History of Mathematics from a working mathematician's view

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Introduction

During last decades a number of first-rate results were obtained in several branches of mathematics, such as ordinary differential equations, dynamical systems, topology and mathematical physics. These results already belong to History of Mathematics and have greatly enriched it. But is History of Mathematics really important for mathematics and mathematicians? Why do active mathematicians show growing interest in History of Mathematics? I will discuss these questions by examining several historical cases. In my presentation I shall follow a wise recommendation of Freeman Dyson who said in his 1972 Gibbs lecture *Missed opportunities*: "...I have learned from Hilbert and Minkowski that one does not influence people talking in generalities. Hilbert and Minkowski made specific suggestions of things that mathematiciians and physicists could profitably think about. I shall follow their style " [3].

Let me start with several general remarks. A historian of mathematics and a professional mathematician have different views on the very subject of History of Mathematics. A good historic work could consist in studying the origins of the concept of Zero. For a practicing mathematician, such a question could be interesting only from a general cultural point of view, like the history of building the Egyptian pyramids. Mathematicians who are not overtly burdened with general cultural issues are interested not in the question who proved first such and such fact, but mainly in the result itself, particularly, in relation to their own work. Such mathematicians think that all previously obtained valuable knowledge is contained in textbooks, and reading original papers of classics, let alone non-classics, is simply a waste of time.

The modern tendency of reading preprints in the arXive leads to the situation where new generations of mathematicians are not acquainted with papers of their predecessors and even – quite often – the results of past ten years. It is not rare that young mathematicians coming up with emerging results are sincerely surprised when they learn from their peers that these facts have been already proved sometimes fifty or more years ago.

Mathematics is a happy science. Here, a correct result stands forever. It's another thing that a theorem could be re-proven somewhat later by a different method; the value of a theorem may also be changed in one direction or another. However, it occurrs sometimes that a theorem is considered as proven, and people non-critically accepted its statement for many years. Only after some time, after inspecting the original proof, specialists discover mistakes and gaps. Sometimes it becomes clear that the statement was actually wrong. Such examples are particulaly interesting.

One of the examples of such type is the history of the Riemann–Hilbert problem.

1. The Riemann-Hilbert problem

The history of this problem naturally dates from the paper of Bernhard Riemann: " two general theorems on linear differential equations with algebraic coefficients".This paper was written in 1857/1858 but published postumously [12].

In this article Riemann formulated the following problem.

Consider a system of homogenious linear differential equations in the complex plane $\mathbb{C}$:

$$dy_i/dz = \sum A_{ij}(z)y_j \tag{1}$$

where $A_{ij}(z)$ are functions rational in z. Solutions of the system turn out to be multivalued functions. The singularities of of each solution of Eq.(1) are determined by the poles of the matrix $A(z) : a_1, ...a_k, a_0 = \infty$. The solution naturally changes after a circuit about a singular point. Riemann found a condition guaranteeing that the solution following a circuit of the singular point differs from the original one only by a constant matrix. If after a circuit about the point a_1 the solution $\gamma_1 = (y_{11}(z), ..., y_{n1}(z))$ changes to the vector $\sum b^{(1)}_{ij}\eta_j$, while the solution γ_i changes to $\sum b^{(m)}_{ij}\eta_j$ after a circut about the point a_m, then the matrices $B^{(0)}, \dots, B^{(m)}$ are nonsingular and satisfy Riemann's relation:

$$B^{(0)} \times B^{(1)} \times \dots \times B^{(m)} = E \qquad (2),$$

where E is the identity matrix.

In modern terminology, relations of type (2) define a monodromy mapping, that is, a mapping

$$\pi_1(\widehat{\mathbb{C}} \setminus \{a_0, ..., a_m\}) \to GL(n, \mathbb{C}), \tag{3}$$

where the order n of the group $GL(n, \mathbb{C})$ is defined by the dimension of the fundamental matrix of solutions of the system (1). Here $\widehat{\mathbb{C}}$ is the Riemann sphere (the complex plane $\mathbb{C}$ completed by adjoining the point $a_0 = \infty$), and $\pi_1(X)$ is the fundamental group of the set X.

The matrices $B^{(i)}$ are called monodromy matrices and are generated by circuits around the points a_i over simple loops (loops not containing other singularities).

In the same work Riemann posed the Inverse problem: Given a system of points $a_0, ..., a_m$, does there always exist a system of equations of type (1) displaying given singularities and given transformationmatrices satisfying(2)? Riemann made a conjecture about the form of such equations, but did not produce a general proof. In his lectures on hypergeometric functions he considered the case, $n = m = 2$. Solutions of equations of type(1) constitute a very large class of functions ,in paricular Bessel functions, hypergeometric functions,etc. Riemann's work remained unpublished for almost twenty years. Ignoring Riemann's work, another German mathematician Lazarus Fuchs (1833-1902), set to work on this series of questions in 1865. He gave a detailed classification of singular points of equations of type (1). The most important class of such equations with matrix$A_{ij}(z)$ having simple poles as singularities came to be called Fuchsian equations, a term casually coined by Poincaré. Riemann's basic problem on the existence of equations with given monodromy matrices and singularities remained unsolved. It was considered so difficult and important that Hilbert included it in his famous list of "Mathematical problems". In his speech delivered at the Second Mathematical Congress in Paris in 1900, Hilbert posed twenty-three problems whose solutions he considered vital for the future development of mathematics. The problem discussed here got the number 21; alternatively it became known as the Riemann–Hilbert problem. The history of its solution is very engrossing.

Untill recently it was believed that the Riemann–Hilbert problem had been solved in 1908 by Slovenian mathematician J. Plemelj (1873-1967). Since no doubts had been raised about Plemelj's proof, subsequent efforts were directed mainly towards finding effective methods of constructing equations from a given monodromy group. In particular, a detailed analysis of branching points had been carried out by Russian mathematician Ivan Lappo-Danilevsky (1896-1931) who developed the apparatus of analytic functions of matrices, specifically for this purpose. The Riemann problem was extended to an arbitrary Riemann surface by the German mathematician H. Röhrl in 1957. However, in the beginning of the 1980s some doubts arose about the correctness of Plemelj's proof. In the beginning, the issue was not considered serious. But the most dramatic events occured in 1989 when Russian mathematician Andrei Bolibruch (1950-2003) constructed a conterexample to Plemelj's theorem [1, 2] [1] It turned out that for any m points $a_1, ...a_m$, $m > 3$ and $n \geq 3$, there exists a representation (3) not realized by any Fuchsian system. This remarkable result forced a re-examination of this entire domain of differential equations.

The second example concerning another classical theorem.

2. Poincaré finitness theorem for limit cycles

Henri Poincaré, in his memoir *Sur les courbes définis par une équation différentielle*,(1881) posed the following problem. Consider the system of differential equations:

$$x = P(x, y), y = Q(x, y), \text{ where } P,\, Q \text{ are polynomials in } x, y \in \mathbb{R}. \tag{4}$$

Is it true that:

1. The number of of limit cycles of equation (4) is finite?

2. The number of limit cycles of a polynomial vector field of degree n depends only on n ?

Both problems turned out to be difficult. To my knowledge, the second problem is still unsolved. But what happened to the first statement?

Henri Dulac (1870-1955), a student of Poincaré, developed in 1909–1923 the so called local theory of differential equations. In 1923 he published a long paper *Sur les cycles limites* (more than one hundred pages) where he presented the positive solution to Poincaré question [6]. The proof survived for more than 60 years. However, a serious gap was found in it in the beginning of the 1980s. About ten years later two mathematicians, J.Écalle in France and Y. Ilyashenko in Russia, independently proved this theorem [4, 5]. Both papers are long and very difficult. The complexity of their proofs is related to a detail analysis of behaviour of curves near singularity points. The fact that these two proofs were obtained independently and bear some differences instills hope that, at least for the time being, the long

[1] It is curious that this striking result was publishied in the Journal with the title"Matematicheskie Zametki (in Russian)" – "Mathematical notes" , in the section: brief communications and occupaied only two pages.

history of the proof of Poincaré's conjecture is over. [2] Anyway, these concrete and important questions stimulated the development of the theory of ordinary differential equations.

My third example illustrates a thesis that a nice mathematical result perceived of being of a little value or lying far from the mainstream in development of mathematics in one period could become very important many years after.

3. Löwner equation and Brownian motion

In 1923 Karl Löwner (Charles Loewner 1893– 1968), at that time Chech mathematician, studied the following problem [14]. [3]

Consider a disk $D \subset \mathbb{C}$ and a pair of points, a, b, lying on the boundary ∂D.

Löwner was interested in description of curves joining a and b and lying inside D. He gave such a description in terms of an equation involving the so-called driving function g_t. The equation (called the Löwner evolution equation) is

$$\partial_t g_t(z) = \frac{2}{g_t(z) - u(t)}, \tag{1}$$

where g_t is a conformal mapping $D \setminus \gamma[0,t] \longrightarrow D$, and $u(t) = g_t(\gamma(t))$. Sometimes it is convenient to replace D by the upper half-plane $H \subset \mathbb{C}$.

This equation was considered as very special and consigned to oblivion for many years. [4]

However, in 2000, the Löwner equation was used in

an ingenious paper of Israeli mathematician Oded Shramm (1961-2008), devoted to the problem of phase transitions in stochastic systems [8].

The idea of Schramm was to study the curve $\gamma(t)$ where $u(t)$ is a trajectory of a Brownian motion along ∂D (or ∂H), with diffusion coefficient κ: $u(t) = \sqrt{\kappa}B(t)$.

When we consider a random function $u(t)$, we obtain a random curve from a to b inside D; the probability distribution for such a curve turned out to be of a great interest. Accordingly, the emerging equation was called by Schramm "Stochastic Löwner equation", SLE_κ for short (nowadays the abbreviation SLE is referred to as the Schramm–Löwner equation or the Schramm–Löwner evolution). It is remarkable that these equations for different κ are connected with the scaling limits of some famous two-dimensional lattice models from Statistical Physics, including the Ising model, percolation and a number of others.

Nowadays the theory of the Stochastic Löwner equations is one of highly active interdisciplinary fields in science, including probability, complex analysis, statistical physics and conformal field theory.

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[2]The best way to be sure that now we have complete proofs is to ask the authors to check the proof of a colleague :"peculiar cross-fertillization".

[3]Later, escaped from Nazis and became the professor at Stanford University he changed his name to Charles Loewner.

[4]The story with the heritage of the Loewner paper is not so simple. His work [14]was very important for the theory of analytical functions. Loewner himself applied his equation to solve a special case of famous Biberbach problem. The general Biberbach conjecture was solved only in 1985 by de Branges elaborating different method. But some years later C.H. Fitzgerald and Ch. Pomeranke have found some principal improvementin the de Branges theorem applying Loewner equation. But, nevertheless, Loewner's paper was known only for specialists in classical aspects of the Compex analysis.

4. Duality in Non-abelian groups and Statistical physics

In this section I describe the final example from my collection. It illustrates complex relations between mathematics and physics; this is especially close to my own research.

We begin with some mathematics.

In the beginning of the 1930s, Pontryagin and van Kampen built the duality theory for abelian groups. The main result was the following theorem.

Let G be a locally compact Abelian group. Consider the group of characters of G, i.e. the set of mappings

$$\chi(g_1 \times g_2) = \chi(g_1) \times \chi(g_2)$$

This set of mappings make up a group $\widehat{G}$, called the character group (or the dual group) of G. Then $\widehat{\widehat{G}}$, the dual group of $\widehat{G}$, coincides with G.

It is natural for mathematicians to try to generalize this theorem to a non-Abelian case. This is a nontrivial task, since in the non-commutative case the product of irreducible representations is not irreducible and so the set $\widehat{G}$ is not a group. Nevertheless, this problem in some sence was solved by Japanise mathematician T.Tannaka in 1938. Idependently, it was solved by Soviet mathematician M. Krein in 1941, who didn't know about the work of Tannaka. The paper of Tannaka attracted attention of von Neumann who noted the above-mentioned difficulty and indicated several important general properties of $\widehat{G}$. A dual to a non-commutative group is not a group but a commutative space, endowed with a multiplicative operation. The papers of Tannaka and Krein were practically forgotten for almost thirty years, until the first papers aqppeared on non-commutative integration and ring groups. But the real value of these works has been appreciated later, in the 1980s, when the theory of quantum groups was created. Quantum groups are closely related to integrable quantum systems. These systems appeared shortly before in physics.

Now we turn to physics.

In Statistical physics, within the theory of phase transitions, for a long time a number of models have been proposed and analysed, describing lattice aproximation for vartious kinds of physical matter. One of the first such model was the one-dimensional Ising model (1925). Ernst Ising (1900-1998), who was a student of Wilhelm Lenz, wrote a paper were he found an analytical formula for the free energy of the model. A generalization to the two-dimensional case led to serious difficulties. The two-dimensional Ising model was solved only in 1944 by Onsager and till now is a rare example of an exactly solvable model in Statistical physics.

Consequently, physicists tried to find approximate methods to identify points of phase transitions. In 1941, two Dutch physicists Kramers and Wannier have found a very nice method of calculating the point of phase transition in the two-dimentional Ising model. They constructed a transformation between the low-temperature and the high-temperature phases. It was latter called the Kramers–Wannier duality. From the mathematical point of view it is a very interesting object: an infinite-dimensional bundle with the structure group $G = Z_2$. The Kramers–Wannier duality consists in passing to the dual lattice (homological duality á la Poincaré) and to the dual group $\widehat{G}$, in this case coinciding with the same Z_2. Latter, physicists generalized the KW-duality to systems with a Z_n-symmetry (so-called Potts models).

At the end of the 1970s physicists, in connection with problems in Quantum field theory (quark confinement), became interested in a generalization of the KW-duality to non-Abelian groups. Just at this time there appeared a few papers related to the KW-duality for some non-Abelian groups; I, too, became interested in this topic (see [7]). At that time Alexander Zamolodchikov and myself (as other physists) had no idea about the results of Tannaka–Krein.

Then, 25 years later, Victor Buchstaber and myself constructed the KW-duality for non-commutative finite groups based on the ideas of quantum groups [9, 10] Only later we learned about the works of Tannaka and Krein.

Although our results were not covered by Tannaka–Krein, it is evident that the earlier knowledge of their ideas would have allowed us to complete our work much earlier. In the course of our work we have found interesting relations with an old (and almost forgotten) paper by Frobenius [15]. Ferdinand Gotfried Frobenius (1849- 1917) was one of the founders of the theory of group representations (mainly for finite groups). His famous theorems about irreducible representations of groups are presented in all textbooks on the theory of groups representations. But one of his papers, full of interesting ideas, was shelved for decades. For instance, Frobenius introduced for non-commutative groups the concept of generalized characters. He posed the question of whether generalized characters determine a group in the same way as in the commutative case. It is well known that there exist non-isomorphic groups with the same table of characters, viz., the group of unit quaternions Q and the dihedral group D_2, both of order 8. Another intersting notion introduced in the same paper is a noncommutative determinant, an important generalization of the commutative determinant defined earlier by Dedekind. This result of Frobenius was recently applied in the graph theory [13]. One of possible explanations of the fate suffered by the paper of Frobenius is that his successors Issai Shur, William Burnside and Emmy Noether found a new and more transparent way to develop the representation theory. Consequently, a somewhat sophisticated and complex presentation adopted in the Frobenius' paper caused oblivion for years to come.

Two other articles of Frobenius [16, 17] related to the representation of Symmetric groups and rarely cited (see e.g.[18],also deserved additional study. In these papers he developed a method parallel to the well known Young tableaux. His method is now rediscovered and applied in the theory of the infinite symmetric group $S(\infty)$ and in the theory of random surfaces. Similar examples can be found in the history of modern research in such fields as the theory of knots, holomorphic dynamics, the so-called Berry phase in quantum mechanics [11]and some others. I would like to think that I have showed that mathematics and its history form a unified subject; it is a growing tree with many branches. Some of them may be provisionally abandoned or fallen in oblivion; yet they show their usefulness at later times. The history of mathematics, particularly the history of proofs, provides means to reveal the line of succession in the development of this science.

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