case of the Earth they are also important, providing the cyclonic activity waves, due to the Earth rotation).

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, the original Sobolev's paper being classified.

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

Relativity principle

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

Poincaré had published 10 years earlier, in 1895, a philosophical journal paper "on the time measurement", in which he had clearly explained that the Galilean or Newtonian absolute space and absolute time notions have no empiric definitions, the simultaneity being explicitly dependent on the clocks synchronisation convention.

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

In his paper Poincaré avoided the mathematical formulas (well-known to him), trying to be understood by the philosophers, having no mathematical backgrounds.

Minkovsky, being the teacher of Einstein and a friend of Poincaré, had early suggested to Einstein to study Poincaré's theory, and Einstein did it (never referring to this reading until some 1945 article).

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

Lorentz transformations

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

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

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

Calculating more, Poincaré proved that the expected “symmetries” were not preserving the Maxwell equations. At the end he decided to solve by himself the symmetries problem. The resulting Lie group (and the Lie algebra of the infinitesimal symmetries) were included by Poincaré in his course.

Publishing his lectures, Poincaré chosen for these newly-discovered transformations the name “Lorentz transformations”, known today to anybody.

It is similarly interesting to know that the “Stokes Lemma”, basic both for the cohomology theory and for the electromagnetic field Maxwell theory, was never invented or proved by Stokes. Namely, its discoverer was sir Thompson, lord Kelvin (Stokes had transmitted Thompson’s result to Cambridge Tripos Commite, and Maxwell, being there a student, called it “Stokes Lemma” due to this circumstance).

Strangely enough, I plagiarised Poincaré’s manners, who attributed his result to Lorentz, inventing the names for the “Maslov index” and for the “Gudkov’s conjecture” in symplectic topology and in real algebraic geometry.

Maslov told me that the integer named “Maslov index” in my report of his thesis should not be related to him, because only its residue modulo 4 has physical importance in the quasi-classical theories, while the integer number invented by me is useless.

Gudkov suggested that the conjecture (on the divisibility by 16 of some topologic invariant of real plane algebraic curves) that I attributed to him in my review of his thesis, was not conjectured by him, since he was aware of some counter-examples.

Insisting on the relation of this conjecture to both the differential topology of the 4-manifolds and to quantum fields topological theories, I persuaded Gudkov that his counter-examples were wrong and at present the Gudkov conjecture, proved by Rokhlin, is one of the main results of real algebraic geometry. This science started from Hilbert’s 16-th 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, there are 3 arrangements (and the Gudkov conjecture implied the absence of any other).

It is interesting to know that only two problems (the 13-th and the 16-th) of Hilbert’s list, considered by him as a testament of the XIXth century to the XXth century, are related to topology, which has been the most active part of mathematics in the XXth century.

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

In this problem the number of topologically possible arrangements equals 268282855. Gudkov conjecture and other restrictions, known today, reduce this number to about 90 cases. The number of known examples exceeds today 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 ellipses and hyperbolas theories and much more important than, say, the Fermat binary problem).

Publishing Poincaré in Russian

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

About 1970 I had proposed to the Moscow Academy of Sciences Editorial Board “Classics” to translate the main works of Poincaré to Russian. Unfortunately, the answer I got from the Academician Logunov, former student of Bogolyubov –and at the time chief editor of the “Classics”– 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 the Russian edition of any work of Poincaré is impossible.

My friends suggested to me the way to overcome Logunov. They claimed: The chief of the Mathematics Department of the Academy, Bogolyubov, has a very positive opinion on the works of Poincaré (which he extended himself in his papers on averaging theory). He has also a very positive opinion on Arnold (having published a book, extending Arnold’s result). 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 Vorobiev Gory. There, having read Logunov’s letter, he said the following clever words.

All three of us –he told me–, Poincaré, myself and you, are not restricted to be mathematicians, we are also physicists and even natural scientists.

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

I shall show you now –he continued– how to use for the progress of the science one different disastrous phenomenon: the anti-Einsteinism and antisemitism of some particular persons.

Pronouncing these words he took a sheet of white paper, headed with all his degrees: senior member of the Academy of Sciences, director of the Joint Institute of Nuclear Physics and so on. And he wrote:

“Dear Anatolyi Alexeevich,
we are proposing together, with professors Arnold and Oleinik, to the “Classics” of the Academy a project of the Collected Works of Poincaré in 3 large volumes, including in it his relativity papers, published by him preceding the Einstein’s ones...”.

Few weeks later I got from Anatolyi Alexeevich Logunov the needed agreement, and 3 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 critics to Einstein (expected, perhaps, by Logunov).

Averaging theory

It is interesting to know the relation of Bogolyubov's averaging theory to 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 Poincaré's theory to the general, non-Hamiltonian, dynamical systems.

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 a Swedish mathematician and astronomer Lindstedt, but that, trying to apply the general theory of Lindstedt to the Hamilton differential equations of celestial mechanics, Poincaré observed some specific (symplectic in modern terms) properties of the Hamiltonian systems, and therefore described the Hamiltonian systems averaging theory as a specific theory, having its own goals and techniques.

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

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

The latest discoveries by M. Herman (few months before he died) of the differences between the non planar celestial mechanics of more than 3 bodies and of 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 the applications of the same Diophantine theory of approximations on generic varieties, missing its celestial mechanics applications, unknown to Sevryuk.

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

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

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

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

Kovalevskaya and non-integrability Poincaré Theorem

I have read recently in an Encyclopedia of Mathematical Physics that Poincaré was the author

of the celebrated results of S. Kovalevskaya on the new integrable case of the heavy rigid body rotations problem, formulated also by him. Both the Poincaré contribution and the Kovalevskaya 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 to his student Kovalevskaya to apply Poincaré's bifurcation theory of periodic orbits in celestial mechanics to prove the absence of any new analytic first integral in the heavy rigid body rotation problem (where the previous integrable cases had been discovered and studied by Lagrange and by Euler).

Kovalevskaya was completely unsuccessful: she discovered the impossibility of the application of the Poincaré method to her problem. Trying to understand the reasons of her failure, she discovered that it is impossible to prove the conjecture of her teacher for the following reason: the conjecture is wrong, there exists more integrable cases.

Her success was much greater than would be the confirmation of the Weierstrass conjecture: Kovalevskaya's case of the heavy rigid body motions integrability is today the turning point of a large new important "complete integrability" theory of the Hamiltonian systems, including such celebrated model in mathematical physics as the Korteweg-de-Vries and Schroedinger sine-Gordon equation, Fermi-Pasta-Ulam numerical study of nonlinear wave equations and so on.

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

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

The main idea of Poincaré's non-integrability theorem is his description of the resonances influence on the periodic orbits bifurcation 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 the neighbouring periodic orbits.

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

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

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

Many impossibility proofs in mathematics are covering more deep understanding of the events than the negative result on the impossibility.

The Taylor series of the arctan x function is diverging for $|x| > 1$, and one might prove it, evaluating the coefficients.

However the genuine *reason* of the series divergence 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 the solution by radicals of the algebraic equations of degree 5 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 the radicals and univalent function.

Proving the impossibility (of some simple behaviour) one should rather formulate the topological qualitative reasons of it, as a positive statement on the complexity property of the behaviour of the object of study, which makes it different from any representation, whose impossibility one wished to prove.

Knowing many examples of such "topological impossibility" results, I must mention that even Poincaré missed to formulate his results in this way, knowing (at least intuitively) much more than he formulated explicitly.

As a sad example of this general situation I shall mention the results on the topological impossibility of elementary functions integration of the Abelian integrals of positive genus (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 irrepresentability in terms of the finite combinations of elementary functions).

I attributed in 1963 to Abel the proof of the fact that no complex function, topologically equivalent to $t(X)$ or to $X(t)$, is elementary. Unfortunately, Abel had not published the proof (and even the exact formulation) of this impossibility statement.

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

The continued fraction statistics

Returning to the resonances studies in the works of Poincaré on periodic orbits bifurcations, I shall mention also some of his non-mathematical results of great importance.

The statistics of the approximated commensurability of the periods of the events in celestial mechanics provides serious difficulties in the study of the long time behaviour of the perturbed system: will the Moon collide with the Earth? Will Jupiter cross the Earth's orbit?

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

$$x = a_0 + \frac{1}{a_1 + \frac{1}{a_2 + \frac{1}{a_3 + \dots}}} \quad (4)$$

The question is here, what approximation $x \approx p/q$ of an irrational number x by the rational fraction p/q is possible, if q is not too large?

Say, the classic approximation

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

provides 6 digits of $\pi \approx 3,1415929\dots$, 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 are the “continued fractions elements” a_n . Stopping before a large a_n one obtains an excellent approximation

$$x \approx a_0 + \frac{1}{a_1 + \frac{1}{a_2 + \dots + \frac{1}{a_{n-1}}}}$$

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

For the golden ratio $x = \frac{\sqrt{5}+1}{2} = 1,6\dots$ 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

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

(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 would be similar to these theoretic 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, [1].

Later the Swedish mathematician A. Wiman published a 250 pages long article [2] on this problem, but I am unable to understand, whether he proved formula (5), so long is his paper.

The first known proof of formula (5) was published by R.O. Kuzmin in 1928. It is well explained in the nice book by A. Khinchin [3]. Khinchin’s version is based on the Birkhoff ergodic theorem. But Birkhoff’s proof of this theorem (suggested already by Boltzman and Poincaré) appeared later than Kuzmin’s article.

Therefore, one should reconsider the papers of Wiman (1900) and Kuzmin (1928)-do they contain the proof of the ergodic theorem? They need it for the system $x \rightarrow$ (fractional part of $1/x$).

Gauss had observed the invariant measure $\int \frac{dx}{1+x}$ for this mapping $A : (0,1) \rightarrow (0,1)$. The measure invariance under a mapping A is the identity $\text{mes}(A^{-1}M) = \text{mes}(M)$ for any measurable set M .

Formula (5) corresponds to the set $\frac{1}{n+1} \leq x < \frac{1}{n}$, 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 collaboration to the Von Neuman’s weaker version of it; it would be interesting to understand its relations to the works of Poincaré, Wiman and Kuzmin on the continued fractions statistics.

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 its Morse-theoretical proof. Understanding well that these ideas are insufficient for the strict proof, he claimed only that he had verified the result in several hundreds of particular cases, and that he leaves the search of the full proof to the coming generations of mathematicians.

The simplest conjecture of Poincaré in this domain was proved few years after his death by G.D. Birkhoff: an area-preserving mapping of a plane annulus onto itself, turning the opposite 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 hundred examples) replaces the circular ring by a compact closed symplectic manifold (the symplectic structure being a nowhere degenerated exterior differential 2-form on an $2n$ -dimensional smooth manifold).

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

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

I recall that a Hamiltonian vector-field v on a symplectic manifold M^{2n} (with the symplectic structure ω) is defined by a smooth Hamilton function $H : M^{2n} \rightarrow R$:

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

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

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

In the classical case (of the “Darboux coordinates” p, q) the symplectic structure is $\omega = dp \wedge dq$ and the Hamilton vector-field is defining the Hamilton differential equations:

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

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 uniform function, rather than the differential form.

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

The generalised “Poincaré Last Geometric Theorem” claims the existence of at least m fixed points of the mapping $g^t : M^{2n} \rightarrow M^{2n}$, defined by a uniform Hamilton function $H(p, q; t)$, where $m(M^{2n})$ is the Morse number (being the minimal number of the critical points of a smooth function on M^{2n}).

One might consider here either the generic (non-degenerate) Morse functions and mappings g or one might admit the arbitrarily degenerations (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 are not following one from the other).

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

In this case Poincaré's conjecture was proved by G.D. Birkhoff. Later Arnold proved it for the mappings g , which are not too far from the identity mapping (on any closed symplectic manifold).

Then Conley and Zehnder proved the conjecture for the $2n$ -torus $M^{2n} = (\mathbb{S}^1)^{2n}$ (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 the products of the preceding ones), where the conjecture has been recently proved (mostly - using some Floer type versions of the quantum fields theory).

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

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

in terms of the Betti numbers b_i .

The Morse theory inequality $m \geq b_*$ is well known, and $m = b_*$ in many examples (like the tori and the surfaces, mentioned above).

However the general "Poincaré Last Geometric Theorem" conjecture

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

is stronger than the above Betti numbers involving minoration and should be not mixed with it.

Perturbation theory and symplectic topology

Returning to the Poincaré perturbation theory, leading to all these symplectic topology results, I shall mention one more forgotten corollary of Poincaré's approach.

The influence of the simple resonance first approximation on perturbation theory leads, according to the Poincaré averaging, to some generalised pendulum "equation of slow oscillations near the resonance". It is a Lagrangian natural mechanical system, whose configuration space is a circle (of the slow 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" equations can be easily integrated, providing a nice description of the resonant events (at the time scales of order at least $\sqrt{1/\varepsilon}$ for the small perturbations of order ε).

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

Namely, the “pendulum” equation is replaced in the 2-resonances case by a Lagrangian dynamical system whose configuration space is the two-torus $T^2 = \mathbb{S}^1 \times \mathbb{S}^1$. The potential energy is a smooth function $U : T^2 \rightarrow \mathbb{R}$.

The kinetic energy is a quadratic form invariant by translations 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

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

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

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

For instance, there exist closed orbits in any homotopy class of the closed curves on the torus (parallel, meridian and so on), namely, the shortest curve of the Jacobi-Maupertuis metrics 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 for the time $t \rightarrow +\infty$ and $t \rightarrow -\infty$).

These topological results are very useful for study of 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 in the following way: 1) does there exist (generically) a periodic orbit in any homotopy class of the 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 kinematic energy vanishes, with the 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 the asymptotic closed orbits for $t \rightarrow +\infty$ and $t \rightarrow -\infty$?

Both the theoreticians and the practical experiences authors claimed many times, that there should be more instability (and faster “Arnold diffusion”) in the Lorentzian (hyperbolic) case, comparatively with the Riemannian (elliptic) case $a_1 a_2 > 0$.

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

Fruitful mistakes

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 3-body problem non-integrability question and the Poincaré conjecture on \mathbb{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 nonexistence of the converging approximations (for the infinite time interval, where the motion should be approximated).

Poincaré got the Prize for his proof of the nonexistence of new analytical first integral (in the domains of the phase space, corresponding to the perturbed Keplerian elliptic motions).

But these results of Poincaré were contradicting the Sundman theory of collisions regularisation. Namely, this theory implies the analytical 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 the representation of the solutions by the holomorphic functions inside this 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 the Poincaré's non-integrability result or proof. Simply the absence of new analytical 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 Mathematica", containing the prized result, to rewrite and to print it and to send the new copies to all the subscribers and libraries.

The resulting new version became later 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 by far more important than the formal "binary problem" of the Swedish king.

Fortunately, all these celebrated problems, degrees and prizes (including even the Hilbert problems, the Nobel Prizes and the Fields medals) provide few influence on the sciences development, and the works of, say, H. Weyl and M. Morse, J. Leray and H. Whitney, Kolmogorov and Pontryagin, Petrovsky and Turing, Schanone and Moser are representing the best XXth century mathematics in spite of the absence of these names in the Fields' list.

The "Poincaré conjecture" was proved by Poincaré as a theorem. However, later he observed that some of his lemmas were wrong. The mistake had been the mixing of homology and of homotopy (for curves).

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

On the other side, his studies on the ramifications of the multiple integrals (known today as "Picard-Lefschetz theory" and as "Gauß-Manin connection"), especially in his works on the asymptotic expansions of the perturbing functions in celestial mechanics, are purely homological (or even cohomological) works, as well as his transformation of the "Kronecker's characteristic" (generalising the "Sturm characteristics") to the notions of the index of a vector-field singular point, to the degree of a mapping, to intersections ring and to linking theory.

Poincaré constructed the counter-example to his wrong statement (claiming that a homological

sphere is homeomorphic to the true sphere), which he associated to the dodecahedron and which might be written today in the form of the Brieskorn sphere E_8 in C^3 : $x^3 + y^5 + z^2 = 0$, $|x|^2 + |y|^2 + |z|^2 = 1$.

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

Each Milnor's sphere is defined in 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 providing the 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 2-sphere characterisation follows from the surfaces classification.

Starting from dimension 5 one should add, to the "simply-connected" condition $\pi_1(M^3) = 0$, the higher homotopy groups vanishing conditions $\pi_k(M^n) = 0$, 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. Perelman.

In the 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 one point, 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. Perelman has recently proved it".

I think that we should write the correct descriptions of what is happening, otherwise the image of mathematics 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 ([4]): The Poincaré conjecture would follow from the fact that the Vassiliev invariants of knots were distinguishing any two different knots.

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

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 atoms nuclei.

Trying to choose a convenient discrete structure, he suggested to suppose 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 the knots (studying their plane projections with few self-intersections of the projected closed line).

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

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

Vassiliev invariants are special invariants of the knots, whose position in the space of the arbitrary invariants of the knots is similar to the position of the polynomials in the space of the arbitrary functions.

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

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

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

This result would never be possible without the Poincaré conjecture, and thus without Poincaré's mistake (of mixing the homotopies with the homologies) which produced, at the end, these wonderful corollaries.

I think, in general, that the mistakes form an extremely important part of the 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 the 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 ones.

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