Yu.I.Manin. Good Proofs are Proofs that Make us Wiser

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" **YURI I. MANIN**

'Good Proofs are Proofs that Make us Wiser'

Interview by Martin Aigner and Vasco A. Schmidt

This year's International Congress is the last ICM in this century. Do you think a Hilbert is still possible? Are there any contemporary problems corresponding to Hilbert's Problems?

I do not actually believe that Hilbert's list had a great role in the mathematics of this century. It certainly was psychologically important for many mathematicians. For example Arnold told that while being a young graduate student he had copied the list of Hilbert's problems in his notebook and always kept it with him. But when Gelfand learnt about that, he actually mocked Arnold on this. Arnold saw problem solving as an essential part of great mathematical achievements. For me it's different. I see the process of mathematical creation as a kind of recognizing a preexisting pattern. When you study something — topology, probability, number theory, whatever — first you acquire a general vision of the vast territory, then you focus on a part of it. Later you try to recognize "what is there?" and "what has already been seen by other people?". So you can read other papers and finally start discerning something nobody has seen before you.

Is the emphasis on problem solving a kind of romantic view: a great hero who conquers the mountain?

Yes, somehow a kind of sportive view. I don't say it is irrelevant. It is quite important for young persons, as a psychological device to lure young people to create some social recognition for great achievements. A good problem is an embodyment of a vision of a great mathematical mind, which could not see the ways leading to some height but which recognized that there is a mountain. But it is not the way to see mathematics, nor the way to present mathematics to a general public. And it is not the essence. Especially when such problems are put in a list, it is something like a list of capitals of great countries of the world: it conveys the minimal possible information at all. I do not actually believe that Hilbert thought this is the way to organize mathematics.

Would you venture to predict some dominant patterns of mathematics in the next century?

This is very difficult. I think the mathematics of the 20th century is best presented around programs, not problems. Sometimes they are explicitly formulated, sometimes they are gradually emerging as a prevailing tendency. For example the development of mathematical logic and the foundations of mathematics. That was certainly a development of a program which was understood as such. After Cantor's discoveries it was clear that we have to consider very deeply the ways we think about infinity. Or we have Langlands' program of understanding the Galois group. There is one program with which we enter the next century. This program can be thought of as the quantization of mathematics. When one looks at how many mathematical notions changed in the last twenty years in a way that the new notions are quantum versions of the old ones — it is amazing: Look at quantum groups, quantum cohomology, quantum computing — and I think many more are ahead of us. This is very strange because nobody actually conceived anything like that as a program for developing mathematics in general. The desire was just to understand the mathematical tools that physicists invented with fantastic intuition and which they used in a very stimulating but somewhat careless way from the point of view of a pure mathematician.

How do you think the 20th century will be looked at from an historical point of view? Was it an important century?

I think so. Mathematics of this century succeeded in harmonizing and unifying diverse fields on a scale probably never seen before. The most prominent role in this unification was played by set theory. Initially conceived by Cantor as a new chapter of mathematics, "the theory of infinity", set theory, gradually changed its status and developed into the universal mathematical language. It was understood that starting with a rather short list of basic terms and operations, one could generate recursively the linguistic constructions which apparently conveyed equally well the intuition of the founding fathers of calculus, probability, number theory, topology, differential geometry and what not. Thus the whole mathematical community acquired a common idiom. Moreover, allowing the clear distinction between the set-theoretic and geometric content of the mathematical constructions on the one hand, and their flexible linguistic expression (notation, formulas, calculation) on the other, set theory greatly simplified the interaction between the right and left brains of every working mathematician as an individual. This two-fold function of the set-theoretic language became the basis for the development of new technical tools, for the solution of old problems as well as the formulation of research programs. The diversification of mathematics was connected first of all with external social phenomena: the rapid growth of the scientific community in general and the ground-breaking discoveries in physics. In my opinion, the mathematics of the last hundred years did not produce anything comparable to quantum theory or general relativity in terms of the resulting change of our total world perception. But I do believe that without the mathematical language physicists couldn't even say what they were seeing. This interrelation between physical discoveries and mathematical ways of thinking, the mathematical language, in which these discoveries can only be expressed, is absolutely fantastic. In this sense the 20th century certainly will be regarded as a century of great breakthroughs.

Are there certain specific topics that come to your mind, in which our century was really at a top level?

In the 18th and 19th century mathematical language was much vaguer than we are accustomed to. I think the 20th century started with rethinking the basics. When the basics were clear enough there was a great search of technical methods of incredible strength which led to the creation of powerful tools allowing us to develop and expand our geometric intuition to new domains. I have in mind topology, homological algebra and algebraic geometry. As soon as the technical development was accomplished, the solution of several very difficult problems fell into the span of thirty years — Deligne's proof of the Weil conjectures, Faltings' proof of the Mordell conjecture, Wiles' proof of Fermat. All of them could not have been done in the last century just because mathematics was not developed enough.

Some people — some of them mathematicians — proclaim the end of proof, partly in view of the universal availability of computers. How would you comment on this?

If you are speaking of mathematics without proofs you are speaking of something intrinsically contradictory. The proof cannot die — only together with mathematics. But mathematics can die as an accepted part of the culture of humanity. I think, in our generation, mathematicians still keep doing mathematics as we understand it. Proofs are the only way we know the truth of our thoughts; that is actually the only way of describing what we have seen. Proof is not just an argument convincing an imaginary opponent. Not at all. Proof is the way we communicate mathematical truth. Everything else — leaps of intuition, elation of sudden discovery, ungrounded but strong beliefs, remains our private matter. And when we do some computer calculations we are only proving that in the cases we have checked things are as we have seen them.

Just recently, there was a notice in the newspaper that a computer has proved a conjecture of Herbert Robbins by carrying out a full search of all possible strategies.

Of course this is possible. Why not? If you have invented a good strategy of proof which includes however an extensive search or long formal calculations, and afterwards you have written a program implementing this search, it's perfectly OK. But computer assisted proofs, as well as computer unassisted ones, can be good or bad. A good proof is a proof that makes us wiser. If the heart of the proof is a voluminious search or a long string of identities, it is probably a bad proof. If something is so isolated that it is sufficient to get the result popped up on the screen or a computer, then it is probably not worth doing. Wisdom lives in connections. If I have to calculate the first 20 digits of π by hand I certainly become wiser afterwards because I see that these formulas for π that I knew take too much time to produce 20 digits. I will probably devise some algorithms which minimize my effort. But when I get two millions of digits of π from the computer using somebody else's library program I remain as stupid as I was before.

If you have a beautiful theorem with an equally beautiful proof but which needs the calculation of one thousand cases, do you mind giving it away to the computer? Is this a bona fide proof?

It will be a bona fide proof with the same reservations as I would have for any proof written on paper. There can be possible mistakes in the programming, there can be possible mistakes in implementing the calculation and finally there can be possible mistakes in our understanding of how to classify all the cases and so on. We have examples for those proofs. We have the Four-Color-Problem and the classification of finite simple groups. In both cases a huge amount of combinatorial material was partly treated by computer calculation. So, there is still room for doubts and the need to recheck the calculations, but most important, to devise ways for seeing things in a new light.

Let me ask you a question about mathematics internally. In recent years, the mathematical community seems to emphasize applications. Do you think that pure mathematics will have problems, as compared to applied mathematics? Do you have the impression that the money will go in the future only to those fields?

Applications ask for and get much more money than pure mathematics. But I don't think it's actually the problem of money in terms of allocating limited resources. Mathematicians don't need and don't spend much money. It's a problem of the public attention and the public scale of values. I see the growing estrangement of our society from the traditional Enlightenment values, and the public just does not want to spend on mathematics, probably on universities in general. Mathematics — if it will be a victim — will be a victim of this general process, not of the fact that money goes to applications. But, surely, I do think that there will be a continuous shift to applications in terms of the quantitative resources allocated to applications, and the attractiveness of this kind of occupation for young persons. Applied mathematics is connected with computer simulation — computers at large, database programs and things like that. I have once translated a talk by Donald Knuth into Russian. In Usbekistan there was a meeting dedicated to Al'khorezmi. Knuth started his talk with a funny statement. In his opinion the primary importance of computers for the mathematical community is that those people finally took to mathematics who were interested in mathematics but had an algorithmic sort of mind. Now they were able to do what they wanted. Before that, this subculture didn't exist. I take this argument quite seriously and I do believe that among the community of future potential mathematicians there is a subcommunity whose minds are better for writing computer programs than for proving theorems. In the last century they probably would have proved theorems but nowadays they do not. I have a great suspicion that for example Euler today would spend much more of his time on writing software because he spent so much of his time, e.g., in efforts of calculating tables of moon positions. And I believe that Gauss as well would spend much more time sitting in front of the screen.

Let us go back to the question of applied mathematics. Isn't it true that mathematics is often succesful but that the computer science people receive most of the credit? A standard example is computer tomography. No one I ever talked to had ever heard of the Radon transform, the core of computer tomography. Even educated people think that this is the work of computer scientists.

The point is that there is an inherent weakness in trying to justify one's concerns by saying that they are useful. Useful is a word of engineering. Whatever you understand of quantum mechanics (or chips or whatever), it is only understanding of formulas on a piece of paper. There is nothing useful about it. It becomes useful if it is implemented in things, and if it becomes engineered.

Should the mathematicians go on the offensive? Should they step out into the world and say "here we are"? Are we too reluctant to advertise our achievements?

I strongly argue for being reluctant. I am a rather reclusive person and I hate imposing my views on the public. I think whatever is good will come out anyway, although there is a general problem of selling culture — assuming that we are producing something of cultural value. It is up to the public to pay for it or not to pay for it. Of course, some of us probably must try to prove that they are important, but I think it is difficult. How could Rembrandt have argued against the fact that he was dying in total misery as a poor man? How could he argue? I don't really know what mathematics is all about. But this is so with culture, because in the same way, we don't really know what Rembrandt's pictures are about, why he portrayed persons — as he did — an old man and background. Why is it important? We don't know. That's the problem of culture: You cannot say "why".

What do you think is the cultural role of mathematics?

In my opinion, the basis of all human culture is language, and mathematics is a special kind of linguistic activity. Natural language is an extremely flexible tool of communicating essentials required for survival, of expressing one's emotions and enforcing one's will, of creating virtual worlds of poetry and religion, of seduction and conviction. However, natural language is not very well fit for acquiring, organizing and keeping our growing understanding of nature, which is the most characteristic trait of the modern civilization. Aristotle was arguably the last great mind that stretched this capability of language to its limits. With the advent of Galileo, Kepler and Newton, the natural language in sciences was relegated to the role of a high level mediator between the actual scientific knowledge encoded in astronomical tables, chemical formulas, equations of quantum field theory, databases of human genome on the one hand, and our brains on the other hand. Using the natural language in studying and teaching sciences, we bring with it our values and prejudices, poetical imagery, passion for power and trickster's skills, but nothing really essential for the content of the scientific discourse. Everything that is essential, is carried out either by long lists of more or less well structured data, or by mathematics. For this reason I believe that mathematics is one of the most remarkable achievements of culture, and my life-long preoccupation with mathematics in the capacity of researcher and teacher still leaves me with awe and admiration by the end of every working day. However, I do not believe that I can convincingly defend this conviction in the context of contemporary public debate on science and human values.

Why are you so pessimistic?

I will start explaining my pessimism by reminding that in the current usage "culture" became a profoundly self-referential word. Namely, it is taken for granted that any definition of culture is determined by the pre-existing cultural background, even if the latter is not made explicit. This means that no objective account and evaluation of culture is possible. Furthermore, any statement about culture that becomes authoritative changes the public image of culture and thus changes the culture itself. Most importantly, the modern discourse on culture is largely subordinate to the political discourse. We were less aware of all this when four decades ago, C. P. Snow launched the discussion of the "two cultures". Basically, Snow was worried by the fact that in his milieu the scientific knowledge was not considered as an organic part of the education of a cultured person, as opposed to the Greeks and Shakespeare. Moreover, one could openly and even boastfully acknowledge his or her ignorance of basic laws of physics without damaging his or her image as a cultured person. Snow saw this as a result of the distorted public perception of what constituted the actual content of culture and hoped that public debate and reformed education could help to restore the balance.

Is the thesis of the two cultures still relevant?

The relevance of his observation for us depends on our ability to identify ourselves with respect to his idealized Culture with capital C, embracing Homer and Bach, Galileo and Shakespeare, Tolstoy and Einstein. I am afraid that this ability is largely lost. In fact, the popular idea of multiculturalism creates the image of many equally valid cultures. Grand culture of European origin and/or cultivation is put on a par with other regional cultures and is diminished in stature by such pejorative connotations as cultural imperialism and eurocentrism. Environmentalists blame sciences and technology for the destructive uses we made of them, thus further diminishing their cultural appeal. Ironically, the same arguments that scientists employed in order to justify their occupation, are now turned against them. Deconstructionist and postmodern trends of discourse put in doubt the basic criteria of recognizing the scientific truth going back at least to Galileo and Bacon, and try to replace them by wildly arbitrary intellectual constructions. In this way many of the influential thinkers do not just ignore but aggressively dismiss the scientific counterpart of the contemporary culture. I may (as I do) find this situation deplorable, but I cannot realistically count on an improvement in the foreseeable future.

Coming back to the future of mathematics, do you personally have a theory for which you say: "If I live long enough, this is what I would like to see."?

This I do not know for the following reason: During my scientific career I have changed my subjects several times and not so much because I found something more interesting than something else. Basically I find everything very interesting, but there is no possibility to do everything at the same time. The second best strategy is to try mastering several fields in turn. Two main things I was always interested in were number theory on the one hand and physics on the other. So I think in both domains I always tried to use the intuition developed in both domains. Understanding problems in number theory helped me to understand problems in physics and vice versa. On my private list of values a place of honor is held by the Renaissance term "varietà" — richness of life and world matched with variety of experience and thought, achieved by great minds which we try to emulate.

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