In Conversation with Maxim Kontsevich

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Biographical Sketch. Maxim Kontsevich is a Russian-French mathematician who is known for his work in algebraic geometry, mathematical physics, and number theory. He is a professor at the Institut des Hautes Études Scientifiques in Bures-sur-Yvette, France, and a distinguished professor at the University of Miami. He is a recipient of numerous awards, including the Fields Medal, the Crafoord Prize, and the Breakthrough Prize in Mathematics. Kontsevich was born in Khimki, Russia, in 1964. He studied mathematics at Moscow State University. After a stay as a researcher at the Institute for Information Transmission Problems in Moscow, he moved to the Max Planck Institute for Mathematics in Bonn, Germany, in 1990. In 1995, he joined the faculty of the IHÉS.

Kontsevich's work has had a major impact on a wide range of mathematical fields. He is best known for the solution of the Witten conjecture, for his work on the Kontsevich integral, a topological invariant of knots and links, and for his significant contributions to the theory of quantum field theory, algebraic geometry, and number theory. Kontsevich is a highly influential figure in mathematics. His work has been praised for its originality, depth, and beauty. He is considered to be one of the most important mathematicians of his generation.

In this interview, he talks about some of the many research areas he has worked in during his career, and describes his early education in Russia – which, he tells us, was not at all unusual for Russian mathematicians of his generation. He shares his views on some methodological and philosophical aspects of mathematical practice, including the future of AI in mathematics, and some insights into the creative process. He also describes various aspects of his own past research, and, in particular, shares a certain project of his which he feels deserves to be better known. His concern with correct language and notation comes out clearly: "If you cannot say something clear, you shouldn't speak about it at all," is a quote attributed to Wittgenstein he still remembers from very early on in his career; "it … needs to be translated and formalized because otherwise it will be lost knowledge," he tells us about string theory. He speaks much about dialogue between mathematics and physics. His enthusiasm for his research is evident.

NTC: *We're going to introduce ourselves a little bit. I do mathematical general relativity; specifically, I do nonlinear PDE. And then Sebastian:*

SH: *I work on harmonic maps from Riemann surfaces and relations to integrable systems.*

MK: OK, good.

NTC: *We are interested in hearing about your past research and how you feel about your past research.*

MK: I have done many, many things; I have changed subjects and been in different communities. I can't really say which communities, mostly mathematical physics, but all around in algebra, analysis, geometry, and beautiful number theory.

There have been different periods. One of my first works was in the early 1990s, when I solved the Witten Conjecture about intersections on moduli spaces of curves. But also a little before that I developed an interest in conformal field theory and actually I, simultaneously with Graeme Segal, found axioms of conformal field theory; he came to Moscow in the late '80s and discussed them and we realized that we had the same story. Two years ago we wrote a paper: we finally collaborated and proposed axiomatics of general field theory in higher dimensions. It was, again, a subject we discovered independently maybe ten years before but we are both very slow writers and it took us some time to get it done.

Then in the early 90s I introduced techniques based on Feynman diagrams as a rigorous mathematical tool, which had a lot of consequences for topology. For example, three years ago people proved that the diffeomorphism group of the four-dimensional sphere is not homotopic to the rotation group; in dimension 3 it's true, but in dimension 4 it fails. The proof uses invariants which I introduced during that period.[1] There are several developments. About the same time, motivated by this Feynman technique, together with Albert Schwarz and his students, we introduced what is now called the AKSZ [Alexandrov, Kontsevich, Schwarz and Zaboronsky] model, which is a way to write Lagrangians, especially for topological field theories, and is something people are very interested in now.

Then there was mirror symmetry, various aspects of mirror symmetry. I listened to a talk of Kenji Fukaya in 1992 or 1993 and I got the idea of homological mirror symmetry. Then I gave a talk in the International Congress of Mathematicians in 1994, a plenary talk about homological algebra of mirror symmetry.[2] I think that was pretty brave because there was really very little evidence at that point. And then, motivated by mirror symmetry, I came to the deformation quantization formalism which is also based on the language of graphs, and is in fact a version of the AKSZ model.

Maybe about the same time there was a different development, with a foray into dynamical systems. A friend of mine, Anton Zorich, started to make some experiments with interval exchange maps and observed some numerology; then, starting with some numerological coincidences, we found a kind of formula of Lyapunov exponent which took something like 15 [years] to get a rigorous proof – even in the simplest case. Now it's a big subject in dynamical systems and very, very popular.

Then I worked on the foundations of noncommutative geometry, also motivated by homological mirror symmetry, in order to put the language right. There were these things called the Deligne conjecture, operads; there were many things, some collaboration on determinants, I can go on and on and on.

SH: *May I ask a question about your way of working: You said you started with some numerology – some experiments –*

MK: No, actually it's my collaborator's. But in certain situations I've done some calculations. I used to program a lot in Paris, I was introduced to it by Don Zagier who was kind of my thesis advisor (in fact it was a formality concerning the thesis itself, nevertheless I learned a lot from him and consider him as one of my teachers).

NTC: *So was there any kind of common thread running through all of this?*

MK: No, I think not really – homological mirror symmetry continues to be the main thread through it because from this one can see many new questions. Recently one starts to see some kind of merger between the purely algebraic notion of stability, nonarchimedean geometry and differential geometry, like what

[1] See [3].

[2] See [4].

you [SH] do with gravity theory on membranes, and with abstract questions of homological algebra.

NTC: *Would you say these ideas from homological mirror symmetry relate to the work you did in dynamical systems, for example?*

MK: Not yet. Definitely not yet. But even so, I got experience with these quadratic differentials, and then a few years ago with my collaborators, we discovered it's the right way to do the simplest example of these calculations with Fukaya categories. We actually proposed some things by parallel to dynamics in surfaces, we proposed the idea of using entropy in categorical settings. It's a subject which is getting some popularity especially here in China, there's a couple people –

NTC: *Yeah, we have a couple people here at YMSC that are working on that.*

MK: And also, maybe 2000 I started a collaboration with Yan Soibelman (actually I met him back in Russia, but we went to the West) and we have many joint papers. He is my main collaborator but there are some other groups also.

NTC: *We'd be interested in hearing your perspective on questions related to mathematical creativity. For example, you mentioned that in your work in dynamical systems, it started out with numerology, but then eventually it was able to become a formal mathematical theory. So there has to be some kind of bridge from the numerology that suggests something and then some way of constructing –*

MK: My collaborator Anton Zorich calculated Lyapunov exponents and then in the simplest examples he got 0.333333. OK, I was convinced it should be exactly 1/3. But then it needed a kind of quantum leap to get where it comes from.

NTC: *Do you have any insight into how in mathematics one makes that kind of a quantum leap? For you personally, are there any kinds of processes –*

MK: I don't know. I think if possible one should have some kind of experience. When I start to work on something new I try to see whether it resembles something familiar – maybe from a completely different subject – and how things behave. It's really a game of analogy – when I start to explore a new subject, I kind of try to see what it might resemble. It's like a hall of mirrors, I have to say. I have found it's really kind of important to think a bit about how I think, just separate from myself and try to analyze things.

NTC: *What do you mean exactly, Separate from yourself?*

MK: Just kind of try to analyze what I pay attention to. Recently I have started to be really, really careful about notations and right notations. It makes a lot of sense because one should hide something and reveal something. When you have heavy structures, very cumbersome structures, it's a question of how to deal with them and write notations, where to put indices, where not to put them. I do some calculations, I share my computer calculations with somebody, I'm really very careful just to make it clear, so that the problem is written correctly. It should be transparent so we see that things are the same.

NTC: *Actually this makes me think of something else we wanted to ask about that's a bit related: do you have any insights into the use of AI in mathematics; using computers in mathematics?*

MK: Definitely this will come. People are trying to make proof verification systems and so on, it takes enormous time just to find a common language with the computer. And in fact I've seen several people get interested in computer verification as a kind of hobby, but then it became their main occupation; they kind of moved from actual mathematics to this. It's really interesting; in principle, I'm really glad to give advice to people as a potential future user. But at this moment, from what I've seen – it should be a situation where one has a really precise, narrow language.

SH: *But certainly the point will come when AI plays an important role.*

MK: It's kind of like an assistant.

SH: *So what do you think, if AI outdoes mathematicians –*

MK: It's also possible. Actually I remember a long time ago I had another similar discussion and people asked if AI could get a Fields medal; I said, definitely, yes, it's under 40 years –

[**Laughter.**]

NTC: *The only question is whether it gets anything before it gets more than 40 years old.*

MK: Right. Actually I am pretty sure some proper result will come from AI, but the real question is how to make it kind of understandable.

SH: *So you would still do mathematics for yourself?*

MK: Sure, sure.

NTC: *So you see mathematics as not just being a pursuit for answers but also something that's interesting to do in its own right.*

MK: Yes, yes.

NTC: *OK, thank you. We were interested in going back a little bit – from what I understand of your early mathematical education (secondary and then post-secondary education) you had a very, I would say, unusual education –*

MK: Not really, no, in Russia it wasn't really unusual for Russians, at least for professional mathematicians. It was a system of mathematical classes in the last three years of school, from ages about 15 to 17. It was established quite a long time ago, maybe before the Second World War, but maybe in the '50s or '60s. It was started by Kolmogorov and also Gel'fand. Leading mathematicians gave classes, but then it started to be a self-reproducing system. It would just take maybe 2–3 hours per week in addition to the usual curriculum, and it became kind of a self-sustained system: some people who went through the system liked this kind of activity and continued to give lectures to others 5 years younger than they. It was completely free experimentation, at least in my case; they just said, OK, let's do this from the very beginning for you.

In my case the teacher was a logician, a student of Kolmogorov, his name is Aleksander Shen (he is half-Chinese, half-Russian). I remember he gave us a text; it was a long time ago, I think it was like 6 by 9 centimeters [indicates the size]. He typed on a typewriter and then made many copies. It was just that you had to have truly good eyes to read this because it was a usual A4 page, but shrunk really small.[3] I remember I was in 8th grade (grades went from 1 to 10) and the first photo which I received was this kind of preface with a citation from Wittgenstein: If you cannot say something clear, you shouldn't speak about it at all.

NTC: *Sounds very much like Wittgenstein.*

MK: Yes.

SH: *So I imagine you got a lot out of this special education.*

MK: Ah yes, yes. Also there were Olympiads and also translations, for example books of Martin Gardner and also some Russian authors. There was really a great amount of books to get kids interested. I was good at Olympiads, and then I went to the school. Essentially almost all Russian mathematicians or Soviet mathematicians who were well known in those days, who were of my generation or a bit older or younger, almost all of them went through these mathematical classes.

For example, in Moscow State University there were maybe 400 students in mathematics and mechanics – maybe 200 in mathematics – each year, and from these mathematics classes there were maybe 40 – 30 or 40, not everybody, so you got this kind of huge advantage. For example, I remember the first year I didn't ever go to those lectures on analysis or calculus or whatever, ordinary differential equations. I knew that all in advance. I used my time to go to special seminars; there were maybe 100 seminars per week, great activity.

SH: *So you would say that this is very important for the education of talented people –*

MK: Yes. It gives some kind of preliminary knowledge so that they can start things 2–3 years earlier.

SH: *Besides these classes, what sort of papers, books, lectures, or seminars have been most important for you? I guess the Gel'fand seminar?*

MK: Yes. Also there were a great amount of very good books and translations published at that time. I remember I also spent quite a lot of time in used book-shops, because some books were published 10–15 years previously and you could find them in reprint and really look at them there.

Seminars – it was the seminar by Gel'fand. I was his student but in practice it was a student of his student Sascha [Alexander] Goncharov. He kind of delegated. It was kind of like a small seminar.

There were also seminars with [Alexandre] Kirillov – and actually in Kirillov's seminar he gave some problem, and I solved this problem when I was maybe in my third year in university. I had really zero experience in mathematical writing but Kirillov helped me – we were coauthors, he helped me to finish it. It was a pretty good result, I'm still pretty proud of this result.[4] He first made the very geometric observation that if you take several generic vector fields on a variety of a given dimension, and turn it into a Lie algebra, and count the number of expressions of a given number of commutators – if you look just at the number

[3] The numbers Kontsevich give suggest shrinking by a factor of 2 in each dimension.

[4] See [1, 2].

of expressions you should get a much smaller growth than in free Lie algebras. So unavoidably all such equations should get some relations. If you take two generic vector fields on the line and turn it into a free Lie algebra, it will be bigraded by how many of each vector field appear, and we proposed an explicit formula using partition functions and other things. But still it's a very difficult subject, I don't think many people work on it.

SH: *Have you done more work in that direction afterwards?*

MK: Not at all, not at all. But it's really very nice, we introduced some question in commutative algebra from Lie algebras.

SH: *That brings up another question: you have won many prizes for your great works. Are there some of your works which you like very much but think people might underestimate?*

MK: [Gets excited.] Yes! Some of my preferred works are works which are not known. For example, I have kind of a new collaborator (he's now in Canada), Alexander Odesskii. He's absolutely great in calculations. With him I solved some puzzle I discovered again first experimentally. We have a paper about some p-curvature and differential operators which is a very mysterious result.[5] I think it should be very interesting to people who work in arithmetic geometry. It's not yet translated to usual language and I think nobody noticed it. I tried to communicate it a little here.

It's a very surprising result. You do some calculations with something called p-curvature and other things, and you get a series. That series is very remarkable: first I calculated it on a computer, it took me several days to get maybe 250 coefficients. The coefficients have really enormous growth. You get a series with zero radius of convergence p-adically, which I have never seen at all in real life. All series which appear in practice have positive radii of convergence in the p-adic world. But this series has zero radius of convergence, so it's a new phenomenon to study. I think it is similar to something like resurgence in complex analysis.

Also the formula, you can get some differential equation, you can write it as a linear ODE, you get monodromy around some loop, you get some global monodromy operator and then take the determinant of this matrix. I never saw anything like this: the determinant of the logarithm of a matrix! I didn't expect it a priori. It took really like some quantum leap to get something which we have never seen before.

SH: *So you will continue to work in that direction in the future?*

MK: Yes. We kind of have a formal proof; in a sense the wish is to translate to usual language, and maybe get a little generalization (we can work in coordinates). But the proof – we checked it several times; maybe even some referee checked it, because now it's published. But we still don't understand it. It's maybe like five pages, but what is going on? It's kind of elementary things with matrices, but it took us two years to get it.

[5] See [5].

NTC: *It reminds me of your proof of the Witten Conjecture – it also used some matrix theories, right?*

MK: Yes.

NTC: *Any connection between – not between the results but between any thought processes or any kind of methods between the matrices you used in your most recent result and the types of matrix things you did in proving the Witten Conjecture?*

MK: First of all, the Witten Conjecture was already a matrix model, but what I invented was another matrix model. I still don't understand it, I kind of have to say. It was kind of like the result of formal calculations and naturally led to different matrix models. I didn't really internalize myself what was going on, I have to say. I think some physicists understood it, but it's yet for me to understand, even though it's my own proof.

NTC: *We were wondering if you could give us a nontechnical description of mirror symmetry or homological mirror symmetry.*

MK: Maybe I'll talk about homological mirror symmetry. There is quantum mechanics, with a Hilbert space and an algebra of observables. In some approximation, classical mechanics is a limit of quantum mechanics. You describe wave functions by Lagrangian submanifolds, kind of geometrically; instead of vector spaces you get symplectic manifolds, instead of vectors you get Lagrangian submanifolds. And roughly, an algebra of functions on a Hilbert space will be approximated by a Poisson bracket, some product on a symplectic manifold; so you get algebras associated to real symplectic manifolds.

There is another kind of noncommutative object called a Fukaya category (introduced by Kenji Fukaya in the early '90s) associated to symplectic manifolds. It looks like noncommutative algebraic geometry, and variables suddenly become noncommutative – you cannot commute variables. You get a general kind of categorical viewpoint on noncommutativity and triangulated categories. These things were invented maybe by Verdier – it was based on some algebraic topology and stable homotopy theory; but in Grothendieck's hands it was used essentially to develop all of these analogues of topology in algebraic geometry, like étale topology and so on. But there was a part of the subject that was kind of overlooked as Grothendieck developed it. Mostly you consider vector bundles of coherent sheaves on algebraic varieties; these are also examples of triangulated categories. But they didn't play a role in this development towards the Weil Conjecture.

It was kind of very lucky that when I was in Moscow some friends of mine came to the idea that it's really an important object in itself; also somebody just from my year in university (Gorodentsev) discovered that on $\mathbb{CP}^2$ you have a really huge amount of rigid vector bundles – zero moduli, you cannot deform them; vector bundles with huge Chern numbers, and with very strange ranks. Their Chern classes are solutions of what's called Markov equations, $a^2+b^2+c^2=3abc$ (there are some related great questions). Then they realized there's some game with triangulated categories. In Moscow Bondal and Kapranov started noncommutative algebraic geometry so I was very lucky to have this knowledge about this new viewpoint. And then I realized that mirror symmetry was equivalent to these new things invented by Fukaya; but these were things which they had done – but it explained a lot of mysteries. So I really got to know a lot of really great people with great ideas I have to say.

SH: *So certainly physics had a lot of impact on your work. Do you think your mathematical work had some impact on physics?*

MK: Definitely. For physics I have to say for me it was like imprinting: when I was a young student at Moscow University around 1984, the first conformal field theories were discovered by Belavin, Polyakov, Zamolodchikov, like the Ising model and the conformal limit of the Ising model. The idea of conformal invariance was originally discovered by Wilson, but it was kind of theoretical, there were really no concrete examples.

In dimension 2 there was a huge variety of things and they were already discovered because mathematicians made some calculations; specifically, students of Gel'fand, Fuchs and Feigin, they made calculations for representations of central extensions of groups of diffeomorphisms of circles – it wasn't really related to physics, it was just a calculation and they discovered some interesting numbers. And then physicists – Polyakov, I think, maybe Zamolodchikov – realized it should be critical dimensions for Ising models. So mathematicians were really the first, I have to say, but they didn't know what to do with it, and the physicists made this great discovery. So it was these things that really started this dialogue between physics and mathematics, and physicists very often gave talks in this exciting period.

For me it was like imprinting: I was very young and saw the whole thing born with my own eyes. But then I was also a bit of a lucky case, I had just finished university in 1985 and went to work in the Institute of Information Transmission in a small lab of Dobrushin. He was in probability theory and statistical physics and he invented the notion of a Gibbs random field with some kind of independence. So here were some kind of categorical ideas – axiomatics of locality in field theory – and I got an idea parallel to Sir Michael Atiyah, the idea of topological invariants. Really one of the great things here is the spirit, I just listened to a talk maybe by Beilinson about this early – it didn't even have a name, Wess-Zumino-Witten models wasn't even a name yet – but what was kind of like two-dimensional gauge theory, something now called a conformal block. At the time I don't think we really had a name, it was some structure. And then I had my idea about topological invariants, this idea that it should be invariants of three-dimensional manifolds.

Then the kind of funny thing was Albert Schwarz, he was one of the mathematicians who became a physicist and actually he contributed a lot. He had this idea of supersymmetry, it was basically from his students. And then he was thinking about maybe determinants and tried to go beyond that. He gave a very short talk, like five minutes, in Gel'fand's seminar, on an odd thought about making invariants of differentiable manifolds: you take a Chern-Simons section and take an exponent of the Chern-Simons section; this integral should make sense, he said, but he did not know how to calculate it. But definitely it should be something.

Then there was a little break, and he said there was also the idea of invariants, we should consider 3-manifolds, consider Morse functions, and make this into conformal blocks, and all these things should be invariant. And it was a year before Witten came up with it –

[Laughter.]

MK: – and we didn't realize it: he [Schwarz] never wrote anything and we kind of rediscovered things before. But we didn't know it was the same stuff, but it was really funny.

NTC: *So we were also curious: have you ever had points in your research where you felt you were stuck on something?*

MK: Oh, all the time. [Laughter.] It's kind of permanent, it's normal, I fear to say. It's stuck there, but it's not everywhere, just try to discuss things from a different subject. There are some places which are kind of movable. But I think it's normal.

NTC: *So are you usually working on more than one project? So if you're stuck on one –*

MK: Yes, I have many projects, and I hope not so many dead ends.

NTC: *Do you have any suggestions for researchers who get stuck, who come to a dead end in their research?*

MK: Yes, maybe one can – there's some kind of big questions. The things which maybe one can pose, for example dream about Arakelov geometry, geometry along prime numbers. It's a dangerous subject, people speak about fields of one element for decades, I'm not really optimistic.[6]

SH: *You think people should work towards the big problems?*

MK: Yes, there are these big problems, and one should try to see kind of the big questions.

NTC: *So where do you see your research going in the future? Is that possible to foresee at all?*

MK: Not really. I have a lot of loose ends, some things to finish. But I'm not sure I'll be able to do it for all of them. For example, for big directions there's my collaboration with Yan Soibelman, we've been discussing things for maybe 7 or 8 years, now we're starting to write something about it. It's called holomorphic Floer theory, so it's about mirror symmetry and analysis – it became really a kind of unified picture of how to deal with special functions with some continuation, and how Fukaya categories play a role. In fact what we've done is the following: in mirror symmetry roughly one can use a sort of duality between complex geometry and symplectic geometry, which are two even-dimensional geometries, and of course they exist together. But it's important to have a holomorphic symplectic structure and here we can see the keys of many secrets in mathematics, analysis, and algebra.

[6] While this might sound like a joke, the so-called "field with one element", $\mathbb{F}_1$, is a serious attempt to construct a nontrivial algebraic object which behaves like a field of characteristic one. See, e.g., [6] for an introduction.

NTC: *So that means some sort of unification?*

MK: Yes, it's really a great unification in a kind of algebraic analysis.

NTC: *Do you have any thoughts about where the research fields you have been in are headed or maybe should head in the future?*

MK: [Laughs] Not really, not really, no.

NTC: *Perhaps a slightly better question would be, are there any major open problems that you see?*

MK: Oh, yes, so many problems. I have another kind of line of thought which I have developed with some collaborators like Ludmil Katzarkov and Fabian Haiden. It is about stability. There's this notion, invented by Tom Bridgeland, and actually one could see the whole direction of physics and mathematics this way. First, there's this homological mirror symmetry which I proposed and then there were these categories. And it took physicists some years to figure it out.

So physicists eventually learned it was D-branes. But then Mike Douglas started to see that in these categories you have a kind of $\mathbb{Z}$-grading. What was the origin of the $\mathbb{Z}$-grading? It wasn't really obvious from the physics point of view. It was really tricky; it was a spectral flow and he started to see the origin of the grading coming from this mirror symmetrical perspective and how to relate it to the stability of black holes and things like that. Then he discussed it with Tom Bridgeland and Bridgeland came to wonderful, very abstract axiomatics. And now physicists have adopted these axiomatics. It's really kind of a dialogue.

This stability comes from Mumford stability, kind of the Morse part of a differential equation, to get a nice minimal energy form. I've discussed this with my collaborators for many, many years; the discussions are literally endless. It's really hard to keep a record of what we are discussing, the language changes over time, we have to invent names for things, drawings for what is going on, but it's a very big picture that I think will relate hard analysis and abstract algebra. Fukaya categories will be really huge for this, to develop the sheaf theoretic viewpoint with coefficients, it will be one big structure.

SH: *If you were a young student now, what kind of research direction would you go to?*

MK: There are really a lot of interesting things going on in other fields, like p-adic Hodge theory, things around what Scholze is doing, very very interesting things. Also we have a new language for analysis, which is very ambitious and could be very promising but I'm not an expert.

SH: *But you would definitely do math.*

MK: Math, yes, and kind of one maybe very ambitious goal that people in string theory have created. There are so many interesting things; it's not physics, at least most of it is not physics of the real world, but it's definitely something very interesting, and needs to be translated and formalized because otherwise it will be lost knowledge.

NTC: *We were interested in asking – since you grew up in the Soviet Union, we were interested what your experience was with international collaboration, particularly between East and West, back in the 1980s.*

MK: Actually, from like the mid-'80s there were many, many visitors. I remember Witten came, and Graeme Segal came, and Don Zagier, and Atiyah. Atiyah came to give one seminar, and Gel'fand asked what topics he could talk about, and he listed five or six different things, and asked Gel'fand which he would choose, and Gel'fand said, "All of them!" [Laughter.] It was a great period. It was very nice and then the country became open and we started to travel – it was 1988 – to France, and to Marseilles.

NTC: *We see now international collaborations being disturbed by –*

MK: Oh yes, by modern events.

NTC: *Do you see this kind of issue spreading to the mathematical world?*

MK: It hasn't yet but I'm afraid it could. This new venue for an international conference in [July] I think is a really great opportunity to kind of avoid these tensions, because here it's a kind of neutral country.

SH: *So maybe back to the '80s in Moscow: how important was the international communication between people from the West and the East?*

MK: It started really only at the beginning of Perestroika. Before there was a huge break. There was a logician who was put in psychiatric guard, and then people wrote a letter in support of him, maybe 100 leading mathematicians wrote in support of him. And the authorities were very unhappy, and as a kind of punishment they didn't really let them travel for a really long period, from '69, for about 15–18 years. But of course people communicated by mail and people from the West were coming to Russia so there was no real isolation.

In the case of Manin, it actually played a good role, I have to say, in several ways for him. They just said "He's dangerous for the young generation", and so he was only given a special course of his own choice, on a kind of really totally new research. And this was very nice research, therefore it was actually good.

That reminds me actually about some incident: When I came to Berkeley in '95 I applied for some grant, maybe it was a Sloan grant, I forget what it was, something to have money for students. And the grant was rejected. The people from the foundation wrote and cited some referees who said, Kontsevich is a danger to modern mathematics.

[Laughter.]

SH: *So after Covid, people are going back to conferences –*

MK: Yes, yes.

SH: *How important do you think it is that people really meet in person to discuss?*

MK: Absolutely – what is kind of nice on Zoom is that you can skip and nobody will notice. But it's really not the same thing. I think Zoom is overused and people are forced to go listen to lectures when they're sleepy. You can't freely discuss on Zoom meetings, in person is much better.

NTC: *Let's close with one non-math question. I understand you had an interest in Baroque and Renaissance music –*

MK: Oh, yes.

NTC: *Is this an interest you have kept up?*

MK: No, not really. I used to, you know, play Baroque music with some group of friends back in Moscow but we all moved in different directions so I didn't. I went to a Baroque concert recently. But I'm open to other kinds of music.

SH: *Have you been to the Beijing Opera?*

MK: Not yet.

SH: *Maybe the last question then, how do you like it in Beijing? You aren't here for long but –*

MK: No, it's a really huge city.

NTC: *I feel like I have to apologize that you had to be here for a dust storm. We don't usually have that bad of pollution.*

MK: Ah, just so. I haven't really had time to explore, just a little bit, but not really.

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