Interview with Mikhail Gromov

Martin Raussen and Christian Skau

Mikhail Gromov is the recipient of the 2009 Abel Prize of the Norwegian Academy of Science and Letters. On May 18, 2009, prior to the Abel Prize celebration in Oslo, Gromov was interviewed by Martin Raussen and Christian Skau. This interview originally appeared in the September 2009 issue of the *Newsletter of the European Mathematical Society* and is reprinted here with permission.

A Russian Education

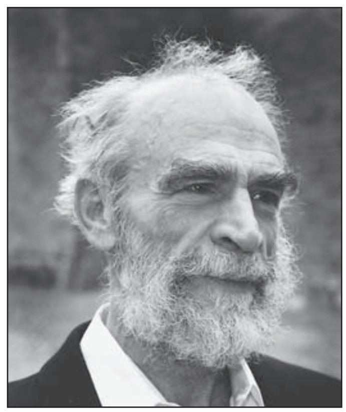
***Raussen and Skau:** First of all, we would like to congratulate you warmly for having been selected as the 2009 Abel Prize winner. We would like to start with some questions about your early years and your early career. You were born towards the end of World War II in a small town called Boksitogorsk, 245 km east of St. Petersburg (at that time Leningrad).*

Gromov: My mother was a medical doctor in the fighting army—and to give birth at that time, she had to move a little away from the frontline.

***Raussen and Skau:** Could you tell us about your background, your early education, and who or what made you interested in mathematics?*

Gromov: My first encounter with mathematics besides school was a book my mother bought me called *Numbers and Figures* by Rademacher and Toeplitz, which had a big influence on me. I could not understand most of what I was reading but I was excited all the same. I still retain that excitement by all the mysteries that you cannot understand but that make you curious.

***Raussen and Skau:** Did you know you would go into mathematics while at high school?*



Mikhail Gromov

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Gromov: In my middle and later years at high school I was more interested in chemistry than in mathematics. But then I was hooked. There were some very good books in Russia on mathematical problems for youngsters. I was going through them and I immersed myself in all this for a year. In my last year of high school I was attending a so-called mathematics circle, something for youngsters at the university, run by two people, Vasia Malozemov and Serezha Maslov (Maslov became a logician; coincidentally, he was the one who suggested Hilbert's tenth problem to Matiasevic). They were running an extremely good group for young children that I attended. This was in St. Petersburg in 1959, the year before I started at university, and it was the major reason for my decision to study mathematics.

***Raussen and Skau:** You started studying mathematics at Leningrad University. Please tell us about the environment there, how you were brought up mathematically and about the teachers who were important for you.*

Gromov: I think it was a pleasant environment despite the political surroundings, which were rather unpleasant. There was an extremely high spirit in the mathematical community and among professors. I remember my first teachers, including Professor Isidor Pavlovich Natanson, and also I attended a class run by Boris Mikhailovich

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Makarov. You could see the high intensity of these people and their devotion to science. That had a very strong impact on me, as well as the interactions with the senior students. Let me mention one, the young algebraist Tolia Yakovlev, who projected this image of absolute dedication to mathematics. On the other hand, there was a general trend in Leningrad of relating mathematics to science. This was influenced, I think, by Kolmogorov and Gelfand from Moscow. Kolmogorov made fundamental contributions to hydrodynamics, and Gelfand was working in biology and also in physics. Basically, there was an idea of the universality of knowledge, with mathematics being kind of the focus of intellectual ideas and developments. And that, of course, shaped everybody who was there, myself included. And I learned very much of the Moscow style of mathematics from Dima Kazhdan, with whom we were meeting from time to time.

Raussen and Skau: *Can you remember when and how you became aware of your exceptional mathematical talent?*

Gromov: I do not think I am exceptional. Accidentally, things happened, and I have qualities that you can appreciate. I guess I never thought in those terms.

Raussen and Skau: *At least towards the end of your studies, your academic teacher was Vladimir Rokhlin. Do you still sense his influence in the way you do mathematics today?*

Gromov: You see, Rokhlin himself was educated in Moscow, and the Moscow mathematical way of thinking was very different from that in Leningrad. They had a different kind of school that was much more oriented towards Western mathematics. Leningrad was more closed and focused on classical problems; Moscow was more open to new developments. And that is what he brought to Leningrad. Another person with the same attitude was Boris Venkov, an algebraic geometer. From him and from Rokhlin, I got a much broader view and perception of mathematics than what I could have got from the traditional school in Leningrad. On the other hand, the traditional school was also very strong; for instance, the geometry school of Aleksandr Danilovich Alexandrov. There were people like Zalgaller and Burago from whom I learned most of my geometry. Burago was my first teacher in geometry.

Raussen and Skau: *You were very successful at Leningrad University at the beginning of the 1970s. Still, you left Leningrad and the Soviet Union shortly after in 1974. What was the background for your desire to leave?*

Gromov: This is very simple. I always say, if someone tells you you should not do something, you try to do exactly that. You know what happened when God prohibited Eve eating the apple. This is human nature. It was said that you cannot leave the country; it is just impossible, it is wrong, it is horrible. It is like in scientific work: if it is impossible, you try to do it anyway.

Raussen and Skau: *It was probably not that easy to get out of the Soviet Union at that time?*

Gromov: For me it was relatively easy. I was very lucky. But in general it was difficult and risky. I had to apply, I waited for several months, and then I got permission.

Russian Mathematics

Raussen and Skau: *Jacques Tits, one of the Abel prize winners last year, praised Russian mathematical education and Russian schools for the strong personalities and the strong ties between motivations, applications, and the mathematical apparatus, as well as the lively seminars and discussions sometimes lasting for many hours. What is your perception: what is special about the Russian mathematical style and school?*

Gromov: Like I said, it was somewhat different in Leningrad compared to Moscow. What Tits was probably referring to was Gelfand's seminars in Moscow. I attended this seminar in Moscow only once, when I was invited to give a talk, so my recollection might not be typical. But when I came, it took about two hours before the seminar could start because Gelfand was discussing various matters with the audience. Another seminar was run by Piatetsky-Shapiro and that was very rigorous. When something was presented on the blackboard and the audience asked questions, then Shapiro would express his attitude, which was very strong and a bit aggressive: on what students should know and should not know, the idea that they should learn this and this and that… Extremely powerful indications of his personality!

Raussen and Skau: *Do you still feel that there is a specific Russian mathematical background that you build your work upon?*

Gromov: Yes, definitely. There was a very strong romantic attitude towards science and mathematics: the idea that the subject is remarkable and that it is worth dedicating your life to. I do not know whether that is also true in other countries because I was not elsewhere at that time of my education. But that is an attitude that I and many other mathematicians coming from Russia have inherited.

Raussen and Skau: *Is there still a big difference between Russian mathematics and, say, Western mathematics in our days? Or is this difference about to disappear, due to the fact that so many Russians are working in the West?*

Gromov: This I cannot tell, given there are so many Russians working in the West. I do not know much about mathematical life in Russia nowadays; certainly, things have changed tremendously. In my time in Russia, this intensity was partly a reaction to the outside world. Academic life was a peaceful garden of beauty where you could leave a rather ugly political world outside. When this all changed, this sharp concentration went down. It might be so. I don't know. This is only a conjecture.

Raussen and Skau: *Do you still have a lot of contact with Russian mathematicians? Do you go there once in a while?*

Gromov: I have been there twice since I left the country. You still feel the intensity of life there but things go down, partially because so many gifted people are leaving. They are drawn to larger centers where they can learn more.

Raussen and Skau: *Can you tell us about other Russian mathematicians that have influenced you, like Linnik?*

Gromov: Yes. Yuri Linnik was a great scientist, professor, and academician in Leningrad. He was running educational seminars in algebraic geometry one year. A remarkable thing was that he always admitted his complete ignorance. He never pretended to know more than he did, rather the contrary. And secondly, there was always a complete equality between him and his students. I remember one time I was supposed to give a talk there but I overslept and arrived one hour late. But he was just laughing at it—not annoyed at all. And that, I think, exhibits some of his spirit in mathematics—the atmosphere of how we were all in the same boat, regardless of who you were.

Raussen and Skau: *How would you compare him with Rokhlin as a person?*

Gromov: Rokhlin was a more closed person as he had gone through a very complicated life. He was a prisoner in the Second World War. He was Jewish but he somehow managed to conceal it. He had an extremely strong personality. After he was liberated, he was sent to a prison in Russia, a labor camp, because it was considered that he hadn't finished his military service. Being a prisoner of war didn't count as military service! After some work he came to Moscow. It was difficult to say what he thought. He was very closed and tried to keep high standards on everything, but he was not so relaxed and open as Linnik was. It was at first unclear what it was, but then you realized that he was shaped by those horrible experiences.

Raussen and Skau: *Was Linnik also Jewish?*

Gromov: I think Linnik was half Jewish, but he did not participate in the war. He had a different kind of life. He was better positioned in his career as a member of the Academy and so on. Rokhlin was always discriminated against by the authorities, for reasons I don't know. I heard some rumors that he was getting into conflict with some officials in Moscow.

For some time he was a secretary for Pontryagin because Pontryagin was blind and, as an academician, needed a secretary. Rokhlin had this position until he had defended his second thesis. Then he was kicked out of Moscow because he was overqualified. A. D. Alexandrov, then the rector at Leningrad University, made a great effort to bring him to Leningrad in 1960. That had a very strong influence on the development of mathematics in Leningrad. The whole school of topology grew out of his ideas. Rokhlin was a very good teacher and organizer.

Raussen and Skau: *Is it true that Pontryagin was anti-Semitic?*

Gromov: I believe he became anti-Semitic after his second marriage. He was blind, and it is unclear how independent his perception of the world was. In his later years he became anti-Semitic, and he also wrote pamphlets that sounded absolutely silly. It is unclear what or who influenced him to get those ideas.

History of Geometry

Raussen and Skau: *You are the first Abel laureate to receive the prize explicitly for your "revolutionary contribution to geometry". From Euclid's time geometry was, so to say, the "face" of mathematics and a paradigm of how to write and to teach mathematics. Since the work of Gauss, Bolyai, and Lobachevsky from the beginning of the nineteenth century, geometry has expanded enormously. Can you give us your thoughts on some of the highlights since then within geometry?*

Gromov: I can only give a partial answer and my personal point of view. It is very difficult to find out about how people thought about the subject in ancient times. Seen from today, geometry as a mathematical subject was triggered from observations you make in the world; Euclid gave a certain shape of how to organize observations and made an axiomatic approach to mathematics and what followed from those. It happened that it worked very badly beyond the point that it was designed for. In particular, there was a problem with the parallel postulate, and people tried to prove it.

There was a mixture: on one hand they believed that the way you see the world was the only way for you to see it, and they tried to justify that axiomatically. But it did not work. Eventually, mathematicians realized that they had to break out of the naïve way of thinking about axioms. The axioms happened to be very useful but only useful in a limited way. Eventually, you had to deny them. This is how they served. From this point on, mathematics started to move in different directions. In particular, Abel was one of the people who turned mathematics from just observing and formalizing what you see to formalizing what you cannot see directly—what you can only see in a very opaque way. Modern mathematics was shaped in the beginning of the nineteenth century. Then it became more and more structural. Mathematics not only deals with what you see with your eye but what you see in the structure of things, at a more fundamental level, I would say. If you formulate the problem in modern language, the mathematicians at the

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Interview in Oslo in May 2009. Left to right, Christian Skau, Martin Raussen, and Mikhail Gromov.

time faced trying to understand the limitations of Euclidean geometry; it is completely obvious. But it took centuries to develop this language. This work was started by Lobachevsky, Bolyai, and Gauss, and in a different domain by Abel and Galois.

The Laureate's Research in Geometry

Raussen and Skau: *It is said that you revolutionized Riemannian geometry in the late 1970s. Could you explain to us what your novel and original idea consisted of, the idea that turned out to be so groundbreaking?*

Gromov: I cannot explain that since I never thought of them as groundbreaking or original. This happens to any mathematician. When you do something new, you don't realize it is something new. You believe everybody knows it, that it is kind of immediate and that other people just have not expressed it. This happens in fact with many mathematical proofs; the ideas are almost never spoken out. Some believe they are obvious and others are not aware of them. People come from different backgrounds and perceive different things…

Raussen and Skau: *A hallmark of your work has been described as the softening of geometry, whereby equations are replaced by inequalities or approximate or asymptotic equations. Examples include the "coarse viewpoint" on Riemannian geometry, which considers all Riemannian structures at once. This is very original. Nobody had thought about that before. Isn't that true?*

Gromov: That is probably true. But again, I am not certain whether somebody else had had this idea before. For me it was clear from the very beginning, and I actually never articulated it for a long time, believing everybody knew it. I believe that some people knew about it but they never had an occasion to say it aloud. In the end, I formulated it because I gave a course in France.

Raussen and Skau: *First of all, you had this new perspective. The basic ideas are perhaps very simple but you were the first to get any deep results in that direction.*

Gromov: Well, there were predecessors. This trend in Riemannian geometry started with the work of Jeff Cheeger. Earlier, up to some point, people were thinking about manifolds in very abstract terms. There were many indices and you could not take the subject into your hand. I think that one of the first works in which Riemannian geometry was turned into something simple was by John Nash. Actually, he had a tremendous influence on me. He was just taking manifolds in his hands and putting them in space, just playing with them. From this I first learned about this very concrete geometry. Simple things, but you had to project it to very high dimensions. And then there was the work by Jeff Cheeger, formally a very different subject but with the same attitude, realizing that things got quite simple when formalized, if that was done properly. So I was just following in the steps of these people.

Raussen and Skau: *This means that you read Nash's work and were impressed by it very early?*

Gromov: Yes, I read it very carefully. And I still believe I am the only person who read his papers from the beginning to the end. By judging what people have written about it afterwards, I do not think they have read it.

Raussen and Skau: *Why not?*

Gromov: At first, I looked at one of Nash's papers and thought it was just nonsense. But Professor Rokhlin said: "No, no. You must read it." I still thought it was nonsense; it could not be true. But then I read it, and it was incredible. It could not be true but it was true. There were three papers; the two more difficult ones, on embeddings, they looked nonsensical. Then you look at the way it is done, and you also think that it looks nonsensical. After understanding the idea you try to do it better; many people tried to do it in a better way. But when you look at how they were doing it, and also what I tried, and then come back to Nash, you have to admit that he had done it in a better way. He had a tremendous analytic power combined with geometric intuition. This was a fantastic discovery for me: how the world may be different from what you think!

Raussen and Skau: *John Nash received the Nobel Prize in economics and he was also the person behind the* Beautiful Mind *movie. Many people think he should have gotten the Fields Medal for his efforts. Do you subscribe to this idea?*

Gromov: Yes. When you think about this guy and his achievements in science, forgetting about medals, the discoveries he made were fantastic. He was a person thinking in a most unusual way. At least, his work in geometry was contrary to what everybody would expect, concerning the results, the techniques, and the ideas he used. He did various matters in an extremely simple way, so that everybody could see it but nobody would believe it could work. He also had a tremendous power of implementing it, with a dramatic analytic power. What he has done in geometry is, from my point of view, incomparably greater than what he has done in economics, by many orders of magnitude. It was an incredible change in attitude of how you think about manifolds. You can take them in your bare hands, and what you do may be much more powerful than what you can do by traditional means.

Raussen and Skau: *So you admit that he had an important influence upon you and your work.*

Gromov: Yes, absolutely. All over, his work and the work of Smale, which was explained to me by Sergei Novikov at a summer school in the early 1960s, have had the most important influence on me.

Raussen and Skau: *You introduced the h-principle, where "h" stands for homotopy, in order to study a class of partial differential equations that arises in differential geometry rather than in physical science; it has proved to be a very powerful tool. Could you explain the h-principle and your ideas behind introducing the concept?*

Gromov: This was exactly motivated by the work of Smale and Nash. And I realized then that they dealt more or less with the same topic—which had not been clear at all. In particular, if you use Nash's techniques you immediately get all the results of immersion theory. You do not have to go deep. The first lemma in Nash proves all immersion theorems in topology! I was thinking about this for several years, trying to understand the mechanism behind it. I realized there was a simple general mechanism, which was rather formal but incorporated the ideas of Nash and Smale by combining them. This applies to a wide class of equations because you interpolate between rather remote topics and then you cover a very large ground.

Raussen and Skau: *You proved a celebrated theorem, precursors of which were theorems by Milnor-Wolf and Tits. It tells us that if a finitely generated group has polynomial growth, then it contains a nilpotent subgroup of finite index. A particularly remarkable aspect of your proof is that you actually use Hilbert's Fifth Problem, which was proved by Gleason, Montgomery, and Zippin. And this is the first time, apparently, that this result has been used in a significant way. Can you explain and expand on this?*

Gromov: I thought previously about applying this theorem in Riemannian geometry, though in a different context, inspired by Margulis' 1967 paper on 3-dimensional Anosov flows and by his 1970 rendition of Mostow's rigidity theorem, where Margulis introduced and exploited quasi-isometries. I wanted to prove something that happened to be wrong. I tried to apply a version of the Shub-Franks construction in topological dynamics. It didn't work either. Also, there was a paper by Hirsch concerning exactly this question about polynomial growth—a special case of this problem—where he tried to apply the classification of topological groups; and again it didn't work. So I believed it couldn't be applied. It was kind of clear to us that it was close but it didn't seem to work. But when I was formalizing the idea of limits of manifolds, I tried to think in those terms and then I saw that it might work. This was kind of a surprise to me.

Raussen and Skau: *It must have been a very nice experience when you realized that this would work out?*

Gromov: Well, it was not really a sudden insight. I realized what was needed was just a slight change in conceptions. Then it is not difficult to do it. The proof is extremely simple in a way. You take an obvious concept of a limit, and then, by the power of analysis, you can go to the limit many times, which creates structures that you have not seen before. You think you have not done anything but, amazingly, you have achieved something. That was a surprise to me.

Raussen and Skau: *You introduced the idea of looking at a group from infinity, which is an apt description of looking at the limit of a sequence of metric spaces associated to the group in the so-called Gromov-Hausdorff metric. You have used this technique with impressive effect. Please give us some comments.*

Gromov: After proving the theorem about polynomial growth using the limit and looking from infinity, there was a paper by Van den Dries and Wilkie giving a much better presentation of this using ultrafilters. Then I took it up again and I realized it applied to a much wider class of situations where the limits do not exist but you still have the ultralimits, and it gives you a very good view on many mathematical objects, including groups. But it is still not tremendously powerful.

In the context of groups, I was influenced by a survey of the small cancellation theory by Paul Schupp in the book *Word Problems* (1973) where he said—and I think this was a very honest and very useful remark—that "people don't understand what small cancellation groups are." And I felt very comfortable because I didn't understand it either. I started thinking about what they could be, and then I came up with this concept of hyperbolicity. This was rather pleasing to me, but there were some technical points I could not handle for some time, such as the rough version of the Cartan-Hadamard theorem, before I could write an article about it.

Raussen and Skau: *When did you introduce the concept of a hyperbolic group?*

Gromov: My first input on the geometry of groups came from Dima Kazhdan, who explained to me in the middle of the 1960s the topological proof of the Kurosh subgroup theorem. Later on I read, in the same 1971 issue of *Inventiones*, the paper by Griffiths on complex hyperbolicity and the paper by Klingenberg on manifolds of hyperbolic type. The latter contained the idea of rough hyperbolicity, albeit the main theorem in this paper was incorrect. And, as I said, I had read the paper by Schupp.

I presented the first definition of hyperbolicity during the 1978 meeting at Stony Brook under the name of Is(2)-groups as they satisfy the linear isoperimetric inequality in dimension two. The article appeared three years later. Also, I recall, I spoke about it at the Arbeitstagung in 1977. I tried for about ten years to prove that every hyperbolic group is realizable by a space of negative curvature, which I couldn't do, and this is still unknown. Then Steve Gersten convinced me to write what I already knew about it, and I wrote that but I was very dissatisfied because I couldn't decide if you needed the theory of such groups. If they were "geometric", the way I said, we would not need hyperbolicity theory, and we would have much better theorems.

Raussen and Skau: *You said that almost all groups are hyperbolic?*

Gromov: Right. That was actually the point. When I realized that we could see hyperbolicity in certain generic constructions better without an appeal to curvature, then I accepted it as a worthwhile notion. In my first article I suggested a rather technical definition and terminology. I believed it was a preliminary concept. But then I realized eventually that it probably was the right concept, regardless of whether the geometrization theorem I was trying to prove was true or not. Also, I was encouraged by talking to Ilia Rips in the early 1980s, who, by that time, had developed hyperbolic group theory in a combinatorial framework, well beyond what I knew at the time, by the ongoing development of Thurston's 3-D theory and by Cannon's solution of Thurston's rationality conjecture.

Raussen and Skau: *We move to a different area, symplectic geometry, that you have also made a revolutionary contribution to. You introduced methods from complex analysis, notably pseudo-holomorphic curves. Could you expand on this and explain how you got the idea for this novel approach? And also on the Gromov-Witten invariant, which is relevant for string theory and which came up in this connection.*

Gromov: Yes, I remember very vividly this amazing discovery I made there. I was reading a book by Pogorelov about rigidity of convex surfaces. He was using the so-called quasi-analytic functions developed by Bers and Vekua. He talked about some differential equations and said that the solutions were quasi-analytic functions. I couldn't understand what the two had in common. I was looking in his books and in articles of these people but I couldn't understand a single word; and I still don't. I was extremely unhappy about this, but then I thought about it in geometric terms. And then you immediately see there is an almost complex structure there, and the solutions are just holomorphic curves for this almost complex structure. It is nothing special because any elliptic system in two variables has this property. It has the same principal symbol as the Cauchy-Riemann equation. The theorem he was using is obvious once you say it this way. You didn't have to use any theory; it is obvious because complex numbers have a forced orientation. That's all you use!

Raussen and Skau: *You say obvious but not many mathematicians were aware of this?*

Gromov: Yes, exactly. They were proving theorems but they never looked at this. If you look at this in certain terms, it becomes obvious because you have experience with algebraic geometry. Once you know algebraic geometry you observe it as the same. We have this big science of complex analysis and algebraic geometry with a well-established theory; you know what these things are, and you see there is no difference. You use only some part of this but in higher generality. Then, I must admit that for some time I was trying to use it to recapture Donaldson theory, but I couldn't do it because there were some technical points that did not work. Actually, it was similar to the obstruction of being Kähler in dimension four. I spoke with Pierre Deligne and asked him whether there was an example of a complex surface that was not Kählerian and that would have certain unpleasant properties. He said, yes, and showed me such examples. I turned then to the symplectic case, and I realized that it worked very well. And once again, things were very simple, once you knew where to go. It was so simple that I had difficulty believing it could work because there was only one precedent, due to Donaldson. It was Donaldson's theory that said that such mathematics can give you that kind of conclusion. It had never happened before Donaldson, and that was very encouraging. Otherwise I probably wouldn't have believed it would work if not for Donaldson's discovery. Besides, I was prepared by Arnold's conjectures, which I learned from Dima Fuks in the late 1960s, by the symplectic rigidity ideas of Yasha Eliashberg developed by him in the 1970s, and by the Conley-Zehnder theorem.

Raussen and Skau: *Could you say something about the proof by Perelman and Hamilton of the Poincaré conjecture? Did they use some of your results?*

Gromov: No. If at all, then just some very simple things. That is a completely different mathematics. There are interactions with the geometry, I know, but they are minor. It is essentially a quite different sort of mathematics, which I understand only superficially, I must admit. But I must say that it is a domain that is basically unexplored compared to what we know about Cauchy-Riemann equations in a generalized sense, or Yang-Mills, Donaldson, or Seiberg-Witten equations. Here, it is one theorem and it is still somewhat isolated. There is no broader knowledge around it, and we have to wait and see what comes. We certainly expect great developments from this yet to come.

Raussen and Skau: *Do you have any interaction with Alain Connes?*

Gromov: Oh yes, certainly. We have interacted quite a bit, though we think in very different ways. He understands one half and I understand the other half, with only a tiny intersection of the two parts; amazingly, the outcome turns out to be valid sometimes. I have had two joint papers with him and Moscovici, proving particular cases of the Novikov conjecture.

Raussen and Skau: *You came up with an example of some expanders on some groups and thus produced a counterexample to the Baum-Connes conjecture.*

Gromov: This counterexample is due to Higson, [Vincent] Lafforgue, and Skandalis, where they used the construction of random groups.

Raussen and Skau: *Is there one particular theorem or result you are the most proud of?*

Gromov: Yes. It is my introduction of pseudoholomorphic curves, unquestionably. Everything else was just understanding what was already known and to make it look like a new kind of discovery.

Raussen and Skau*: You are very modest!*

Mathematical Biology

Raussen and Skau: *We have been told that you have been interested in questions and problems in mathematical biology recently. Can you describe your involvement and how your mathematical and geometric insights can be useful for problems in biology?*

Gromov: I can explain how I got involved in that. Back in Russia, everybody was excited by ideas of René Thom on applying mathematics to biology. My later motivation started from a mathematical angle, from hyperbolic groups. I realized that hyperbolic Markov partitions were vaguely similar to what happens in the process of cell division. So I looked in the literature and spoke to people, and I learned that there were so-called Lindenmayer systems. Many biologists think that they represent a very good way of describing the growth of plants by patterns of substitution and cell division. Then, at the base of that, we had a meeting at the IHES [Institut des Hautes Etudes Scientifiques] in Bures on pattern formation, in particular in biology. I got interested and I wanted to learn more about biology. Soon, I realized that there had been a huge development in molecular biology in the 1980s, after the discoveries of genetic engineering and of PCR (polymerase chain reaction). It was really

Photo by Erlend Aas/Scanpix, for the Abel Prize, the Norwegian Academy of Science and Letters.

Mikhail Gromov on left, being greeted by the king and queen of Norway.

mathematical procedures applied to living cells. Mathematicians could invent PCR. It didn't happen, but mathematicians could have invented PCR. It was one of the major discoveries of the century. It changed molecular biology completely. I started to learn about these mathematical procedures and to realize that it led to fantastic mathematical questions. But it was hard to say exactly what it is; I just cannot formulate it. Of course there are very particular domains like sequencing, and there are specific algorithms used there. But this is not new mathematics; it is old mathematics applied to this domain. I believe there is mathematics out there still unknown to us that is yet to be discovered. It will serve as a general framework, just like differential equations give a framework for classical mechanics. It will be rather abstract and formal, but it should embed our basic knowledge of biology and maybe accumulate results that we still do not know. I still think about this but I do not know the answer.

Raussen and Skau: *Would you please explain the term PCR?*

Gromov: It means polymerase chain reaction, and you can see it as follows. You come to a planet that is populated by rats, and they all look the same. In your lab, you also have rats that are very similar. They look absolutely identical, but they are of a different species. Now, one of the female rats escapes. One year later you want to decide whether it has survived or not. There are billions of those rats, so you cannot check all of them, so what do you do? Here is the idea. You throw in several billion male rats, and if the escaped rat is still there, then you will find a certain population of your rats. Then you wait a little bit, and the number of them will grow into billions. You take a sample and check if it contains your rat. This is how a polymerase chain reaction works, but instead of rats you use DNA. There are billions of different DNAs of various kinds, and if you want to know if a particular one of them is out there, then there is a way to do that with a given molecule that amplifies exponentially. If one had been out there, you would have billions of them after several cycles. This incredible idea is very simple and powerful. One fundamental thing happening in biology is amplification; it is specific for biology. Mathematics should be useful for biologists. We cannot make it yet, but I believe it can be done. It will have impact on problems in genetic engineering and identifying gene functions, but it has not been developed yet. It will be very different from other kinds of mathematics.

Mediation Between Mathematics and Science

Raussen and Skau: *Is it your impression that biologists recognize and appreciate your work and the work of other mathematicians?*

Gromov: I have not done anything. I just communicated with biologists. But I think many of them were quite satisfied talking to me, as well as to other mathematicians. Not because we know something but because we ask many questions. Sometimes they cannot answer but that makes them think. That is about it, but this is not so little in my opinion. In this way, mathematicians can be useful by being very good listeners.

It happens very rarely that something is done by mathematicians in science. One of the most remarkable examples happened here in Norway in the middle of the nineteenth century. In collaboration, the mathematician Guldberg and the chemist Waage invented chemical kinetics. I do not know of any other situation since then where mathematicians have contributed to experimental science at this level. This shows that it is possible, but it happened through a very close collaboration and in a special situation. I think something like that may happen in biology sometime but it cannot come so easily.

Raussen and Skau: *You came across Guldberg and Waage in connection with your interest in chemistry?*

Gromov: Yes. This is kind of the fundamental equation in chemistry and also in molecular biology, always in the background of things. Mathematicians can have their word, but it is not so easy. You cannot program it. You have to be involved. Sometimes, very rarely, something unexpected happens, with a very strong impact!

Raussen and Skau: *To our amazement, we realized that one of the Abel lectures in connection with the prize, the science lecture, was given on computer graphics. It is said that computer graphics or computer vision, and shape analysis in particular, benefits from your invention: the Gromov-Hausdorff distance. Can you explain where this notion comes in and how it is used?*

Gromov: When you have to compare images, the question is how you compare them. Amazingly—for a geometer it looks unbelievable—the early work on computer vision was based on matching images with another, taking differences in intensity—which is certainly completely contrary to what your eyes do! Actually, the idea of how eyes operate with images goes back to Poincaré. In his famous book called *Science and Hypothesis* he thinks, in particular, about how the human mind can construct Euclidean geometry from the experience we have. He gives an almost mathematical proof that it would be impossible if your eyes could not move. So, what you actually reconstruct, the way your brain records visual information, is based on the movement of your eyes and not so much on what you see. Roughly, the eye does this. It does not add images. It moves images. And it has to move them in the right category, which is roughly the category that appears in Riemannian geometry, with Hausdorff convergence or whatever, using small distortions and matchings of that.

For a mathematician who has read Poincaré, this is obvious. But for the people in computer science, following different traditions from linear analysis, it was not obvious at all. And then, apparently, they brought these ideas from geometry to their domain… Actually, several times I attended lectures by Sapiro since I became interested in vision. He is someone who has thought for a long time about how you analyze images.

Raussen and Skau: *It seems that there is not enough mediation between science and mathematics.*

Gromov: Absolutely, I completely agree. To say "not enough" is an understatement. It is close to zero. The communities have become very segregated due to technical reasons and far too little communication. A happy exception is the Courant Institute. We still have many people interacting, and it happens that mathematicians fall in love with science. To see these young people at Courant is extremely encouraging because you don't see this kind of applied mathematicians anywhere else. But they are well aware of the body of pure mathematics where they can borrow ideas and then apply them. Typically, applied mathematicians are separated from the pure ones. They, kind of, don't quite like each other. That's absurd. This has to be changed because we have the same goals. We just understand the world from different sides.

Raussen and Skau: *Do you have any ideas of how to improve this situation?*

Gromov: No. But I think in any subject where you have this kind of problem, the only suggestion is that you have to start by studying the problem. I don't know enough about this; I just have isolated examples. We have to look at where it works, where it doesn't work and just try to organize things in a new way. But it has to be done very gently because you cannot force mathematicians to do what they don't like. The obvious way to do it is to design good combined educations in mathematics and science. Actually, there is a very good initiative by François Taddei in Paris who organizes classes with lectures on biology for nonbiologists—for young people in mathematics and physics. He is extremely influential and full of enthusiasm. I attended some of those classes, and it was fantastic. He was teaching biology at Ecole Normale for mathematicians and physicists, and he manages to make those ideas accessible for everybody. That is what I think should be done at the first stage. We have to have this special kind of education that is not in any curriculum; you cannot formalize it. Only people who have enough enthusiasm and knowledge can project this knowledge to young people. An institutionalized system is much harder to design, and it is very dangerous to make it in any way canonical, because it may just misfire. Forcing mathematics on nonmathematicians only makes them unhappy.

Raussen and Skau: *We have already talked about your affiliation with the Courant Institute in New York, but for a much longer time you have been affiliated with the Institut des Hautes Etudes Scientifiques (IHES) at Bures-sur-Yvette, close to Paris. Can you explain the role of this institution for your research—and for your daily life, as well?*

Gromov: It is a remarkable place. I knew about it before I came there; it was a legendary place because of Grothendieck. He was kind of a god in mathematics. I had met Dennis Sullivan already at Stony Brook but then met him again at IHES, where I learned a lot of mathematics talking to him. I think he was instrumental bringing me there because he liked what I was doing and we interacted a lot. Dennis interacted with many people. He had a fantastic ability of getting involved in any idea—absorbing and helping to develop an idea. Another great man there was René Thom but he was already into philosophy apart from doing mathematics. Pierre Deligne was also there. From Pierre I learned some stuff rather punctually; on several occasions, I got fantastic answers when I asked him questions. He would take an idea from your mind and turn it in another direction.

Basically, the whole atmosphere created at this institution was very particular. You are almost completely free of anything except for doing research and talking to people—a remarkable place. I think my best memories go back to when I was there as a first-year visitor. Then I was really free. When I became a part of it there were some obligations. Not much, but still. It is ideal for visitors to come for half a year and just relax, but being there permanently was also not so bad.

Raussen and Skau: *Did you get your best results when you were at Bures?*

Gromov: Yes. When I was between 35 and 39, I would say. That's when I was the most productive.

Computers for Mathematicians and for Mathematics

Raussen and Skau: *It is clear that the use of computers has changed the everyday life of mathematicians a lot. Everybody uses computers to communicate and editing is done with computer tools by almost everybody. But other people use computers also as essential research equipment. What are your own experiences? Do you use computers?*

Gromov: No, unfortunately not. I am not adept with computers. I can only write my articles on a computer, and even that I learned rather recently. I do believe that some mathematics, particularly related to biology, will be inseparable from computers. It will be different mathematics when you, indeed, have to combine your thinking with computer experiments. We have to learn how to manipulate large amounts of data without truly understanding everything about it, only having general guidelines. This is, of course, what is happening but it is not happening fast enough. In biology, time is the major factor because we want to discover cures or at least learn about human diseases. And the faster we do it, the better it is. Mathematicians are usually timeless. You are never in a hurry. But here you are in a hurry and mathematicians can accelerate the process. And there, computers are absolutely a part of that. In this way, I believe computers are playing and will play a crucial role.

Raussen and Skau: *And that will change the way mathematics is done in the long run, say within the next fifty years?*

Gromov: I think that within fifty years there will be a radical change in computers. Programming develops very fast, and I also believe mathematicians may contribute to the development in a tremendous way. If this happens, we will have very different computers in fifty years. Actually, nobody has been able to predict the development of computers. Just look at how Isaac Asimov imagined robots and computers thirty years ago when he was projecting into the twenty-first century how they looked like in the 1970s. We probably cannot imagine what will happen within fifty years. The only thing one can say is that they will be very different from now; technology moves at a very fast speed.

Raussen and Skau: *What do you think about quantum computing?*

Gromov: Well, I am not an expert to say anything about that. You have to ask physicists, but they have very different opinions about it. My impression is that the experimental physicists believe we can do it and theorists say: "No, no, we cannot do it." That is the overall impression I have, but I cannot say for myself because I don't understand either of the aspects of it.

Mathematical Work Style

Raussen and Skau: *You have been described as a mathematician who introduces a profoundly original viewpoint to any subject you work on. Do you have an underlying philosophy of how one should do mathematics and, specifically, how one should go about attacking problems?*

Gromov: The only thing I can say is that you have to work hard and that's what we do. You work and work, and think and think. There is no other recipe for that. The only general thing I can say is that when you have a problem then—as mathematicians in the past have known—one has to keep the balance between how much you think yourself and how much you learn from others. Everybody has to find the right balance according to his or her abilities. That is different for different people so you cannot give any general advice.

Raussen and Skau: *Are you a problem solver more than a theory builder? Would you describe yourself in any of those terms?*

Gromov: It depends upon the mood you are in. Sometimes you only want to solve one problem. Of course, with age, you become more and more theoretical. Partly because you get wiser but you can also say it is because you get weaker. I suppose it depends on how you look at it.

Raussen and Skau: *Concerning your mathematical work style, do you think about mathematics all the time?*

Gromov: Yes, except when I have some problems of a personal nature; if there is something else that disturbs me then I cannot think. But if everything is okay and, at least, if there is nothing else to do at the moment, I immerse myself in mathematics, or other subjects, like biology, but in a mathematical way, so to say.

Raussen and Skau: *How many hours per day do you work with mathematics?*

Gromov: Not as much as I used to. When I was young I could go on all day, sometimes from nine in the morning to eleven at night. Nothing could distract me. Of course, now I cannot do that any longer. I can only do five, six hours a day without getting tired.

Raussen and Skau: *When you were younger, you had more energy, but now you are a lot wiser, right?*

Gromov: You can say you become more experienced and wiser when you get older. But you also lose your mental powers and you become weaker. You certainly just have to accept that. Whether you become wiser is questionable. But it is obvious that you become weaker.

Raussen and Skau: *John von Neumann once said that you do the most important things in mathematics before you are thirty. When he himself turned thirty he added that you get wiser as you get older. Do you think that the best mathematics is done before you are thirty?*

Gromov: I can say about myself that I think my best work was done when I was between thirty and forty years old. When I started, I didn't have any perspective and was just doing whatever was coming first. As I was learning more, I kept changing my attitude all the time. Now, if I had to start anew, I would do something completely different, wrongly or rightly, I cannot judge. On the other hand, I must say that everything I think about now, I had already thought of forty years ago. Ideas were germinating in me for a long time. Well, some people probably create radically new work late in life, but basically you develop certain feelings very early. Like your abilities to talk, right? You learn to talk when you are three years old but it doesn't mean you say the same things when you are thirty as when you are three. That's how it works.

Raussen and Skau: *We are surprised that you are so modest by playing down your own achievements. Maybe your ideas are naíve, as you yourself say; but to get results from these ideas, that requires some ingenuity, doesn't it?*

Gromov: It is not that I am terribly modest. I don't think I am a complete idiot. Typically when you do mathematics you don't think about yourself. A friend of mine was complaining that anytime he had a good idea he became so excited about how smart he was that he could not work afterwards. So naturally, I try not to think about it.

Raussen and Skau: *Having worked so hard as you say, have you ever suffered from depression because you have overexerted yourself?*

Gromov: No. Sometimes some outside unhappy things have distracted my work. Of course, sometimes you get very tired and you are glad that someone interrupts your work but other times you cannot stop. You work and work, like an alcoholic, so then it is good to get some rest.

Abel and the Abel Prize

Raussen and Skau: *You once complained that the mathematical community only has digested a minor part of your work, rather the technical details than the underlying big ideas and vistas. Do you think that being awarded the Abel Prize may change that situation?*

Gromov: First about this complaint: it was kind of a half-joke. There were some pieces of work where there happened to be ideas that could not be developed, unlike more successful ones, and I was unhappy about that. It depends on how you look at it; either the ideas were no good or people were not paying attention. You just never know. I wished something I was saying could be developed further but this was not happening. And that was my complaint, or rather the motivation for my complaint. It has nothing to do with the Abel Prize.

Raussen and Skau: *What do you think about prizes in general and, in particular, about the Abel Prize?*

Gromov: Objectively, I don't think we need these prizes for mathematicians who have already achieved much. We need more to encourage young people at all levels, and we must put more effort into that. On the other hand, it is very pleasant to receive this prize. I enjoy it, and it may have some overall positive effect on the perception of the mathematical community in the eyes of the general public. That may be just self-justification because I like it, of course, for appreciation of my work by my friends and by receiving this prize. But as the general scientific concern, the far more serious issue is projecting a much greater effort in getting funds for educating and motivating young people to embrace mathematics. What I have seen here in Oslo, at the high school I visited earlier today—with these young people—I was tremendously impressed. I want to see this kind of event everywhere in the world. Of course, mathematicians are not so ascetic that they don't like prizes, but in the long run it is not prizes that shape our future.

Raussen and Skau: *Coming back to Abel, do you admire him as a mathematician?*

Gromov: Yes, absolutely. As I said, he was one of the major figures, if not the major figure, in changing the course of mathematics from what could be visualized and immediately experienced to the next level, a level of deeper and more fundamental structures.

Raussen and Skau: *There is a posthumous paper by Abel where he writes about the theory of equations, which later became Galois theory, and in the introduction he says something very interesting. He says something like: "A problem that seems insurmountable is just seemingly so because we have not asked the right question. You should always ask the right question and then you can solve the problem".*

Gromov: Absolutely. He changed the perspective on how we ask questions. I do not know enough about the history of mathematics but it is obvious that the work of Abel and his way of thinking about spaces and functions has changed mathematics. I do not know enough history to say exactly when this happened, but the concept of underlying symmetries of structures comes very much from his work. We still follow that development. It is not exhausted yet. This continued with Galois theory and in the development of Lie group theory, due to Lie, and, in modern times, it was done at a higher level, in particular by Grothendieck. This will continue, and we have to go through all that to see where it brings us before we go on to the next stage. It is the basis of all we do now in mathematics.

Future of Mathematics

Raussen and Skau: *After this excursion into the history of mathematics, may we speculate a little about the future of mathematics? You once compared the whole building of mathematics with a tree, Hilbert's tree, with a metric structure expressing closeness or nearness between different areas and results. We know from Kurt Gödel that there are parts of that tree we will never reach. On the other hand, we have a grasp of a certain part of the tree, but we don't know how big this part is. Do you think we know a reasonable part of Hilbert's tree? Is the human mind built for grasping bigger parts of it or will there stay areas left uncharted forever?*

Gromov: Actually, I am thinking about that now. I don't know the answer, but I have a program of how we can approach it. It is a rather long discussion. There are certain basic operations by which we can perceive the structure. We can list some of them, and apparently they bring you to certain parts of this tree. They are not axioms. They are quite different from axioms. But eventually you cannot study the outcome with your hands and you have to use computers. With computers you come to some conclusions without knowing the intermediate steps. The computational size will be too huge for you. You have to formalize this approach to arrive at certain schemes of computations. This is what I think about now but I don't know the answer. There are indirect indications that it is possible but those are of a nonmathematical nature, rather biological.

Raussen and Skau: *If you try to look into the future, fifty or one hundred years from now…*

Gromov: Fifty and one hundred is very different. We know more or less about the next fifty years. We shall continue in the way we go. But in fifty years from now, the Earth will run out of the basic resources, and we cannot predict what will happen after that. We will run out of water, air, soil, rare metals, not to mention oil. Everything will essentially come to an end within fifty years. What will happen after that? I am scared. It may be okay if we find solutions, but if we don't then everything may come to an end very quickly!

Mathematics may help to solve the problem, but if we are not successful, there will not be any mathematics left, I am afraid!

Raussen and Skau: *Are you pessimistic?*

Gromov: I don't know. It depends on what we do. If we continue to move blindly into the future, there will be a disaster within one hundred years, and it will start to be very critical in fifty years already. Well, fifty is just an estimate. It may be forty or it may be seventy, but the problem will definitely come. If we are ready for the problems and manage to solve them, it will be fantastic. I think there is potential to solve them, but this potential should be used, and this potential is education. It will not be solved by God. People must have ideas and they must prepare now. In two generations people must be educated. Teachers must be educated now, and then the teachers will educate a new generation. Then there will be sufficiently many people who will be able to face the difficulties. I am sure this will give a result. If not, it will be a disaster. It is an exponential process. If we run along an exponential process, it will explode. That is a very simple computation. For example, there will be no soil. The soil is being exhausted everywhere in the world. It is not being said often enough. Not to mention water. It is not an insurmountable problem, but it requires solutions on a scale we have never faced before, both socially and intellectually.

Education Systems for the Future

Raussen and Skau: *Education is apparently a key factor. You have earlier expressed your distress about realizing that the minds of gifted youths are not developed effectively enough. Any ideas about how education should change to get better adapted to very different minds?*

Gromov: Again I think you have to study it. There are no absolutes. Look at the number of people like Abel who were born two hundred years ago. Now there are no more Abels. On the other hand, the number of educated people has grown tremendously. It means that they have not been educated properly because where are those people like Abel? It means that they have been destroyed. The education destroys these potential geniuses—we do not have them! This means that education does not serve this particular function. The crucial point is that you have to treat everybody in a different way. That is not happening today. We don't have more great people now than we had one hundred, two hundred, or five hundred years ago, starting from the Renaissance, in spite of a much larger population. This is probably due to education. This is maybe not the most serious problem with education. Many people believe in very strange things and accordingly make very strange decisions. As you know, in the UK, in some of the universities, there are faculties of homeopathy that are supported by the government. They are tremendously successful in terms of numbers of students. And anybody can learn that nonsense. It is very unfortunate.

Raussen and Skau: *You point out that we don't have anybody of Abel's stature today, or at least very few of them. Is that because we, in our educational system, are not clever enough to take care of those who are exceptionally gifted because they may have strange ideas, remote from mainstream?*

Gromov: The question of education is not obvious. There are some experiments on animals that indicate that the way you teach an animal is not the way you think it happens. The learning mechanism of the brain is very different from how we think it works: like in physics, there are hidden mechanisms. We superimpose our view from everyday experience, which may be completely distorted. Because of that, we can distort the potentially exceptional abilities of some children.

There are two opposite goals education is supposed to achieve: firstly, to teach people to conform to the society they live in; on the other hand, to give them freedom to develop in the best possible way. These are opposite purposes, and they are always in collision with each other. This creates the result that some people get suppressed in the process of adapting them to society. You cannot avoid this kind of collision of goals, but we have to find a balance between the two, and that is not easy, on all levels of education.

There are very interesting experiments performed with chimpanzee and bonobo apes and under which conditions they learn, or even how you teach a parrot to talk. How do you do that? The major factor is that it should not see the teacher. You put a mirror between you and the parrot and then you speak behind the mirror. The parrot then sees a bird—it talks to a bird. But if it sees you, it will learn very badly.

That is not an obvious thing. The very presence of a teacher, an authority, moves students in a particular direction and not at all the direction the teacher wants them to move. With all this accumulated evidence, you cannot make any simple decision. If you say "do this and this," you are wrong for sure. Solutions are not obvious; they can only come after analyzing deeply what is actually known and by studying the possibilities. I think the answers will be unexpected. What children can learn and what they cannot learn, we don't know because we don't know how to conduct experiments to be ethical and instructive at the same time. It is a very nontrivial issue, which has not been studied much. With animals we have results but not very much with people.

Raussen and Skau: *Let us come back to mathematics and to mathematics education. It seems that many people stop dealing with mathematics as soon as they have left high school. But as mathematicians we know that mathematics is everywhere, though often hidden: as the workhorse in science and technology, but also as a pillar in human culture, emphasizing rigor and organized thinking. Do you have any ideas on how we can make this double role perceived and appreciated by society and how to make decision makers realize that mathematics needs support?*

Gromov: It is a very difficult question because we have to project mathematical ideas to people who work very far from mathematics—to people who make decisions in society. The way we think is very different from the way they operate.

I don't know but I think that within our mathematical society we can make some steps towards education, like creating good mathematical sources for children. Today we have the Internet so we should try to make Internet presentations. Actually, in France there are some people trying to organize extracurricular activities for younger children on a small scale. We should try to do something like that on a big scale: big centers of stimulating creativity in all directions. I would not only focus on mathematics but on science and art and whatever can promote creative activity in young people. When this develops, we may have some influence but not before that. Being inside our ivory tower, what can we say? We are inside this ivory tower, and we are very comfortable there. But we cannot really say much because we don't see the world well enough either. We have to go out, but that is not so easy.

Raussen and Skau: *You mentioned that you first got interested in mathematics after reading the book* Numbers and Figures *by Rademacher and Toeplitz. We could also mention the book* What Is Mathematics? *by Courant and Robbins. Should we encourage pupils in high school who show an interest in mathematics to read books like that?*

Gromov: Yes. We have to produce more such books. Already there are some well-written books, by Martin Gardner, by Yakov Perelman (*Mathematics Can Be Fun*), by Yaglom and co-authors—very remarkable books. Other mathematicians can contribute by writing such books and combine this with the possibilities of the Internet, in particular visualization.

It is relatively simple to write just one page of interesting mathematics. This should be done so that many different subjects in mathematics become easily available. As a community we should go out and create such structures on the Internet. That is relatively easy. The next level is more complicated; writing a book is not easy. Within the community we should try to encourage people to do that. It is a very honorable kind of activity. All too often mathematicians say: "Just vulgarization, not serious". But that is not true; it is very difficult to write books with a wide appeal, and very few mathematicians are actually able to do that. You have to know things very well and understand them very deeply to present them in the most evident way.

Raussen and Skau: *This could be a way to get more young people to take up mathematics?*

Gromov: You will attract more young people. Moreover, the political figures will sense it on a much larger scale because it will have a much wider appeal than what we do internally.

Poetry

Raussen and Skau: *You have mentioned that you like poetry. What kind of poetry do you like?*

Gromov: Of course, most of what I know is Russian poetry—the so-called Silver Age of Russian Poetry at the turn of the twentieth century. There were some poets but you, probably, do not know them. They are untranslatable, I guess. People in the West know Akhmatova, but she was not the greatest poet. The three great poets were Tsvetaeva (also a woman), Blok, and Mandelstam.

Raussen and Skau: *What about Pushkin?*

Gromov: You see, with Pushkin, the problem is as follows. He was taught at school, and that has a tremendously negative impact. But forty years later I rediscovered Pushkin and found him fantastic—when I had forgotten what I had learned in school.

Raussen and Skau: *What about modern poetry and English poetry?*

Gromov: I have read some English poetry. I know some pieces but I don't know it on a larger scale. It is difficult. Even with modern Russian poetry, e.g., Brodsky, I find it difficult to absorb a new style. To absorb a poet is nontrivial. For English poetry, there are a few particular pieces that I learned and appreciate. Some of them are easy to deal with; some have Russian translations. A remarkable one is Edgar Allan Poe. He is kind of simple in a way. But many other English poets are more remote from Russian style. I know a little bit of French poetry, like François Villon; I can appreciate him in French. But modern poetry is very difficult for me.

Raussen and Skau: *To finish the interview, we would like to thank you very much on behalf of the Norwegian, the Danish, and the European Mathematical Societies.*