Some Trends in Modern Mathematics and the Fields Medal

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Introduction

The Fields Medal is now indisputably the best known and most influential award in mathematics. Sometimes it is compared with the Nobel prize, since there is no Nobel prize for mathematics. Publishers and journalists especially like this comparison. It seems to me that such a comparison is not adequate. The Fields medal was established on different principles. Unlike the Nobel prize, which is mostly awarded to mature scientists to crown their careers, the Fields medal is awarded to young scientists, less than 40 years old. The prize is intended not only to recognize results already obtained, but also to stimulate further research. Besides this it is awarded only every four years, at the International Mathematical Congress.

The first Fields Medal was awarded in 1936 in Oslo and the second one 14 years later, in 1950, in Cambridge, Massachusetts. So mathematicians born during 1900-1910 were automatically excluded from the list of candidates, for example brilliant mathematicians like A. Kolmogorov, H. Cartan, A. Weil, J. Leray, L. Pontryagin, S. S. Chern, and H. Whitney. Nevertheless, if we look at the achievements of Fields laureates from the point of view of the development of mathematics in the 20th century, we see an impressive picture.

The founder of the prize, John Charles Fields, considered two fundamental principles for the award:(a) the solution of a difficult problem and (b) the creation of a new theory enlarging the fields of applications of mathematics. Both these principles are important for the development of mathematics. It is quite clear that they are not independent. Very often the solution of a concrete difficult problem is based on the creation of a new mathematical theory and, conversely, the creation of a new theory may lead to the solution of an old classical problem.

It is absolutely impossible to cover in a one-hour talk the results of Fields laureates even in a condensed form. In this talk I shall take a stroll through modern mathematics, giving a kaleidoscopic view of some exciting pictures. I shall try to explain the characteristic features of the mathematics of the 20th century, what kind of mathematics is considered important in this or that period, and how the results of the Fields medallists look from this point of view.

The role of prizes, like the role of international recognition in general, is important for individual scholars. Despite Franz Neumann's beautiful quote, "The discovery of new truth is the greatest joy; recognition can add almost nothing to it," this wise idea is only partially true. According to Niels Bohr, the opposite conclusion is also valid. Recognition is especially important to young researchers. Selecting young mathematicians supports the continuing development of mathematics. The Fields Committees consist of outstanding mathematicians of the older generation, which makes their assessment of the creativity of the young all the more interesting.

As I already mentioned, the first Fields Medal was awarded in 1936, and the next one in 1950, so with one exception the medals are connected with the second half of the 20th century. The second world war greatly affected the development of society and science in general, mathematics especially. The development of mathematics is a good illustration of the more general thesis about the continuous but "nondifferentiable" nature of the development of science. If we consider the graph of the development of mathematics, we evidently see the changes of interest in the periods of the world wars. It is natural for science to develop continuously, a fact based both on internal factors and the succession of generations. Also, science is characterized by some conservatism, which I consider in general as a robust phenomenon. Great ideas appear in the world by noiseless steps, as Nietsche said. The acceptance of new ideas proceeds against great obstacles and requires long testing. As Max Planck joked, "a new scientific truth does not triumph by convincing its opponents and making them seeing the light, but rather because its opponents eventually die, and a new generation grows up with it." That each tragic world war destroyed a whole generation of scientists accelerated in addition an apparently objective process to accept new points of view in mathematics.

If we look at the prizes of 1936 and 1950 from this point of view we can see that new waves such as the explosion of interest in algebraic topology and geometry in the first years after the Second World War are not yet reflected in the first postwar award. The 1950 prize was awarded to L. Schwartz (for the theory of distributions) and to A. Selberg for his remarkable achievements in number theory, namely, the distribution of zeros of Riemann ζ-function and an"elementary" proof of the asymptotic distribution of primes. But in 1954 the prize was awarded to K. Kodaira and J. P. Serre for postwar achievements. Hermann Weyl, who chaired the Fields committtee in 1954, delivered a speech on the papers of Kodaira and Serre. Curiously, Weyl had difficulty distinguishing the areas of research of the two mathematicians. He said, "The uninitiated may get the impression that our committee erred in awarding the Fields Medals to two men whose research runs on such closely neighboring lines. It is the task of the Committee to show that, despite some overlap in methods, they give the solutions of completely different, extremely difficult problems."

In the subsequent awards, we see a definite balance between the two leading principles established by the founder of the prize. For example, in 1958 Klaus Roth was honoured for the proof of a delicate estimate that refines the Thue-Siegel theorem on the approximation of algebraic numbers by rational numbers. **Roth's theorem:** If α is any algebraic number, not itself rational, then for any $\nu > 2$ the inequality

$$|\frac{p}{q} - \alpha| < \frac{1}{q^{\nu}}$$

has only a finite number of solutions in rational p/q.

The second medalist was René Thom, who constructed a powerful method in topology known as the cobordism theory.

In 1962 the prize winners were Lars Hörmander and John Milnor. Hörmander developed the general theory of linear partial differential equations, including hypoelliptic operators. The work of the other laureate was absolutely astonishing and has had great influence on the future development of topology. It is very difficult to find an analogous invention in the past to his beautiful construction of the different differential structures on the seven-dimensional sphere. Later, the result became the cornerstone of a new branch of topology — differential topology. The original proof of Milnor was not very constructive but later E. Briscorn showed that these differential structures can be described in an extremely explicit and beautiful form.

Four medals were awarded in 1966. Among those honoured was Paul Cohen, who showed that if the Zermelo-Fraenkel axioms are consistent, then the negation of the axiom of choice or even the negation of the continuum hypothesis can be adjoined and the theory will remain consistent. It was the first and the last time that the award was given to a specialist in mathematical logic. Alexander Grothendieck, one of the most original and puzzling mathematicians of our time, revolutionized algebraic geometry. The concept of schemes that he introduced raised algebraic geometry to a new level of abstraction, beyond the reach of mathematicians with a traditional education. The theory of sheaves, spectral sequences, and other innovations in the late 1940's and earlier 1950's are subsumed by this complicated technique. But if certain mathematicians could console themselves for a time with the hope that all these complicated structures were "abstract nonsense"(in algebra, the term "abstract nonsense" has a definite meaning without any pejorative connotation), the later papers of Grothendieck and others showed that classical problems of algebraic geometry and the theory of numbers, the solutions of which had resisted efforts of several generations of talanted mathematicians, could be solved in terms of the Grothendieck K-functor, motives, l-adic cohomology, and other equally complicated concepts.

Two remarkable mathematicians are present at this conference. The traditions of a scientific community are rather different from those of writers, movie stars, and fashion models. It is not an accepted practice to compliment a renowned scientist in his presence. So I really will not touch on the results of the mathematicians present here, but make some exception and say some words about the results of Steven Smale and Michael Atiyah, because they beautifully characterised the level of the prize and the realisation of its principles.

The results of Smale are especially near to me, since I started my own career in mathematics as a student of the well-known Russian mathematician Dmitry Anosov, and his first advice was to study the papers of Smale about dynamical systems.

S. Smale was honoured mostly for two of his achievements. The first one is the solution of the Poincaré conjecture in higher dimensions. The Poincaré conjecture is among the most difficult problems in topology. It can be stated as follows in modern terms:

Poincaré conjecture *A closed smooth simply connected manifold M^n with the homology groups of the sphere S^n is homeomorphic to S^n.*

Poincaré stated his conjecture in three dimensions. He believed that a stronger assertion was true, namely that M^n is diffeomorphic to S^n. But as follows from the existence of Milnor's exotic spheres, the conjecture is not true in this form. Smale proved a more general theorem on h-cobordism, from which it follows that Poincaré conjecture holds for dimensions $n \geq 5$. In dimensions 5 and 6, a stronger conjecture is true: M^n is diffeomorphic to S^n.

At first sight it seems paradoxical that the proof of the Poincaré conjecture for higher-dimensional spaces is more accessible than for three- and four-dimensional manifolds. The reason is that a map of a surface into a manifold of fewer than five dimensions cannot be approximated by an embedding. The situation is similar to the classification of manifolds. This indisputably classical result corresponds to the first principle of the Fields award.

The second achievement of Smale is connected with the theory of dynamical systems. This field has its origin in classical mechanics and the theory of ordinary differential equations. It was developed at the beginning of the twentieth century by H. Poincaré, G. D. Birkhoff, J. Hadamard, and I. Bendixson. In the middle 30's, remarkable results were obtained by E. Hopf, G. Hedlund, M. Morse, A. Andronov, L. Pontryagin, and some others. But almost all of them were of a two-dimensional nature. Smale substantially developed a multidimensional case. He showed that so-called structurally stable dynamical systems in higher dimensions have radically different properties. Unlike two-dimensional systems, studied by Andronov and Pontryagin, in a multidimensional situation structurally stable systems may have infinite number of singular points, limit cycles, etc . His first construction was the famous horseshoe, generated by discrete automorphisms of the torus. He proposed a very interesting hypothesis about the structural stability of geodesic flows on compact manifolds of negative curvature, later proved by Anosov. These results led to the creation of the theory of multidimensional dynamical systems, a new field of mathematics still actively being developed. These results of Smale are an excellent illustration of the second Fields principle.

The other laureate of this year, M. Atiyah, was recognised for his work in algebraic topology, especially for the proof of the index theorem which is known as the Atiyah-Singer Theorem. This theorem is remarkable from several points of view. Firstly, it generalized the long sequence of famous theorems begining with the Euler theorem on polyhedra and including the Riemann-Roch Theorem and the Poincaré-H. Hopf Theorem about the singularities of vector fields.

The original proof of Atiyah and I. M. Singer was extremely complicated and used a wide spectrum of mathematical concepts developed in algebraic topology, geometry, and partial differential equations in previous years. Later, essential simplifications were obtained and, especially remarkable, in recent years the relation between this theory and important problems in quantum field theory, for example the problem of quantum anomalies, became clear.

The work of Atiyah and Singer, Grothendieck, F. Hirzebruch, and many other mathematicians established a new field of mathematics, where the ideas of algebraic topology and geometry and complex analysis are so interwined that traditional division is absolutely impossible now. Using a nice phrase Atiyah said, "topologists used to study simple operators on complicated manifolds while analysts studied complicated operators on simple spaces." The time has arrived to study complicated operators on complicated spaces.

These results not only raised mathematics to a very high level of abstraction, but proved the fruithfulness of these methods in the solution of long standing unsolved classical problems. One of the best examples is the solution by J. Adams of the famous problem of the existence of division algebras. From the time of Cayley, the following division algebras were known: real numbers, complex numbers, quaternions, and Cayley numbers. As the dimension grows we lose some properties, e.g. quaternions are non-commutative. A natural question is: Are there other division algebras? The negative answer was obtained only in the 1960s and proved to be closely related to the following topological problem: find all spheres on which the number of independent, continuous vector fields is equal to its dimension. There are only three such: S^1, S^3, S^7.

I hope that this gives at least a hint of how the two principles of Fields are linked in the work of M. Atiyah. Mathematics is a single subject, a fact that is not always obvious when you study the daily reality of research. It becomes clear, however, when you become acquainted with results of great mathematicians. This realization is one by-product resulting from an analysis of the works of the Fields medallists. Although honours went to authors of the greatest achievments obtained in the years immediately preceding each congress and sometimes in areas of mathematics widely separated from one another, truly wonderful connections between them were discovered with the passage of time. For that reason an ϵ-grid over the works of the Field medalists covers a significant portion of the achievements of modern mathematics.

Editors' note: This article will be continued in the next issue.

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by Michael Monastyrsky

This is the second and concluding part of this article. The first part appeared in last month's issue.

If we very quickly review the results of the Fields medallists, keeping in mind the fundamental principles of J. Fields, we can observe several interesting developments:

1. Allocation of stable fields of interest.
2. Succession of mathematics.
3. Zigzags of mathematical fashion.

I will try to illustrate these theses with excerpts from the Fields medallists' results.

1. Allocation of Interest

Indisputably, if we divide the mathematics of the second half of the century into two parts, the first thirty years is mainly concentrated around problems of algebraic topology, algebraic geometry, and complex analysis. Here new concepts and methods appeared, and this evidently is reflected in the list of Fields medallists. A definite change in this tendency, a return to the more classical topics, but of course on a new level, can be observed in mathematics from the end of the 70's. With some delay, this has been reflected in the Fields medals awarded at the last two congresses.

It is important to note the new convergence between mathematics and physics. The traditional contacts between mathematics and physics are well known. If we consider the parallel development of mathematics and fundamental physics, we are astonished that the most revolutionary theories in 20th century physics are based on mathematics, which was especially developed for this purpose. It is enough to mention Einstein's special and general relativity based on the classical differential geometry of Riemann spaces, quantum mechanics and Hilbert spaces and the theory of linear operators, the Schrödinger equation and spectral theory, and so on. This connection was broken, somewhere in the 30's, at the time of the solution of several more concrete problems in physics, when it seemed to physicists that most of their problems could be solved without the application of sophisticated and abstract modern mathematics. The development of pure mathematics in the period between the two world wars, and especially in the post-World War II period, was also characterized by weak connections with applied science, in particular with physics. This association was especially true of the areas of mathematics in which many Fields medalists worked. It was difficult to imagine that the concepts of sheaf, étale cohomology, J-functor, and the like would ever be applied in physics. It was still more difficult to imagine that physics could assist algebraic topology and geometry.

This point of view was widespread. The French mathematician Jean Dieudonné, one of the founders of Bourbaki, expressed himself unambiguously on this subject in 1962. "I would like to stress how little recent history has been willing to conform to the pious platitudes of the prophets of doom who regularly warn us of the dire consequences that mathematics is bound to incur by cutting itself off from applications to other sciences. I do not intend to say that close contact with other fields, such as theoretical physics, is not beneficial to all parties concerned; but it is perfectly clear that of all the striking progress I have been talking about, not a single one, with the possible exception of distribution theory, had anything to do with physical applications." (Quoted from an address delivered at the University of Wisconsin in 1962, in which Dieudonné gave a survey of the achievements of the preceding decade in pure mathematics. He emphasized algebraic topology, algebraic geometry, complex analysis, and algebraic number theory.) But as often happens with globally expressed opinions, the situation underwent a vast change ten years later.

At the begining of the 70's, both in mathematics and physics, results were obtained that absolutely changed this point of view. Among the mathematicians who quickly understood the new opportunities and challenges hidden in the new physics were some Fields laureates. It is enough to mention S. Novikov, S. T. Yau, A. Connes, S. Donaldson, and E. Witten. Witten was the first physicist to be awarded a Fields medal. Among the results of Fields laureates which were inspired by physical ideas, let us mention first of all the work of Simon Donaldson. After the work of Milnor on differential structures on S^7, the paper of Donaldson appearing in 1983 had a similar striking impact. Donaldson proved the existence of different differential structures on simply-connected 4-dimensional manifolds. (Unfortunately the case of S^4 is not covered by his method and is still open.) Immediately after the work of Donaldson, in papers of R. Gompf and C. Taubes, the following remarkable result was proved: There exist an infinite number of different differential structures on R^4. This result, that the "well-known" space R^4 hides such deep structures, is absolutely astonishing. It has very deep consequences for quantum gravity, where integrating over all metrics and so over different differential structures is necessary. It is not less important than the proof, which is based on earlier discoveries in field theory, mostly in the gauge theory of strong and weak interactions. Such interactions in the world of elementary particles are described by highly nonlinear equations with deep topological properties—the so called Yang-Mills equations. These equations were invented by the physicists C. N. Yang and R. Mills in 1954, but for many years were considered as nonphysical and attracted very little attention from physicists. Only the newest development of the theory of elementary particles—the creation of the theory of weak and strong interactions based on the Yang-Mills equations—led physicists to a deeper study of the structure of these equations.

In the early 70's, the physical-mathematical union lessened the gap in the transmission of information, leading to the final score in this striking mathematical achievement. These and other more recent results led to a new and deeper connection between mathematics and physics. The value of this union for modern mathematics is indispensible and is based on a series of achievements of the first rank. It is enough to mention the results of V. Drinfeld, M. Kontsevich, and many others.

2. Mathematical Succession

The best confirmation of continuity and fruitfulness in the development of mathematics is the solution of deep classical problems left by the previous generations of mathematicians. And here the results of Fields medalists confirm this idea nicely.

The first recipient of the Fields Medal was Jesse Douglas, who solved the classical two-dimensional Plato problem. It is necessary to say that this problem was solved simultaneously by Tibor Rado, but Douglas' solution was considered as deeper and could be applied to higher dimensions.

Mathematicians of this mind-set include the famous number theorists like A. Selberg, K. Roth, and A. Baker. In the latest period, we see this tradition in the works of Gregory Margulis and Pierre Deligne.

The most important result of Margulis is his proof of Selberg's conjecture that a certain class of discrete subgroups of the group of motions of symmetric spaces of higher rank with finite volume is arithmetic. While the conjecture can be stated rather easily, its proof required a virtuoso mastery of the technique of the theory of algebraic groups, use of the multiplicative ergodic theorem, the theory of quasiconformal mappings, and much more. In recent years Margulis has examined the properties of discrete groups in different and sometimes unexpected areas. By combining ideas from the theory of discrete groups and ergodic theory, he recently solved an old problem of the geometry of numbers: Oppenheim's conjecture on the representation of numbers by indefinite quadratic forms.

Pierre Deligne received the prize for a proof of a conjectures of A. Weil on zeta functions over finite fields. His results are included as a special case of the proof of the classical Ramanujan conjecture.

Ramanujan Conjecture: Consider the parabolic form

$$2\pi^{-12}\Delta(z) = x\Pi_{n=1}^{\infty}(1-x^n)^{24} = \sum_{n=1}^{\infty}\tau_n x^n$$

where $x = \exp(2\pi i z)$. Then $|\tau_p| \leq 2p^{11/2}$ for all primes p.

The proof of Deligne is one of the most brilliant and striking examples of the unity and continuity of mathematics. It is striking in its beauty and complexity, but required the application of the wealth of techniques accumulated in algebraic geometry over preceding years.

The last example which I give, but only to mention in passing to support this thesis, is the proof of the "Moonshine hypothesis" by Richard Borcherds. Here the statement regarding the relations between the coefficients of special modular forms, dimensions of the representations of the Monster group and some infinite-dimensional Kac-Moody algebras led to the proof by applying methods from different fields of mathematics. It was inspired by the recent development of string theory.

3. Zigzags in Mathematics

What I mean are the zigzags of mathematical fashion. I already talked about the domination of three mathematical diciplines in the list of Fields awards. Some reaction to this bias, even beside some objective background, appeared at the two last congresses. The awardees were mathematicians working in more classical fields. Let us mention here Jean Bourgain and Tim Gowers–Banach spaces, harmonic analysis, combinatorics; P-L. Lions–partial differential equations; Jean-Cristoph Yoccoz, Curtis McMullen–dynamical systems, holomorphic dynamics; and the algebraist Efim Zelmanov, who solved the classical restricted Burnside problem. This result capped off an extended period in group theory. J. Bougain and T. Gowers solved several classical problems in the theory of Banach spaces, discovered in very deep structures. J. C. Yoccoz and C. McMullen got important results in the so-called holomorphic dynamics. Here the study of sequences of mappings of complex sets led to the theory of dynamical systems. A typical problem of holomorphic dynamics is to describe the limiting sets of points of the mapping $z \to R(z)$, where $R(z)$ is a rational function and z is in $\mathbb{C}$ or $\overline{\mathbb{C}}$. Even the the study of sequences of iterations of such a seemingly simple map as $f_c(z) = z^2 + c$ conceals highly nontrivial results. This theory is placed at the meeting point of many beautiful mathematical theories, such as dynamical systems, Kleinian groups, Fricke-Teichmüller spaces and many others, including computer graphics.

This theory is very remarkable and instructive if you look at it from a historical perspective. Created in the end of the 19th and the beginning of the 20th centuries in the works of the famous mathematicians P. Fatou, P. Montel, and G. Julia, it was seriously forgotten for more than forty years and was restored only in modern times. Now, besides being a very interesting theory, it has a wide field of applications in physics. Let us mention the famous universality law of Feigenbaum which has important applications in turbulance.

The unity of mathematics is shown best with these seemingly simple yet extraordinary complicated examples.

To finish this very sketchy review of some of the achievements of modern mathematics in the light of Fields medals, let me say that the results honored by Fields medals substantially determined the development of mathematics in our time and its laureates are worthly representives of the mathematical community. Whether or not the Fields medal can be compared with Nobel prize, Fields' idea of awarding it to the young has met with complete success.

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