## Introduction


The Fields Medal is universally considered as the highest distinction in the field of mathematical sciences. It is awarded every four years during the session of the International Congress of Mathematicians. The most recent such congress was held last summer in Beijing, China. It is therefore a good opportunity for us to give an overview of the history of the prize and to present briefly the work of some outstanding mathematicians who received the medal.

As it is well known, there is no Nobel Prize in mathematics. There are several rumours circulating about the causes of this disfavour, among which the most entertaining is the one according to which Nobel had a grudge against mathematicians because the most influential Swedish mathematician of his time, Gösta Mittag-Leffler had seduced his wife. This anecdote has a minor shortcoming, namely that Nobel never married. On the other hand, it is rather probable that Nobel and Mittag-Leffler were not on good terms. However, a good friend of Mittag-Leffler's was the Canadian mathematician John Charles Fields, who, coming from a country where scientific research was only starting to develop at the beginning of the last century, was a great promoter of the development of international scientific relations. In particular, in 1931 he initiated the foundation of an international prize honouring outstanding mathematicians, conceived in part as a replacement for the Nobel Prize. According to his project, an international committee would select two laureates (in 1966 the maximal number of laureates was raised to four) every four years who would receive their prize during the International Congress of Mathematicians. But unfortunately his 1931 death prevented him from assisting at the first prize ceremony, held in 1936. In his will he bequeathed 46 000 Canadian dollars of the time to the international foundation supporting the prize, a sum that of course cannot be compared to Nobel's legacy. Accordingly, the cash prize accompanying the medal is rather modest: currently it is 15 000 Canadian dollars (less than 10 000 USD).

However, the prestige of the prize is all the more important, due to the eminence of the members of the prize committee and of the previous laureates. Just like the Nobel prize, the Fields Medal is not an award for lifetime achievement, but rather a recognition for particular outstanding results. There is, however, an important restriction which distinguishes it from the majority of similar scientific prizes, namely the condition that the recipient should have discovered the result for which he/she is distinguished before the age of 40. The reason why Fields imposed this restriction was that he wanted the prize to be not only a recognition of achievements in the past but also a stimulus for further research. In such a way, he maybe also wanted to dissipate the misbelief, still rather virulent today, according to which mathematicians make their greatest discoveries at a very early age and then quickly fade away. In fact, the truth is that most mathematicians are at the pinnacle of their career during their thirties and forties, so the limit drawn by Fields seems to be justified. Fields' intention is moreover confirmed by the fact that most Fields Medalists continued to discover important results after receiving the honour — some of them even in fields other than those in which their work was recognized by the prize.

On the other hand, the age limit can also be merciless: many eminent mathematicians could not get the prize because they attained the acme of their career after the age of 40. The most prominent of such cases is that of *Andrew Wiles,* who proved Fermat's Last Theorem at the age of 41. After a long debate, he was finally awarded a special tribute by the Congress for this seminal achievement. Scholars who have missed the Fields Medal can also draw consolation from several awards for lifetime achievement. The most famous of these is the Wolf Prize donated by an Israeli foundation, among whose recipients we find three mathematicians of Hungarian nationality or origin: the late academician *Paul Erdős,* Professor *Peter D. Lax* of the Courant Institute, New York, and the academician *László Lovász,* formerly a professor at Eötvös University for many years, currently working at Microsoft Research. Also, in May 2003, the Norwegian Academy of Sciences awarded the first Abel Prize in Mathematics, intended as a substitute for the Nobel Prize, to the French mathematician (and Fields Medalist) Jean-Pierre Serre.

## The list of Fields Medalists

Now let us look at the list of those mathematicians who have received the award since its foundation in 1936.

1936: *Lars Ahlfors* (Finnish; complex analysis)
*Jesse Douglas* (American; analysis, differential geometry)

1950: *Laurent Schwartz* (French; analysis)
*Atle Selberg* (Norvegian; number theory)

1954: *Kunihiko Kodaira* (Japanese; algebraic and analytic geometry)
*Jean-Pierre Serre* (French; topology)

1958: *Klaus Friedrich Roth* (German; number theory)
*René Thom* (French; topology)

1962: *Lars Hörmander* (Swedish; analysis)
*John Milnor* (American; topology)

1966: *Michael Francis Atiyah* (British; analysis, analytic geometry, K-theory)
*Paul Cohen* (American; set theory, mathematical logic)
*Alexander Grothendieck* (French; algebraic geometry)
*Stephen Smale* (American; topology)

1970: *Alan Baker* (British; number theory)
*Heisuke Hironaka* (Japanese; algebraic geometry)
*Sergei Novikov* (Russian; topology)
*John Thompson* (British; algebra)

1974: *Enrico Bombieri* (Italian; analysis, number theory)
*David Mumford* (British; algebraic geometry)

1978: *Pierre Deligne* (Belgian; algebraic geometry)
*Charles Fefferman* (American; complex analysis)
*Grigory Margulis* (Russian; differential geometry, dynamical systems)
*Daniel Quillen* (Canadian; topology, K-theory)

1982: *Alain Connes* (French; analysis, differential geometry)
*William Thurston* (American; topology)
*Shiu-Tung Yau* (Chinese; analysis, algebraic and analytic geometry)

1986: *Simon Donaldson* (British; topology)
*Gerd Faltings* (German; algebraic geometry, number theory)
*Michael Freedman* (American; topology)

1990: *Vladimir Drinfeld* (Ukranian; algebraic geometry, algebra, mathematical physics)
*Vaughan Jones* (Australian; topology, mathematical physics)
*Shigefumi Mori* (Japanese; algebraic geometry)
*Edward Witten* (American; mathematical physics, topology)

1994: *Jean Bourgain* (Belgian; analysis)
*Pierre-Louis Lions* (French; analysis)
*Jean-Christophe Yoccoz* (French; dynamical systems)
*Yefim Zelmanov* (Russian; algebra)

1998: *Richard Borcherds* (British; algebra)
*Timothy Gowers* (British; analysis, combinatorics)
*Maxim Kontsevich* (Russian; algebraic geometry, mathematical physics)
*Curtis McMullen* (American; analytic geometry, dynamical systems)

2002: *Laurent Lafforgue* (French; algebraic geometry)
*Vladimir Voevodsky* (Russian; algebraic geometry, K-theory)

Browsing through the above list, one perhaps first pays attention to the nationalities of the laureates. It is no surprise that the list is dominated by nations of great mathematical tradition: the Americans, the British, the French and the Russians. But it is somewhat surprising to see that another nation with a glorious mathematical history is underrepresented, namely the Germans. Unfortunately, there are historico-political reasons for this: the bloodshed of World War I, the Nazi dictatorship and the division of the country during the Cold War have damaged German mathematics so gravely that it is recovering only nowadays. We must also mention the absence of Hungarian mathematicians. Here the situation is that after the golden era of the first half of the 20th century Hungarian mathematics drifted off the mainstream of international research: despite the work of some leading personalities, most of the research fields honoured by Fields Medals were not studied in Hungary. Fortunately during the past decade there have been remarkable changes in this respect and further progress can be expected with the growing number of possibilities for studying and travelling abroad.

If we examine the distribution of laureates according to their fields of research, we can observe no significant changes during the decades of the award's history. Basically, we find the great classical branches of pure mathematics:

mathematical analysis, algebra, number theory and different branches of geometry (topology, differential geometry, algebraic geometry). On the other hand, it is regrettable that some important fields are completely neglected; for instance, probability theory is totally absent and a single medal is allotted to mathematical logic. Also, one can search in vain for some fields important for applications such as computer science, numerical analysis or information theory. Workers in these domains are being honoured by the Congress since 1982 with the *Rolf Nevanlinna Prize,* awarded at the same time as Fields Medals.

Let us now briefly discuss what sort of achievements are recognized by the Fields Medal. Roughly, these can be divided into two categories: the solution of famous open problems and the elaboration of new theories. Of course, the two categories are by no means independent of each other. In most cases, the proof of a famous conjecture is achieved by developing new tools which then can be used to attack other problems as well. On the other hand, the significance of a new theory is testified by the previously open questions it enables to answer.

There is, however, another kind of mathematical activity which is no less important than solving problems or building theories but can hardly be recognized by awards: the setting up of conjectures. Opening a new circle of problems or remarking the possibility of generalizing a known theorem, testing the possible statements through examples and counter-examples or developing research programs to attack them often require more invention than the solution of an already well-formulated problem. Those who are daydreaming about such new research programs sometimes determine the direction of research for decades to come, but since they are offering daydreams instead of concrete results, it is hard to award them with a prize for this activity. For example, no Fields Medal was attributed to *Robert Langlands* or *Alexander Beilinson,* to name but two among the most influential mathematicians of the past decades, although their importance cannot be better demonstrated than by the fact that the laureates of the year 2002, *Laurent Lafforgue* and *Vladimir Voevodsky* were recognized for their breakthrough in realizing these mathematicians' programs.

## Some prize-winning results

In this last section we present a few results whose authors have been distinguished by the Fields Medal. Of course, we are by no means claiming that these are the most important among the many important theorems that were given this honour. Rather, we have selected results from different branches of mathematics which can be briefly presented by elementary means.

• The seventh among the 23 celebrated problems put forward by David Hilbert in 1900 concerns *algebraic numbers.* A real or complex number $\alpha$ is called algebraic if there exists a one-variable polynomial with rational coefficients of which $\alpha$ is a root. For example, $\sqrt{2}$ is an algebraic number, being a root of the polynomial $x^2-2$. Those numbers which are not algebraic are called *transcendental.* Two of the great achievements of 19th century mathematics were the proof of the transcendence of $e$ by Hermite in 1873, and of that of $\pi$ by Lindemann in 1882. As it can be shown that sums, products, quotients and even rational powers of algebraic integers are themselves algebraic, one of the next interesting questions that arise is to decide whether $2^{\sqrt{2}}$ is transcendental. Hilbert gave a more general formulation: he conjectured that if $\alpha$ is an algebraic number different from 0 and 1, and if $\beta$ is an irrational algebraic number, then the power $\alpha^\beta$ is always transcendental. Hilbert thought that this problem was very hard and even believed that (unlike the Riemann Hypothesis and Fermat's Last Theorem) even the youngest of his generation won't see its solution. However, the transcendence of $2^{\sqrt{2}}$ was established by A. O. Gelfond as early as 1929, and then in 1934 the general conjecture was also settled by him and, independently, by Th. Schneider. By taking the natural logarithm we can reformulate the Gelfond-Schneider theorem as the statement asserting that if $\alpha_1, \alpha_2$ are algebraic numbers different from 0 and 1, there is no irrational number $\beta$ for which $\log\alpha_1+\beta\log\alpha_2=0$. *Alan Baker* received the Fields Medal for the following far-reaching generalisation:

*If $\alpha_1,\dots,\alpha_n$ are algebraic numbers different from 0 and 1 and $\beta_1,\dots,\beta_n$ are algebraic numbers different from 0 such that*

$$\beta_1\log\alpha_1+\cdots+\beta_n\log\alpha_n=0,$$

*then there exist* rational *numbers $\gamma_1,\dots,\gamma_n$, not all equal to 0, for which*

$$\gamma_1\log\alpha_1+\cdots+\gamma_n\log\alpha_n=0.$$

From this theorem one can derive for instance the transcendence of the products $e^\beta\alpha_1^{\beta_1}\dots\alpha_n^{\beta_n}$ for arbitrary nonzero algebraic numbers $\alpha_1,\dots,\alpha_n,\beta_1,\dots,\beta_n,\beta$. The theorem also has important applications in the theory of diophantine equations.

• The following interesting problem was put forward by Paul Erdős and Paul Turán in 1936:

*Let $k>2$ be an integer and $0<\delta<1$ a real number. Prove that there exists a positive integer $N_0$ (depending on $k$ and $\delta$), such that for any $N\geq N_0$ an arbitrary subset of the set $\{1,2,3,\dots,N\}$ whose cardinality is at least $\delta N$ contains an aritmetic progression of length $k$.*

In other words, if a subset of the set $\{1,2,3,\dots,N\}$ is „not too sparse", then for $N$ large enough it must contain an arithmetic progression of length $k$. The question is difficult already for $k=3$; in fact, the solution of the problem in this special case was one of the results for which *K. F. Roth* received the Fields Medal. (The other one was a basic theorem from the field of diophantine approximation.) But the general case is much more difficult, and the first to tackle it was the Hungarian mathematician Endre Szemerédi, by means of a very ingenious and intricate combinatorial construction. Then in 1977, with the help of methods from ergodic theory, Furstenberg found a simpler but less elementary proof. A shortcoming of both proofs is, however, that neither of them reveals much about the bound $N_0$: Furstenberg only proves its existence, and the estimate obtained by Szemerédi is very high, in contrast to the much finer bounds obtained by Roth in the case $k = 3$. The new proof by *Timothy Gowers* is more elementary than that of Furstenberg, less complicated than that Szemerédi, and gives a much better bound. It was for this, and for results obtained in the theory of so-called Banach spaces, that he obtained the medal.

• *Paul Cohen* solved the most famous open problem of set theory, going back to the founding father of the field, Georg Cantor. It was Cantor who first gave a rigourous definition for the *cardinality* of a set: according to him, two sets have the same cardinality if there exists a one-to-one correspondence between their elements. Infinite sets always contain a proper subset that has the same cardinality: one of the earliest examples for this, pointed out by Cantor himself, is the case of the rationals and the integers. It was also Cantor who showed that the set of real numbers has cardinality strictly greater than that of the rationals, and he conjectured that there is no infinite set whose cardinality is strictly larger than that of the rationals but strictly smaller than that of the reals. Kurt Gödel showed in 1939 that this assumption does not contradict the universally accepted axioms of set theory due to Zermelo and Frenkel. But this was not enough to settle the question for it only meant that there exists a mathematical logical model in which the axioms of Zermelo and Frenkel are satisfied and Cantor's conjecture is true. Then in 1961 Cohen constructed a model in which the Zermelo–Frenkel axioms hold but there exists a set of cardinality larger than that of the rationals and smaller than that of the reals. This theorem is a milestone in the history of science for it gives the first example for a famous open question that cannot be decided within the framework of a given system of axioms. On the other hand, Cohen used in the construction of his logical model a new method called „forcing" whose development opened up the way for very fruitful further reseach.

• A basic problem in the theory of diophantine equations is to find those triples $(x, y, z)$ of rational numbers that are not all 0 and that satisfy a given homogenous polynomial equation $f(x, y, z) = 0$. As examples, we may mention the „Fermat-type" equations $x^n + y^n - z^n = 0$ or the quadratic equation $y^2 - xz = 0$. The equation $f = 0$ defines an algebraic curve in the projective plane (for instance, in the case of the equation $y^2 - xz = 0$ we get a conic), and the triples $(x, y, z)$ that we are looking for give rise to points on the curve with rational coordinates. It is customary to assume that the curve is *smooth,* which means that one can draw a single tangent line to it in each of its points. If the polynomial $f$ has degree one, the rational points are easy to find. The quadratic case is not much more difficult: if there exists a rational point at all, then there are infinitely many of them, and they can be projected from a given point to the projective line. The case of degree three belongs to the theory of so-called elliptic curves: here it may happen that there are no rational points, or that they are finite in number but also that there are infinitely many. In the latter case, a basic theorem proven by Mordell in 1930 provides a description of their structure. It was also Mordell who first speculated about the higher degree cases. Here of course one should exclude those curves which can be transformed into a curve of degree at most three by a rational change of coordinates. Of the remaining curves of degree at most four Mordell conjectured that *they always have finitely many rational points.* As this conjecture was not corroborated by many numerical examples, it caused universal surprise when it was proven by *Gerd Faltings* in 1983. The proof used difficult methods from algebraic geometry, but later Faltings (building upon ideas by Paul Vojta and Enrico Bombieri) gave a second, more elementary but more computational proof. In any case, he found the second proof sufficiently elementary so that he could remark in his customary sarcastic style: *Nowadays anyone can prove Mordell's conjecture.*

• One of the most important areas in modern algebra is the theory of *groups.* A group is a set endowed with a two-variable associative operation (usually called multiplication) having two important properties: *a*) there exists a unit element, i.e. an element $e$ for which $eg = ge = g$ for any element $g$ of the group; *b*) any element $g$ has an inverse, i.e. an element $g^{-1}$ satisfying $gg^{-1} = g^{-1}g = e$. As examples of groups one may consider the set of integers with respect to the addition law (but not multiplication!), the set of nonzero rational numbers with respect to multiplication, isometries of the plane or the space with respect to the composition of isometries, or — to give an example of a finite group — symmetries of a (fixed) regular polygon or polyhedron, also with respect to composition. In the study of groups a basic role is played by so-called *normal subgroups.* A subset $H$ of a group $G$ is called a normal subgroup if it contains products and inverses of any two of its elements and moreover for any $g \in G$ and $h \in H$ it contains the element $ghg^{-1}$. In 1963 Walter Feit and *John Thompson* proved an open conjecture of Burnside formulated several decades before, which asserts that *any finite group $G$ whose cardinality is an odd composite integer contains a normal subgroup other than $G$ itself or the one-element subgroup formed by the unit element.* This result is of crucial importance in the classification of finite groups for it reduces the study of „larger" groups to that of „smaller" building blocks. Thompson was awarded the Fields Medal for this theorem together with other important results that he obtained in the theory of finite groups.

• Our last example comes from the field of topology and is intended for those familiar with the rudiments of this theory. Recall that a *d-dimensional topological manifold* is a connected topological space in which each point has an open neighbourhood admitting a homeomorphism (i.e. a bijection continuous in both directions) onto an open subset of Euclidean $d$-space. One of the basic problems of topology is the classification of manifolds. One may try to classify them up to homeomorphism but it is at least as interesting to consider the weaker problem of classifying them according to their *homotopy type.* Intuitively, two manifolds have the same homotopy type if they can be continuously deformed into each other. For instance, the disk or any convex figure in the plane has the homotopy type of a point (because they are continuously contractible). [The precise definition is the following: two continuous maps $f, g\colon X \to Y$ are *homotopic* (denote this by $f \sim g$) if there exists a continuous map $h\colon X \times [0,1] \to Y$ satisfying $h(x,0) = f(x)$ and $h(x,1) = g(x)$ for any $x \in X$. This being said, two manifolds $M$ and $N$ have the same homotopy type if there exist continuous maps $F\colon M \to N$ and $G\colon N \to M$ with $G \circ F \sim \mathrm{id}_M$ and $F \circ G \sim \mathrm{id}_N$.] The *generalised Poincaré conjecture* predicts that any compact manifold having the homotopy type of the $d$-dimensional sphere $S^d$ is in fact homeomorphic to $S^d$. It is not very difficult to verify the case $d = 2$ of this conjecture, but the case $d = 3$ (the original conjecture of Poincaré) is still an open problem today. Therefore it may sound surprising that in 1982 *Michael Freedman* managed to prove the case $d = 4$ of the conjecture; moreover, he was able to classify all so-called simply connected compact 4-dimensional manifolds. Among the Fields Medalists the work of *Stephen Smale* is also related to the Poincaré conjecture: he received the honour for his proof of the conjecture for any manifold of dimension $d > 4$ admitting a differentiable structure. However, the 3-dimensional case still resists all attempts, so it is no wonder that in the year 2000 the Clay Foundation listed it among the seven Millennium Problems for whose solution a cash prize of 1 000 000 dollars is awarded. Therefore if one of the readers happens to solve this 100-year-old problem before the age of 40, he/she can safely expect to become both a Fields Medalist and a millionaire.

*Added in proof:* Life may spoil the best of jokes. After the Hungarian version of this article was published, G. Perelman announced a proof of the Poincaré Conjecture.