We Do Not Choose Mathematics as Our Profession, It Chooses Us: Interview with Yuri Manin

Mikhail Gelfand

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Gelfand: *Has the style of mathematical research changed in the past fifty years?*

Manin: Individual or societal?

Gelfand: *Either.*

Manin: I think that people engaged in research in mathematics today are doing so the same way it was done 200 years ago. This is partly because we don't choose mathematics as our profession, but rather it chooses us. And it chooses a certain type of person, of which there are no more than several thousand in each generation, worldwide. And they all carry the stamp of those sorts of people mathematics has chosen.

The social style has changed, in the sense that social institutions have changed within which one studies mathematics. This evolution was not unusual. There was the period of Newton, later of Lagrange and so forth, when academies and universities were being formed, when individual mathematical amateurs, who once studied alchemy or astrology in the same way, by exchanging letters, started forming social structures. (I omit the period of antiquity, whose natural development was interrupted in Europe during the first thousand years of Christianity.) Then came the scientific journals. This all was put in place 300 years ago. In the last half of the twentieth century, computers have contributed to this development.

Gelfand: *But between Newton and Lagrange, and the second half of the twentieth century, nothing significant changed?*

Manin: No. This social system was consolidated, academies plus universities plus journals. These developed bit by bit and assumed the form in which we now know them. Take for example the first volume of *Crelle's Journal* (*Journal of Pure and Applied Mathematics*), which came out in 1826—well, it doesn't differ at all from a contemporary journal. Abel's article appeared there, on the unsolvability in radicals of the general equation of degree higher than four. A wonderful article! As a member of the editorial board of *Crelle*, I would accept it even today with great pleasure.

In the last few decades, the interface between society and professional mathematicians has changed. This interface now embraces computer folks and people around them, including various PR people whom we need because of new methods

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of financing our work, related to proposals, grants, and things like that. In mathematics this looks odd—you must first write just what it is you are doing that is so great, then later give an accounting of what you've accomplished.

Gelfand: *A student of Kantorovich*[1] *used to tell how in a midyear report Kantorovich wrote, with a straight face, "The theorem is 50 percent proven."*

Manin: In the Mathematics Institute in Moscow there was a clear-cut system: I would write that I was planning to prove the theorems that in fact were proven in the past year. Then I had a whole year to continue my work.

But these are all trifles. So long as mathematics chooses us, and so long as there are people such as Grigory Perelman and Alexander Grothendieck, we will remember our ideals.

Gelfand: *Yes, grants in mathematics are something very odd. On the other hand, if we don't have grants, what other mechanisms might there be?*

Manin: Well, what do we need? Salaries for people and a budget for the institution. I was lucky, I worked for a salary and on a budget, not just in Moscow, but in Bonn for fifteen years. I don't see anything bad in that.

But the fact that the organizations that provide these salaries and budgets have decided to adopt the marketplace language is another thing entirely. The marketplace debases three areas: health care, education, and culture. Roger Bacon keenly spoke about the "idols of the marketplace" fallacy. Mathematics is a part of culture, in the broad sense of that term, and not part of industry or services or something of that sort.

Gelfand: *But won't market-free methods lead to stagnation, so that there will be no progress?*

Manin: Up to now there has been no stagnation.

Gelfand: *What you talk about is possible for mathematics, because mathematics is an inexpensive science.*

Manin: Exactly. I always say, "Why should we put ourselves on the market? We (a) don't cost anything, and (b) don't use up natural resources and don't spoil the environment." Give us salaries, and leave us in peace. I don't wish to generalize at all: I speak only of mathematics.

Gelfand: *You mentioned computers. What has changed in mathematics since their appearance?*

Manin: What has changed in pure mathematics? The unique possibility of doing large-scale physical experiments in mental reality arose. We can try the most improbable things. More exactly, not the most improbable things, but things that Euler could do even without a computer. Gauss could also do them. But now, what Euler and Gauss could do, any mathematician can do, sitting at his desk. So if he doesn't have the imagination to distinguish some features of this Platonic reality, he can experiment. If some bright idea occurs to him that something is equal to something else, he can sit and sit and compute a value, a second value, a third, a millionth. Not only that. People have now emerged who have mathematical minds, but are computer oriented. More precisely, these sorts of people were around earlier, but, without computers, somehow something was missing. In a sense, Euler was like that, to the extent that he was just a mathematician—he was much more than just a mathematician—but Euler the mathematician would have taken to computers passionately. And also Ramanujan, a person who didn't even really know mathematics. Or, for instance, my colleague here at the institute, Don Zagier. He has a natural and great mathematical mind, which is at the same time ideally suited to work with computers. Computers help him study this Platonic reality, and, I might add, quite effectively.

Photo courtesy of Xenia Semenova.

Yuri Manin, Cinque Terre, Italy, 1994.

I myself am not this sort of person at all, but I understand what this is about and would be glad to have collaborators who might help me in this. So this is what computers have done for pure mathematics.

Gelfand: *What about the relationship between mathematics and theoretical physics? How is that structured?*

Manin: This relationship has changed during my own lifetime.

It is important to note that in the time of Newton, Euler, Lagrange, Gauss, the relationship was so close that the same people did research in both mathematics and theoretical physics. They might have considered themselves more as mathematicians or more as physicists, but they were exactly the same people. This lasted until about the end of the nineteenth century. The twentieth century revealed significant differences. The story of the development of the general theory of relativity is a striking example. Not only did Einstein not know the mathematics he needed, but he didn't even know that such mathematics existed when he started understanding the general theory of relativity in 1907 in his own brilliantly intuitive language. After several years dedicated to the study of quanta, he returned to gravitation and in 1912 wrote to his friend Marcel Grossmann: "You've got to help me, or I will go out of my mind!" Their first article was called "A sketch of a theory of general

[1] *L. V. Kantorovich (1912–1986), Soviet mathematician and economist, Nobel Prize in Economics 1975.*

relativity and a theory of gravity: I. Physics Part by Albert Einstein; II. Mathematics Part by Marcel Grossmann."

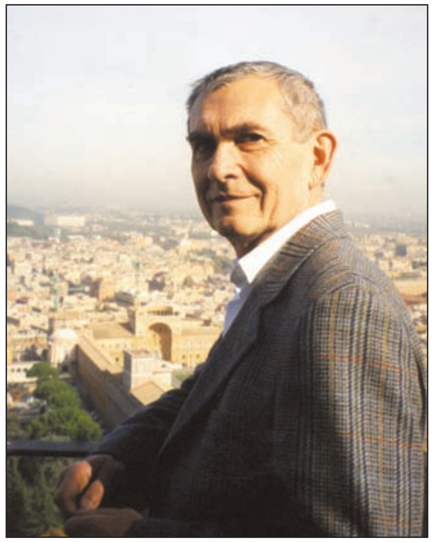

Photo courtesy of Xenia Semenova.

Manin, in front of the panorama of Rome from the gallery of San Pietro, 1998.

This attempt was half successful. They found the right language but had not yet found the right equations. In 1915 the right equations were found by Einstein and David Hilbert. Hilbert derived them by finding the right Lagrangian density—the importance of this problem, it seems, for some time eluded Einstein as well. It was a great collaboration of two great minds that unfortunately prompted historians to start silly fights about priorities. The creators themselves have been grateful and generous in recognizing each other's insights.

For me, this story marks the period in which mathematics and physics parted ways. This divergence continued until about the 1950s. The physicists dreamed up quantum mechanics, in which they found a need for Hilbert space, Schrödinger's equations, the quantum of action, the uncertainty principle, the delta function. This was a completely new type of physics and a completely new type of philosophy. Whatever pieces of mathematics were necessary—they developed them themselves.

Meanwhile, the mathematicians did analysis, geometry, started creating topology and functional analysis. The important thing at the beginning of the century was the pressure by philosophers and logicians, trying to clarify and "purify" the insights of Cantor, Zermelo, Whitehead, et al., about sets and infinity. Somewhat paradoxically, this line of thought generated both what came to be known as the "crisis in foundations" and, somewhat later, computer science. The paradox of a finite language that can give us information about infinite things—is this possible? Formal languages, models and truth, consistency, (in)completeness—very important things were developed, but quite disjoint from physicists' preoccupations of that time.

And Alan Turing appeared, to tell us: "The model of a mathematical deduction is a machine, not a text." A machine! Brilliant. In ten years, we had von Neumann machines and the principle of separation of programs (software) and hardware. Twenty years more—and everything was ready.

During the first third of the century, except for particular minds—von Neumann was undoubtedly both a physicist and a mathematician, and I know of no other person with a mind on that scale in the twentieth century—mathematics and physics developed in parallel and after a while stopped taking notice of each other. In the 1940s Feynman wrote about his wonderful path integral, a new means of quantifying things, and worked on it in a startlingly mathematical way—imagine something like the Eiffel Tower, hanging in the air with no foundation, from a mathematical point of view. So it exists and works just right, but standing on nothing we know of. This situation continues to this very day. Then, in the 1950s the quantum field theory of nuclear forces started to appear, and it turned out that mathematically the respective classical fields are connection forms. The classical equation of stationary action for them was known in differential geometry. The equation of Yang-Mills entered the scene, mathematicians began to look askance at the physicists, and the physicists at the mathematicians. It turned out, paradoxically—and for me pleasantly—that we began to learn more from the physicists than they learned from us. It turned out that with the help of quantum field theory and the apparatus of the Feynman integral they developed cognitive tools that allowed them to discover one mathematical fact after another. These weren't proofs, just discoveries. Later the mathematicians sat themselves down, scratched their heads, and reshaped some of these discoveries in the form of theorems and began trying to prove them in our honest manner. This shows that what the physicists do is indeed mathematically meaningful. And the physicists say, "We always knew that, but of course, thanks for your attention." But in general, as a result, we learned from the physicists what questions to ask, and what answers we might presuppose—as a rule, they turn out to be correct. The renowned physicist and mathematician Freeman Dyson in his Gibbs lectures "Missed opportunities" (1972) has beautifully described many cases when "mathematicians and physicists lost chances of making discoveries by neglecting to talk to each other." Especially striking for me was his revelation that he himself "missed the opportunity of discovering a deeper connection between modular forms and Lie algebras, just because the number theorist Dyson and the physicist Dyson were not speaking to each other."

Then Witten appeared, with his unique gift for the production of glorious mathematics from this very Eiffel Tower that hangs in the air. I looked in Wikipedia: before getting his Ph.D. in physics in 1976, when he was twenty-five, he was planning to engage in political journalism, then economics… until he finally heard the call of mathematics and physics.

He is the master of such astonishing mental equipment, which produces mathematics of unlikely strength and force, but arising from physical insights. And the starting point of his insights is not the physical world, as it is described by experimental physics, but the mental machinery developed for the explanation of this world by Feynman, Dyson, Schwinger, Tomonaga, and many other physicists—machinery that is entirely mathematical but that has very weak mathematical foundation. It is such an earthshaking heuristic principle, not at all some triviality, but, I must say again, an enormous structure without a foundation, at least of the kind we have gotten accustomed to.

Gelfand: *So has everyone grown used to the fact that there is no foundation, and lived with it, or are they trying to build a foundation?*

Manin: None of the attempts that have been made have succeeded in sufficient generality. Mathematicians have developed a few approximations to what we might call the Feynman integral; for example, Wiener integration, which was invented as early as the 1920s. It was used to study Brownian motion, where there is a rigorous mathematical theory. There are also some interesting variants, but the theory is much more narrow than is required to cover all varied applications of the Feynman integral. You see, as a mathematical theory it's small—in strength or power it is not comparable to the machinery that now produces really great mathematics.

I don't know what will happen with the machinery when Witten stops working on it, but I very much hope that it will soon permeate the mathematical world. A small industry has arisen whose goal is to prove the theorems that Witten guessed, in particular, in the so-called Topological Quantum Field Theory (TQFT), and its output is ample and well known.

Actually, homotopical topology and TQFT have grown so close that I have started thinking that they are turning into the language of new foundations.

Such things have already occurred. Cantor's theory of the infinite had no basis in the older mathematics. You can argue about this as you like, but this was a new mathematics, a new way to think about mathematics, a new way to produce mathematics. In the final analysis, despite the arguments, the contradictions, Cantor's universe was accepted by Bourbaki without apology. They created "pragmatic foundations", adopted for many decades by all working mathematicians, as opposed to "normative foundations" that logicists or constructivists tried to impose upon us.

Gelfand: *It seems that mathematicians writing about Bourbaki in Russian have different points of view. There are rather harsh critics of all this set-theoretic foundational work, who criticize Bourbaki's isolation from the physicists and the wonderful possibilities they can open for us.*

Manin: There is nothing special in this. The fact that they curse at Bourbaki shows that they don't know how things are now done. What Bourbaki did was to take a historical step, just as what Cantor himself did. But this step, while it played an enormous role, is very simple—it was not creating the philosophical foundations of mathematics, but rather developing a universal common mathematical language, which could be used for discussion by probabilists, topologists, specialists in graph theory or in functional analysis or in algebraic geometry, and by logicians as well.

You start with a few common elementary words, "set, element, subset…", then you build up definitions of the basic structures that you study, "group, topological space, formal language… ". Their names form the second layer of your own terminology. There might come the third, fourth, or fifth layer, but basic construction rules are common, and getting together, people could talk to each other with complete understanding: "Formal language is a set of letters, plus a subset of well-formed words—terms, plus connectives and quantifiers, plus deduction rules… ." From this perspective, Gödel's incompleteness theorem, for example, loses any sort of mystery. The theorem acquires its mystery when you start examining it philosophically, but actually, it is simply a theorem stating that a certain structure does not have finitely many generators. Oh, my God! Such structures are a penny a pound, but just think, here is one more. The profundity appears when we add to this a particular self-referential semantics. Then it enters the philosophical foundation of mathematics.

So Bourbaki in fact did something completely different from what these guys think. (I omit here any discussion of Bourbaki's influence on mathematics education in France: as with all sociological questions, this may arouse a chorus of controversy in any audience.)

Gelfand: *What is the status of hypotheses in mathematics? For example, Fermat's Last Theorem—in recent years no one has been trying to find a counterexample: everyone understood that it was correct and that one must try to prove it. And there are many such well-known propositions, especially in number theory.*

Manin: Here I take a position that sets me apart from many good colleagues. I've heard many arguments against me on this subject. I must explain to you how I imagine mathematics. I am an emotional Platonist (not a rational one: there are no rational arguments in favor of Platonism). Somehow or other, for me mathematical research is a discovery, not an invention. I imagine for myself a great castle, or something like that, and you gradually start seeing its contours through the deep mist, and begin to investigate something. How you formulate what it is you've seen depends on your type of thinking and on the scale of what you have seen, and on the social circumstances around you, and so on.

What you have seen can be formulated as the presence or absence of something. Look at $x^2 + y^2 = z^2$. It is wonderful that we can write down all the integral solutions in one formula—in a certain sense this was known to Diophantus. When you've done this, it raises a question: Fine, but what about cubes? You search and search, and there are none. Hmm. How strange. And if we ask about fourth powers? Hmm. Again nothing. Well, can it be that there is never anything further? And so you discover a difference between the second power and the third, fourth, and so on. This history of Fermat's Last Theorem, well, it is that sort of history. But when you pose a problem, that this-and-this is equal to that-and-that, or that such-and-such never happens, you never know in advance if you have a good problem or a bad one—not until it is solved or almost solved.

Problems have qualities. In number theory, there are many problems that can be formulated in elementary terms, and we know that Fermat's Last Theorem was a wonderful problem. We know this because, throughout its history, from its statement to its solution, it turned out to be connected to a host of things that were not connected to each other a priori. And for its solution, it was necessary to investigate these fundamental things. The problem turned out to be a detail in an enormous edifice.

But we can take other problems, say those concerning perfect numbers or twin primes. Are there infinitely many perfect numbers, that is, numbers that are equal to the sum of their divisors? Or infinitely many pairs of primes whose difference is 2? To this day, no one has built any interesting theory around these problems, although their statements look no worse than that of Fermat's Last Theorem.

Gelfand: *Are these properties of the problems themselves, or is it just that no one is actively investigating them, for some social reason?*

Manin: As a Platonist, I *know* that this is a property of the problems themselves, but it is a property that one cannot recognize at the moment of formulating the problem. It reveals itself in the process of historical development.

Partly for this reason, I am not partial to problems. Solving a problem requires the skill of finding a detail, but you don't know what it is a detail of. As a Platonist, I am partial to complete programs. A program arises when a great mathematical mind sees something as a whole, or not as a whole, but as something more than a single detail. But it is seen at first only vaguely.

Gelfand: *That is, instead of a single distinct detail, you vaguely see a whole building.*

Manin: Yes. And so you begin to blow away the mists, to find appropriate telescopes, seek analogies with edifices that have been discovered before, create a language for the things that you see so vaguely, and so on. This is what I would call, tentatively, a program.

Cantor's theory of the infinite was such a program. It was a rare event: it was at once a program and a discovery, that there were orders of infinity. And, say, the continuum hypothesis—whether there is something between the countable infinity and the continuum—is a question that has turned out to be less important than many other questions, but very stimulating. If Cantor had asked only about this—it would have been bad. Its significance would have been discovered only in the future. But he did considerably more right away; he started a whole program of investigation.

Weil's hypothesis, about how many solutions there are to an equation modulo p, is such a program, which became well known during my lifetime. He immediately saw a striking analogy: in the areas where he was looking, there was a gap, but in other places there was an entire theory, (co)homology theory, implying the Lefschetz theorem on fixed points of maps. Half of Grothendieck's life, and of several people around him including Pierre Deligne, was devoted to filling this gap. They filled the gap, the analogy became precise, and modern algebraic geometry was born. And much more has happened as a result: set theory as *the* language of contemporary mathematics started to recede, and categories, with all subsequent superstructures, started to replace sets in their old function.

In logic, there was Hilbert's program, except that he formulated it too optimistically. He wanted to prove that everything true was provable. He saw the contours of the edifice inaccurately, but the program developed anyway. Gödel, Turing, Church, von Neumann, computers, and computer science—to a great degree this originated with Hilbert.

The four-color problem is for me an example of a bad problem which didn't lead to a program. It was proved with the aid of computers so that to this day swords are crossed over it. But that's not so important as the fact that until now no one has incorporated it into any sort of sufficiently rich context. So it is simply a means of training the mind.

For these reasons, I generally don't like problems as such. But when a problem arises within a program—that's when it can be a good one, when we know in advance to what edifice this detail belongs. The Riemann Hypothesis, without a doubt, is a problem that Riemann originated within a program, although during the course of a century and a half, the narrow number theorists continued to look at it as a very important isolated challenge. I'm somewhat apprehensive that its first solution might be a proof using blunt analytic methods. It will receive every imaginable prize, the solution will be acclaimed in every newspaper in the world, and all of this will be misleading because the "right" solution should be given in a wider context, which we already know. We even know several approaches to a solution. Nevertheless, it is quite possible that the first solution will be a poor and uninteresting one.

Gelfand: *Are there hypotheses that everyone had grown used to and assumed to be obviously correct, but then counterexamples were found?*

Manin: I don't think I know of any long-standing hypotheses that people believed, but then found counterexamples.

Gefand: *If someone found a counterexample to Fermat's Last Theorem, rather than a proof, would this be a great earthshaking event? Or would it simply mean that the problem was not a good one?*

The problem would still have been a good one, because it stimulated the development of a context. And then someone solves it within this context. The answer could be positive or negative—this second question is less significant. The significance of the question is that it helped to establish an important context.

If a counterexample had been found before the 1960s, everyone would have been scratching their heads. If a counterexample had been found somewhere in the 1970s, it would have been very interesting and somewhat shattering, because by that time it had become clear that Fermat's Last Theorem could be deduced from several other conjectures that are far from simple and that had a more far-reaching character, related to the Langlands program. By then it was known that if these things were true, then so was Fermat's Last Theorem. Of course if a counterexample to Fermat's Last Theorem had been found, then these things would have to be false. And this would have meant the destruction of a much more fundamental and complex system of belief. It would have evoked enormous interest and attempts at understanding what was amiss, we would have to rebuild a lot of the edifice, and so on. All that would have followed from the discovery of a counterexample.

Gelfand: *Have there been such strong counterexamples in history? Perhaps Gödel's Theorem? Before that it was supposed that one could prove everything that is true.*

Manin: Hilbert believed this, and I don't know how many others believed it. But this shows that you must view this program correctly. Its first important outcome was the construction of a mathematical context in which one could formulate questions about truth and provability in mathematics as precise mathematical problems rather than vague philosophical ones. By the nature of this quest, one has to introduce self-referentiality, and the rest becomes the matter of inventiveness, brilliantly demonstrated by Tarski and Gödel.

At the start of the formulation of the program, people made wrong guesses about what it would lead to, and the counterexamples showed that these were in fact errors.

Gelfand: *Were there other interesting wrong perceptions?*

Manin: There were some showing a lack of human imagination. In the history of mathematics, such things are not usually called counterexamples, but paradoxes. Take for instance the theorem of Banach-Tarski. You start with a ball, and it turns out that you can cut it into five pieces, rearrange them, put them back together, and you obtain two balls of the same size as the initial one. This construction tells us a lot. For example, to the critics of the set-theoretic approach in general, it means that if this view leads one to such an assertion, then it is not mathematics, but some sort of wild nonsense. For logicians it is an example of a paradoxical application of the axiom of choice of Zermelo and so an argument against accepting it. And aside from all this, it is very beautiful geometry. Once I was asked to deliver a lecture for the general public in an art museum, and I decided that the Banach-Tarski paradox is a great subject for the presentation "The Abstract Art of Mathematics". The key point was that we must not imagine "pieces" as solid material objects, but rather clouds of points. We must imagine that a ball consists of indivisible points. You are allowed to call a "piece" any subset of these points, you can move it and turn it around, but only as a whole, moving it as a single object, so that the pairwise distances between points remain the same. So you split the sphere not into solid pieces, but into five clouds. And these clouds can mutually penetrate each other; in fact, there's nothing solid about them. They have no volume, no weight, they are wonderful objects of a highly trained imagination.

Why is there no obvious contradiction? Isn't it true that two balls contain more points than each one? No, the infinite number of points is exactly the same, that's easy to prove. I explained this to my grandson, that there are as many points in a sheet of paper as there are on the wall of the room. "Take the sheet of paper, and hold it so that it blocks your view of the wall completely. The paper hides the wall from your sight. Now if a beam of light comes out of every point on the wall and lands in your eye, it must pass through the sheet of paper. Each point on the wall corresponds to a point on the sheet of paper, so there must be the same number of each."

The message here is that if you make a dust of individual points out of your initial ball, there will be enough points to fill two, or three, or even an infinity of balls of arbitrary sizes. The difficulty arises when you try to define clouds of points that you will have to move and turn and rearrange into two balls leaving no gaps. This is mathematical trickery, very beautiful, but if you want to explain it well, you need much more time.

So it's not a counterexample, but a paradox baffling an untrained imagination.

Several such paradoxes were discovered during the time of transition between classical mathematics and set theoretic mathematics. There was the theorem that a curve could fill the square. There were many such things, and they taught us a lot.

Many people thought that this was pure fantasy, but the newly trained imagination allowed one to recognize "paradoxical" behavior of Fourier series, to understand Brownian motion, then to invent wavelets, and it turned out that these were not at all fantasies but almost applied mathematics.

Gelfand: *So what will happen in the next twenty years?*

Manin: I don't foresee any revolutionary changes, because in my view there have been none in the last 300 years. Every time new and powerful intuitions arose, mathematics retained its character, in some strange way. This is also a theme of a lecture, one I've not given. I would like to show the development of the idea of the integers from the most remote times to Kolmogorov complexity, and all this can be done almost without appealing to new mathematics. One and the same idea persists. It changes a bit in one era or another, its verbal casing changes. But all the same it stays completely invariant and so lives on. Nothing is forgotten.

And so I don't foresee anything extraordinary in the next twenty years. Probably, a rebuilding of what I call the "pragmatic foundations of mathematics" will continue. By this I mean simply a codification of efficient new intuitive tools, such as Feynman path integrals, higher categories, the "brave new algebra" of homotopy theorists, as well as emerging new value systems and accepted forms of presenting results that exist in the minds and research papers of working mathematicians here and now, at each particular time.

When "pragmatic foundations" of mathematics are made explicit, usually in several variants, the advocates of different versions may start quarreling, but to the extent that it all exists in the brains of the working generation of mathematicians, there is always something they have in common. So, after Cantor and Bourbaki, no matter what we say, set theoretic mathematics resides in our brains. When I first start talking about something, I explain it in terms of Bourbaki-like structures: topological spaces, linear spaces, the field of real numbers, finite algebraic extensions, fundamental groups. I cannot do otherwise. If I'm thinking of something completely new, I say that it is a set with such-and-such a structure; there was one like this before, called this-and-that; another similar one was called this-and-this; so I apply slightly different axioms, and I will call it such-and-such. When you start talking, you start with this. That is, at first we start with the discrete sets of Cantor, upon which we impose something more in the style of Bourbaki.

But fundamental psychological changes also occur. Nowadays these changes take the form of complicated theories and theorems, through which it turns out that the place of old forms and structures, for example, the natural numbers, is taken by some geometric, right-brain objects.

Instead of sets, clouds of discrete elements, we envisage some sorts of vague spaces, which can be very severely deformed, mapped one to another, and all the while the specific space is not important, but only the space up to deformation. If we really want to return to discrete objects, we see continuous components, the pieces whose form or even dimension does not matter. Earlier, all these spaces were thought of as Cantor sets with topology, their maps were Cantor maps, some of them were homotopies that should have been factored out, and so on.

I am pretty strongly convinced that there is an ongoing reversal in the collective consciousness of mathematicians: the right hemispherical and homotopical picture of the world becomes the basic intuition, and if you want to get a discrete set, then you pass to the set of connected components of a space defined only up to homotopy.

That is, the Cantor points become continuous components, or attractors, and so on—almost from the start. Cantor's problems of the infinite recede to the background: from the very start, our images are so infinite that if you want to make something finite out of them, you must divide them by another infinity.

This is parallel to the way we envisage a Feynman integral. At first it is just a hieroglyph charged with an interpretational challenge. The first two, three, four steps of interpretation are all ad hoc, appealing to various analogies with other cases where the mathematics is clean ("toy models"). At a certain stage you may get a formal series that doesn't just diverge, but consists of terms that are themselves divergent (although finite-dimensional) integrals. Then you artificially regularize each term, making it finite. But the series, in general, still diverges. So you invent an interpretation of the series. And finally, having forced your way through a crowd of infinities, you obtain a finite answer. As a bonus, you get a series of marvelous mathematical theorems. I see in this an analogy with a rebuilding of pragmatic foundations in terms of category theory and homotopic topology.