Anatole Katok

What follows is not a literal translation of excerpts from several oral interviews in Russian. All the essential elements of the original are preserved but certain less important sentences are omitted and others are simplified or summarized. An attempt was made to reduce such omissions and simplifications to a minimum. Selected excerpts contain primarily assessment of the work and personalities of several important mathematicians and description or certain trends and tendencies in Moscow mathematical community.

New Year 1981: December 32, 1980 and January 1, 1981, Ithaca, NY
(43:30-46:45)[1]

E. D. Sinai was your mentor, and you worked with him a lot. So tell me about him. How did you meet him?

A. K. The beginning of my academic relationship with Sinai was quite prosaic. I started my third year under the supervision of Minlos. However, by the end of that year my enthusiasm started to evaporate. No doubt Minlos was an inspiring teacher, especially at the earlier stages of my work with him, but the problems that he assigned in his seminar (he ran a seminar for students who worked with him) required an extraordinary amount technical skill. Minlos himself was a master at that. But these problems seemed boring to us. They didn't engage our creativity.

By and large, I think that Minlos did not have much success with his students. Those who continued to work with him (take my classmate Ilya Novikov, for example) didn't create a lot of buzz in mathematics.

Minlos himself, in my opinion, was at his best when he collaborated with others who were capable to show him the way. His work on phase transitions is a case in point. Although Minlos started to work on statistical physics earlier than Sinai, the real progress began only when Sinai entered the field. Sinai was able to find a general direction and to make the best of Minlos' superb technical skills by applying them in the right direction.

E. D. Didn't they also collaborate with Dobrushin?

[1] Numbers in parenthesis describe a time interval of the interview.

A. K. Dobrushin worked in parallel with them, but the first pioneering work on phase transitions in the Ising model was done by Minlos and Sinai.

As for me, I never developed a penchant for complex technical calculations. This is partly due to the Mekhmat policy of voluntary attendance, which allowed me to avoid doing things I didn't like, as well as the fact that we were allowed to take exams ahead of schedule.

Anyhow, my academic relationship with Minlos didn't work out, and I started attending Sinai's special course on ergodic theory. At the time Sinai was brimming with enthusiasm. It was 1963, the year when he published a series of works that brought him wide recognition.

E. D. This was the time when he worked on K-systems and related topics.

A. K. Yes, exactly. Although the course he taught was more general, it was geared toward this area.

(48:08-1:02:30)

A. K. As I was saying earlier, at first I participated in Sinai's special course. Then we started meeting and discussing various mathematical problems. I made a certain amount of progress with them, though perhaps it wasn't sufficient. Eventually it became clear to me that I should continue working with Sinai. I worked with him for the last two and a half years at the university. He was also my supervisor when I was in graduate school, and I kept in touch with him for many years.

On both academic and personal levels our relationship wasn't simple, almost from the very beginning. Don't get me wrong, I consider Sinai an outstanding mathematician, which I believe is a standard view. I also think that he considers me as one of his best students. We have mutual respect for each other. However, my assessment of him as a mathematician is not all positive. [It is a matter of an approach to mathematics.]

Take for example books that were written before the Bourbaki[2] revolution, and I don't mean books on algebra but books on analysis, differential equations or celestial mechanics. These books do not have the kind of clear, deductive structure of the text that we are so

[2] http://en.wikipedia.org/wiki/Nicolas_Bourbaki

used to see today. As far as I understand, this kind of structuralist approach to analysis emerged fairly recently.

E. D. Well, it depends. What about Vallée Poussin, a calculus textbook commonly used in my day?

A. K. Yes, but it was very basic. And even so what you find there is a chunk of text followed by a statement of results, quite unlike how it is done these days: first, a theorem; then, a lemma.

E. D. It depends what book you are talking about.

A. K. This difference may seem superficial, but in fact it is quite profound. It is a matter of how one understands mathematics.

E. D. Does this apply only to books?

A. K. It applies to books but even more so to papers. I think that only after the Bourbaki revolution this kind of structuralist approach to mathematics was widely accepted.

E. D. I don't really consider it a revolution. For example, in probability theory the first rigorous axiomatic was written by Kolmogorov in the early 30s, and Kolmogorov always wrote his works in a very informal style.

A. K. You are right. After all, Sinai is Kolmogorov's student. I think that Sinai is a mathematician who approaches mathematics as a physicist.

E. D. That is, you think he is as an anti-Bourbakist??

A. K. What do you mean anti-? He certainly has some bias against excessive polishing and formalisation. It's not so much a matter of formalisation. I would rather call it structuralism. I am a staunch opponent of rigid formalism myself. Sinai views mathematics as a natural science. Although he certainly knows what a mathematical proof is and he mastered the "epsilon-delta" technique, he views mathematics through the prism of physics. I don't think it is a pure coincidence that he is so immersed in physics these days. It seems to me that he has never been particularly interested in the aesthetic side of mathematics. He has no liking for obtaining elegant and internally beautiful results. This is the first problem. The second problem is accuracy. In the West there is a concept of Russian proof. I would like to know what you think about it.

E. D. What kind of proof?

A. K. Russian proof.

E. D. I've never heard about it.

A. K. Well, I have.

E. D. It probably exists only in your area of specialty. In my area Russian proofs are not different from any other proofs.

A. K. Certainly not every work written by Russian mathematicians contains Russian proofs. In part this term has to do with the common practice of Russian mathematicians of publishing announcements in the *Doklady* ("reports") without subsequent publication of detailed proofs. Unfortunately, this is often the case with young mathematicians, who are not necessarily lazy but simply do not have an opportunity to publish the full text. It also has to do with a large number of errors and inadequacies which are ubiquitous in Soviet scholarship, including works of such famous people as Gelfand.

Here is a story about Sinai's "Russian proof." His most famous paper deals with a rigorous proof of Boltzmann's ergodic hypothesis. This work brought him recognition outside the narrow circle of specialists. People who are quite far from ergodic theory heard about it. Briefly, Sinai considers a system of *n* hard balls inside a vessel of cubical shape and claims that this system is ergodic on a surface of constant energy. And this is in a sense a proof of Boltzmann's ergodic hypothesis. In a limited sense of course, but it can be compared with the proof of stability of the Solar system by Kolmogorov and Arnold. Those are achievements of a comparable level.

The story behind this paper is very strange. It was a part of Sinai's Doctor of Science dissertation, one of its three chapters. An announcement was published in *Dokldy*. It is the most important foundation of Sinai's scientific fame. However, the full text of the paper was never published because Sinai discovered some serious flaws in the argument. I think that these flaws were not superficial but systemic. They were not merely minor errors but were a direct consequence of his "physical" approach. The issues he tackles in his paper are of incredible technical complexity, but the level of technique available at the time was not such as to enable him to write the kind of proof that would meet the rigorous standards of mathematical scholarship. In this paper he has a number of good ideas, and these ideas could perhaps qualify as proof from the point of view of physics.

E. D. I think that what you are talking about may be interpreted in Sinai's favor. I heard many reputable people compare Sinai with Dobrushin. Both of them are excellent mathematicians, but each of them has his stronger and weaker sides. The approach that you call "physical" gave Sinai a clear advantage over Dobrushin, who was more of a mathematician.

A.K. And not only over Dobrushin.

E.D. Nevertheless, Dobrushin's approach to the general Gibbs theory is completely fundamental and it is a world-class achievement. But Dobrushin is more mathematical than Sinai.

A. K. As I said, Sinai never published the full text. When he realized that there were some serious problems with his arguments, he set about to fix them. The result of his work was his first paper on billiards published in 1970, which provides a solution of the problem for two balls.

E. D. What is the relationship of Wasserstein's work to Sinai's paper?

A. K. It is completely unrelated. Sinai worked on billiards together with Bunimovich.[3] It is interesting to notice that the paper published in the 1970 in the *Uspekhi* still contained many errors although of a more traditional character. Sinai worked on the problem together with Bunimovich and they published a joint paper. It was Bunimovich who finally removed the remaining errors.[4]

E. D. Bunimovich? The one who specializes in mechanics?

A. K. No, it is his son, a student of Sinai. No doubt, Sinai's research intuition is unrivalled, but in my view he lacks a sense of mathematical style.

E. D. It's interesting that you say that because in the last few years I've been having a hard time understanding him. Perhaps this is more of my problem than his, but I think the reason is exactly that: he speaks in a scientific language that I don't entirely understand.

A. K. I totally understand that. Although you and I didn't have a lot of direct mathematical interaction, I attended a few of your lectures and I know that style, form, and structure are very important to you. Perhaps it is no coincidence that your first famous

[3] http://en.wikipedia.org/wiki/Leonid_Bunimovich

[4] (Katok's note added in translation) The 1973 joint paper by Bunimovich and Sinai still has no complete and rigorous proof for this basic case. For that one had to wait another fifteen years and emergence of another student of Sinai, Kolya Chernov.

paper contained an elegant presentation of classification of simple Lie algebras where form and style are central.

This is exactly what Sinai did not have, and I think it had a profound effect on his subsequent activities. The person whom I credit with developing my sense of mathematical style was Dmitri Viktorovich Anosov.[5]

E. D. Anosov?

A. K. Yes.

E. D. Do you also consider him as your teacher?

A. K. I cannot say that he was my teacher because when I started to work with him I was already fully developed as a mathematician. Nevertheless, a sense of style is something that I picked up from him.

E. D. I don't know him at all. What about Kirillov? Did he have any significant impact on your development?

A. K. No, he didn't. I always admired him, and we organized a number of joint projects, like the seminar that you just mentioned and other things, but we never seriously collaborated. This was partly because at that time I didn't know enough about the areas that Kirillov was interested in, and partly because I wasn't a big fan of his approach. I wouldn't call it superficial, but in my view Kirillov placed too much emphasis on presentation. But this is not as important. The more important reason is the first one.

E. D. Arnold also knows how to present things nicely.

A. K. Yes, he does.

E. D. It is not his only virtue of course.

A. K. I had some sporadic interaction with Arnold. It was useful both for me and for him, at least at the time when he was interested in dynamical systems. I answered certain questions that he asked in his papers.

1:09:45-1:15:30

E. D. Let us get back to Yakov Grigorevich [i.e. Sinai].

A. K. He was a very good supervisor but in a somewhat unusual way, at least for me. I must say that I have never solved any problem or published any paper that was directly

[5] http/en.wikipedia.org/wiki/Dmitri_Anosov

formulated or inspired by Sinai. It doesn't mean he didn't suggest any problems. It means that the ones he suggested were either not very interesting, or, more often, did not work out.

Nevertheless I consider Sinai my principal adviser, not only formally, but in substance. I owe Sinai more than I owe anyone else. That was education in a broad sense, learning approaches to mathematics. He was also genuinely interested in my own projects. He expressed lively interest in the work I was doing with Stepin[6] and Oseledets. He stimulated that activity. He was a very good adviser but not in a common sense. He was not able to formulate a problem that would stimulate the student to "grow" on that problem, at least not in my case. But he was able to teach and inculcate a certain approach to mathematics.

Now let me say a few words about his personal qualities. There was a period when I admired him. It is quite natural and healthy for a student to admire his teacher. But later my attitude changed. There are several reasons for that. One of them has to do with his attitude toward his famous work on the hard ball gas.

E. D. What exactly do you mean?

A. K. He never honestly acknowledged the errors contained in his paper.

E.D. That there were gaps?

A.K. What do you mean by "were"? They are still there. The whole thing does not exist at the moment. He always tried to avoid broaching the subject. I am not even talking about acknowledging them in print, although Petrovski and Landis did just that.[7] I can understand him. After all, his paper was a part of his dissertation and acknowledging errors may have caused serious problems. But, so far as "Hamburg's score" (i.e. serious and honest assessment) is concerned, he was not entirely honest. He was saying that he was working on it. He was not very happy with people who knew what the actual state of affairs was and who were not above criticism due to their status. In particular, <u>one reason for our fallout</u> is related to the fact that he knew that I was fully aware of the situation. I don't want to go into too much detail about that because it will become too personal. I will just say that in his conversations with colleagues, including foreigners, he used to provide misleading or,

[6] See pp. ix-x in http://www.ams.org/books/conm/567/conm567-endmatter.pdf.
[7] A Landis interview is a part of this Collection. A story of the gap in a paper of Petrovski and Landis can be found in the interview of Il'yashenko.

let's just say, not entirely accurate accounts on the progress of his work. I think that an honest response would have been something like this: "The work does not exist but there is a program. Some parts of this program are realized in such-and-such papers, and I hope to realize the whole program in the future." But this is not what he was saying. He was saying that he was in the process of writing.

E. D. Everyone has his small weaknesses.

A. K. I don't think it is such a small weakness because this work is the foundation of his fame ….

E. D. This is not his only work.

A. K. You are absolutely right but his fame among physicists is to a large extent based on it. His other works are more technical. To understand them one needs to have certain amount of mathematical culture. But ergodic hypothesis of Boltzmann is an entirely different matter. I think that some of his actions and attitudes toward people are related to that and that he has a tendency not to be truthful with himself and others.

(1:19:50-1:26:25)

A. K. Anosov, on the other hand, has a reputation of an opportunist and really a deep opportunist. He works in Steklov Institute, where he was never considered as an open dissident. To be an open dissident in Steklov Institute, one has to be a Shafarevich or a nephew of the president, and even for them dissent is not always allowed. So in the Steklov Institute Anosov never allowed himself to express open opposition.

Nevertheless, I respect him a lot. It is one of those situations where non-participation is an act of courage. He is a man who has a clear understanding of the situation and does not deceive himself. He is absolutely honest with himself and other people like myself.

E. D. Did you discuss political questions with him?

A. K. Yes, we were on friendly footing for many years. This could be one of the reasons for the rift between Sinai and me, although I am not entirely sure about that.

Anosov was a man who clearly defined his own ethical boundaries, which consisted in that he never allowed himself to be in open opposition. He never transgressed these

boundaries and was quite outspoken about that. This is why he acquired the reputation of a time-server.

E. D. Things are not always so simple. There are situations when you have to decide whether you are going to ruin someone's career or not.

A. K. Wait, I haven't finished yet. On the other hand, within the boundaries that he set for himself he was completely honest. I am not sure how widely that was known but takes Anosov's role in the VAK and on the editorial boards of the *Izvestiya Matematicheskih Nauk* and *Matematicheskie Zametki*. This is not the same thing as serving on the editorial board of the *Functional Analysis* or *Uspekhi* where you have mostly good people.

E. D. Yes, Stechkin[8] presided there.

A. K. Anosov served on the board of *Matematicheskie Zametki* for many years, and I know it for a fact that not a single paper reviewed or handled by Anosov was accepted or rejected on any other basis but their scholarly merit, without a single exception.[9] The same is true for his work in the VAK at the time when the witch-hunt in algebra was in full swing and a lot of injustice was being done. I am talking here about the candidate dissertations. Anosov did not deal with the Doctor of Science dissertations. Anosov is a man who has a lot of access to power thanks to his position but he never tries to play games that he is not supposed to play.

E. D. He wants to be a corresponding member of the Academy of Sciences, right?

A. K. Yes, I think so.

E. D. The question is whether he is prepared to pay the price.

A. K. Let me just say what I know because I am one of the few people who defends him. He never had many friends because he comes across as a boring person, although he is extremely intelligent.

E. D. How old is he?

A. K. He was born in 1936. In his position he never had to deal with doctoral dissertations. He dealt mostly with candidate dissertations. It was a committee where he had to serve together with the likes of Ulyanov and similar people. He never rejected

[8] http://en.wikipedia.org/wiki/Sergey_Stechkin

[9] See Notices of the Amer. Math. Soc., Vol.25, No.7, 1978. Discrimination in the publication of mathematical research works is described in a Letter to the Editors "The situation in Soviet mathematics" signed by a group of prominent American mathematicians.

dissertations written by Jews, and these were not only dissertations by people whom he liked or who were my students. He evaluated them solely on their scientific merits. That he is not an anti-Semite by his convictions goes without saying. But we are talking about behavior, not just inner convictions. He tried to avoid confrontation, although he was in a state of permanent confrontation with Pontryagin.

E. D. This wasn't a disagreement on political issues, was it?

A. K. It was. What other issues could they possibly disagree on?

E. D. Scientific?

A. K. It was both. But he didn't like open confrontation.

Part 2 of Interview on January 1981

(2:55-9:35)

A. K. When in 1972 Borya Moishezon[10] applied for a permission to leave the country, it cast a negative light on our department (in CEMI[11]), even though he wasn't a boss of any kind. Our administration suggested, although in a very mild way, that we condemn him. This suggestion smacked of direct blackmail but this blackmail was rather soft, or let's say, generous: we would get a lot if we only agreed to condemn him.

All the people in the department were invited to a meeting under the pretext of discussing the work of the department and its future plans. In reality, however, this was nothing other than a loyalty check in connection with Moishezon's application. It was an interesting meeting, and I am proud to have taken part in it, not so much because of anything I myself did or said but because of what transpired in that meeting in general. There were about thirty people in the department. The meeting was scheduled out of the blue. Many people learned about Moishezon's decision to emigrate on the same day. I knew about it earlier. Moishezon applied for an exit visa immediately after the May holidays, while the meeting took place on May 11 or 12. Many people didn't want to come to this meeting because they understood that it was organized for the purpose of bashing

[10] http://en.wikipedia.org/wiki/Boris_Moishezon

[11] Central Economics and Mathematics Institute of RAN

Moishezon, but the director assured Aron[12] that the issue of Moishezon would not be raised at the meeting.

So the whole department assembled. Moishezon himself was in attendance, but he didn't seem to care about anything at that moment. The director called forth Aron. At that time Aron was already in a precarious situation.

E. D. Sorry to interrupt you, but Aron himself described all of that in his book. Can you comment on his version of this story?

A. K. Unfortunately, I haven't read that part of his book yet. So I will just give you my version, and you will compare it to his.

The meeting was a major event. Aron was supposed to present a report, which he did. I am not going to talk on the subject matter of the report and how competent it was. This is beside the point. After Aron's presentation, the director grumbled that he wanted the people who were mentioned in the report to come forth to the podium as well. These were people who showed some activity in economics. I was one of them. We spoke. Everything went nice and smooth. Then Fedorenko[13] (the director of the Institute) said that he wanted us to react to the news that our colleague, Moishezon, decided to leave the country. This was said in a very nonchalant kind of way, without any drama in his voice. So we had to decide how to react. After the fact there were different views about what we should have said. Vadim Arkin, for example, said that we should have spoken in a very vague way, saying what Fedorenko wanted to hear but at the same time not saying anything against Moishezon. But, you see, there is a huge difference between not saying anything and being vague. There were about thirty people at that meeting and quite a few of them were time-servers. In fact, all of us were to a certain extent.

E. D. True. Otherwise we would not have been able to survive in that system.

A. K. But there were also people like Arkin, who went so far as to provide theoretical justifications for this kind of behavior. It was characteristic of the atmosphere in the department at the time that nobody said anything. Aron of course tried to weasel out by saying that many people just learned this news and didn't have enough time to form a collective opinion on the matter. Fedorenko responded that the matter was clear as a bell:

[12] An interview of Aron_Katsenelinboigen (the department head at CEMI) is a part of the present collection . See en.wikipedia.org/wiki/Aron_Katsenelinboigen.

[13] http://en.wikipedia.org/wiki/Nikolay_Fedorenko

our colleague decided to emigrate. Basically, he had our backs against the wall. He said that we had to speak first and that he was going to give his opinion after. This was a form of direct although mild blackmail. And you do know what happened? Nobody uttered a word. We were sitting there in complete silence, and this is something I am proud of. He then urged the party members to step forth. Luckily the only communist in the department, Misha Antonovski, was away on a work trip. Then he appealed to the members of the Komsomol. Nobody stepped forth. Invoking Komsomol wasn't quite as intimidating. We continued to sit there in silence. "Okay, fine", he said, "then I will tell you what I think." And he started: not enough focus, lots of unimportant directions, and so on and so forth. He didn't say anything about Moshezon, but he let us understand that since we refused to show goodwill, we should not expect it from him either. At that moment we felt ourselves heroes and even martyrs. I remember how after the meeting Borya Mityagin[14] and I were strolling through the Neskuchny garden[15] discussing the possible consequences of our refusal to comply.

Supplement

Interview December 1980, Ithaca, NY

(36:20-40:40)

A.K. The group 4A deserves a separate story.

E.D. Whom else I know in that group ?

A.K. Vitya Kac, Ilya Novikov, Yulik Ilyashenko, Boris Grigoriev (Novikov and Grigoriev were your assistants at School No. 2).

E.D. The group had about 25 people, right? Looks pretty strong.

A.K.. Pretty strong?! In a sense, it was the cream of the class.

E.D And how did this happen?

A.K. It is an interesting story. The year I entered the university, 1960, was quite liberal, there was no explicit witch-hunt but a certain strange thing happened during the admission. It would be interesting to know whose idea it was. The entrance competition was joint for the mathematics, computational mathematics and mechanics divisions of

[14] http://www.math.osu.edu/people/mityagin.1
[15] The oldest park in Moscow.

Mekhmat, although each applicant indicated his/her preferred division. It was assumed that candidates with higher examination scores would be assigned to their preferred divisions and the rest would be distributed among the divisions to fulfill the quotas. But strangely many applicants who received the highest examination score (I was among those) were arbitrarily assigned to mechanics or computational groups. This of course led to protests among those students, and a certain pressure was put on the Mekhmat administration to correct that. I was assigned to the mechanics division, and I was one of the first to protest. It is interesting to contemplate how my career would have developed had I remained in the mechanics division. I was lucky. Lev Abramovich Tumarkin, whom I knew via a family connection, helped me. A recommendation from the faculty member who ran a seminar that I attended was also provided. Anyway, it was demonstrated that I had a clear interest in mathematics, and not in mechanics (nowadays I am working in mechanics in a way) and I was transferred to one of the groups in the mathematics division.

Pressure continued, various students protested helped by faculty members, and eventually the administration decided to yield and to transfer those who actively protested to the mathematics division. But for that it was necessary to create an additional group in the division. This new group would partially absorb the most committed transfers, people from mathematical circles and suchlike. The other part would come by dividing the group 4 since it became too big. So a situation developed when the preliminary list of the students for the new group was made by an initiative group of students themselves. In particular, we decided whom to invite form the group 4 (Ilyashenko was one of those). So the group was formed by a kind of ``clique''. A girl from Moldavia, Maya Hertz was particularly active in the process. Thus this new group was not only strong academically but it also possessed a certain spirit of independence that actually showed up on several occasions.