

## Translation of the Eugene Dynkin Interview with Evgenii Mikhailovich Landis

E. B. Dynkin: September 2, 1990. Moscow, Hotel of the Academy of Sciences of the USSR.

E. B.: Let's begin at the beginning. You entered the university before the war?

E. M.: I entered the university before the war, in 1939. I had been there two months when I was recruited into the army---then the army recruited people the way they did up to a year ago. So there I was in the army, and there I stayed till the end of the war. First I was in Finland<sup>footnote{Finnish-Russian war, November 30, 1939-March 12, 1940, essentially a Soviet war of aggression. Hostilities were renewed in June 1941, when Germany invaded the USSR.}</sup>, where I was wounded, and for some time after that was on leave. Then I returned to the army, and then the war began<sup>footnote{On June 22, 1941, when Germany invaded the USSR.}</sup> and I was in the army for the whole of the war.

E. B.: In what branch?

E. M.: At first I was in the infantry, and after returning from Finland and recovering from my wound, I ended up in field artillery.

E. B.: The probability of surviving the whole of the war as a foot-soldier would have been almost zero.

E. M.: I think that this would have been true also in artillery, since this was field artillery, and that is positioned very close to the front. But this is how it was. When the counter-attack before Moscow began in the spring of 1942, our unit captured a German vehicle---a field ambulance, fully fitted out with new equipment, complete with a set of instructions---in German, naturally. These I translated, figured out what went where, and when they took it to the army's medical section, they had me go with them as interpreter-demonstrator. After I had demonstrated how everything worked, an old man---so he seemed to me then---came up to me and asked me who I was and if I wanted to work in a laboratory. And I, without asking what sort of laboratory, or where or how, said: ``I do!'' My fate was decided there and then by a telephone call. It turned out that the old man was the head of the medical-epidemiological detachment (MED). the Medep detachment was a small unit attached to the army, made up of a number of epidemiologists and laboratories. Two of the laboratories were set up in buses, and a third was called the ``base laboratory.'' There were these boxes, and when the army took up a new position, new dugouts would be dug (if there was no hut) and the boxes---the supplies---would be unpacked. The laboratory was bacteriological and medico-chemical, for detecting poisonous substances. And that's where I ended up. The unit had been formed in Moscow and staffed with people working in scientific institutions there.

E. B.: At that time, you had no, let's say, tertiary degree....

E. M.: Nothing at all, but the fact was that most of them had been teaching, and were missing having students very much. So I became their student, and fairly quickly acquired an education in bacteriology and applied chemistry. Soon I was made a medical orderly and not long after that a lab assistant, and, having of necessity to be promoted to the corresponding army rank, I became an officer. And there I remained. But the unit was a part of the army, and involved in the war, and by the end of the fighting only half of the original staff of that Medep detachment were left.

E. B.: But all the same the chances of surviving were no longer quite nil.

E. M.: Of course, it was thanks to my being in that unit that I survived, because when I was a foot-soldier in the front line during the first German offensive in the early part of the war, the units that remained undefeated were very thin on the ground, and in the case of those that were defeated, the probability of not being killed was about  $3/4$ , so that after a few more defeats.... But thanks to my getting into the Medep detachment, firstly, I survived, secondly---I can appreciate this now---when I returned after a six-year interval to Mekhmat<sup>{The Faculty of Mechanics and Mathematics of Moscow State University.}</sup>, I very quickly got back into form---so much so that by the end of my first year I had already written a paper. Even now I think that paper not at all bad, just on a topic that people don't work on these days, namely set-theoretic topology. I worked on the theory of functions of a real variable under Sasha Kronrod, and made rapid progress.

This was largely due to the way Sasha taught---which in fact I have come to think the most rational one. In the process of solving some specific and sufficiently difficult problem, you very quickly get to the heart of the matter, and anything you need to learn by the way you absorb without noticing. The first few problems were actually textbook exercises, and the problem that I solved towards the end---not of the academic year (my first), but before I began to work on genuine, completely serious problems---was the exercise, essentially, suggested by Kronrod of reconstructing Kolmogorov's proof that the Fourier series of an  $L_1$  function may diverge almost everywhere---even everywhere. In its time this result was considered sensational, but in fact the technique used there is rather primitive, doesn't require much knowledge. In some sense, that is a virtue of the theory of functions of a real variable: it inculcates in one the ability to carry out long logical chains of argument, starting from rather little in the way of knowledge. And after that....

E. B.: What year was that?

E. M.: That was the beginning of 1946. I was late, I wasn't demobbed immediately. When the war ended I was in Germany, and remained there for some time.

E. B.: Was Kronrod a graduate student at that time?

E. M.: Kronrod was then not yet a graduate student. He also had been in the war.

E. B.: You probably know---or maybe not---that towards the end of the war an effort was made by Sofia Aleksandrovna Yanovskaya<sup>{Specialist in the history of mathematics, mathematical logic, and the philosophy of mathematics. Promoted research in mathematical logic in the USSR, and edited and published mathematical works of Karl Marx. Dissuaded Ludwig Wittgenstein from immigrating to the USSR. Worked chiefly at Moscow University. Lived from 1896 to 1966.}</sup> to extricate Kronrod from the army. He had gone into the army as a volunteer, and there his health failed, despite which they wouldn't release him. Sofia Aleksandrovna was a very kind, good person, and she suffered greatly from the ideological role she had to play. Although she was unable to give that up, she tried in every possible way to help people. She looked after me as if I were her son---you know that. And, when I informed her of the situation, she used the connections established when she had been a well-known and influential figure. Thus she turned to Suvorov<sup>{Possibly Georgii Dmitrievich Suvorov (1919-1984), Soviet mathematician.}</sup>, who was then head of the Scientific Section of the Central Committee of the Party---from which he was soon ejected. He respected her and was obliged to her for some reason, it seems. In any case he supported her proposal, and soon the main political commissariat of the army discharged Sasha, placing him at the disposal of the university. And that was a wonderful thing to happen because Sasha was an utterly exceptional person, and his health was already very poor---something to do with his heart and his blood.

E. M.: Right up to his death he never fully recovered from his wounds. That's why they placed him on disability---he continued to work, but he had that little red book.

E. B.: I recall that that was right at the end of 1943, because it was then that Sofia Aleksandrovna returned to Moscow. I got to know Sasha in his first year.

E. M.: And when I returned as a first-year student, he was in his fifth year.

E. B.: And I would have been a graduate student, because there had been no break in my studies.

E. M.: I spent some time after the war in Germany, and then we spent quite a long time returning to Russia through Poland---an interval in eastern Poland, and then back here.... I was demobilized somewhere around the middle or end of November. But nevertheless, when I started my first year, I rapidly hit my stride. At first I thought this was typical of everyone.

E. B.: No, not at all.

E. M.: Possibly that's so, because everyone says that an interval away from studying is harmful.... I do now understand just how harmful since I have encountered such cases, but in my own case, my experience was that

six years away changed nothing---doubtless only because I was using my brain, and it was unimportant that I was working in other sciences, just so long as my brain was kept active. Towards the end of the war I had even managed to write a paper on biology; I didn't get around to publishing it, but there it was---thanks to the fact that I did indeed have very good teachers. They were doctors, professors at Moscow tertiary institutes---real professors. And they were glad to have a student, and tried in every way possible to help me. When I returned, I missed working in a laboratory very much because a laboratory is very useful for coping with everyday life. For example, at one time I needed a passport photograph with the standard blank corner\footnote{Where the photograph would be stamped, once glued in the passport.}, but the one I had had no such corner. Back then it would never have occurred to me that I should go and get myself photographed again, since I knew perfectly well that if you apply fuming nitric acid, then the silver which is in the photograph will in a few thousandths of a second be transformed to silver nitrate, which is highly soluble. Hence if you rub the appropriate corner of the photograph with a little nitric acid and then let water run onto that corner, the darker part of it will be dissolved off. And that's what I did, and went about with the passport containing this photograph until it had to be changed---probably when I had completed university. Yes, generally speaking, it can be very useful for everyday things. We were taught physics then in first year....

E. B.: Silly physics, I imagine. Perhaps by the time you arrived he was no longer there, but we were taught by Mladzievski\u0438.

E. M.: At first we were taught by Kalashnikov, and that was very good, and then a certain Mironova. But she was no physicist, and I stopped attending lectures---went a couple of times and stopped. But there were also laboratory sessions.

E. B.: In physics?

E. M.: There were indeed experimental sessions in physics, and also theoretical exercise periods, and the Fizfac\footnote{The physics faculty at Moscow University.} instructor who ran these proposed that I work with her in the laboratory---with mass spectrographs. So I began to work there, and with pleasure. The only reason I finally left that lab had to do with the fact that in those days the vacuum pumps used mercury, so that working with them was potentially dangerous. So I left, and was then away from experimental physics until later when, in a different capacity, I began to work in the Alikhanov institute\footnote{The Institute for Theoretical and Experimental Physics founded by the Soviet physicist Abraham Isaakovich Alikhanov in 1945.}.

In the meantime I continued working with Sasha. We proved a theorem, and some years later we discovered that it had been proved earlier by Sard. Do you know Sard's theorem\footnote{Stating that the image of the set of critical points of a smooth function from one Euclidean space to another has Lebesgue measure zero.}?

E. B.: Yes, and I remember the celebrated paper of Landis and Kronrod.

E. M.: Yes, it was called the Landis-Kronrod theorem [in the USSR] for a long time, but now I understand that that was wrong: Sard proved it in 1942 or 1943, I think, and we published it only in 1946. We began to think about it when I was in my first year, and completed it when I was in second year. And then I gave a talk on it to the Moscow mathematical Society.

But I continued my studies. We had very good teachers. In my first year, before I met Sasha, for some time I was unable to decide where my interests lay. For a while I attended Shafarevich's seminar. There they were busy with the quaternions, and this didn't very much appeal to me, but when I joined Sasha's mathematics circle, I began to work very hard on the theory of functions of one or more real variables, and on combinatorial topology.

I continued attending the compulsory lectures. First year analysis was taught by Khinchin---and that was boring, more or less, because from the time I began working on functions of a real variable, I had very rapidly absorbed a great deal of classical analysis. All the same, he lectured in a very pleasant way, and I listened. Yes, in first year I was very disciplined and went to all the required lectures. So what did we have? As I said, Khinchin taught analysis, and that was rather dull. Delone taught analytic geometry and that was fun....

E. B.: Yes, we enjoyed it too. Fun, but not very substantive.

E. M.: ... yes, but never mind. Shafarevich taught algebra, and got through rather a lot of material, and lectured quite well. And then there were the other subjects. Astronomy was taught then. Orlov taught it to us. Those lectures were also ... lots of fun---because he told jokes. I believe he's a quite well known astronomer. In any case, my sister, who's older than me, graduated in astronomy before the war, and she related to this Orlov with a great deal of piety. But he did nothing but tell jokes---and with a totally straight face; it was very funny. We learned a great variety of interesting things, for example, that Tycho Brahe had a silver nose: his nose had been cut off in a duel, and they made him a silver prosthesis---things like that. And there was physics. I listened to the physics lectures for a while and then stopped: I listened to Kalashnikov, but not to Mironova. But in everything else I was a disciplined student, although I should mention that in my first year I was reprimanded for skipping lectures. Periodically they carried out spot checks, and on the one day I happened to miss some lectures, I was caught.

Then in second year Gelfand lectured on linear algebra, and I listened. Nemytski\u0439 taught ordinary differential equations, and I did not listen.

Mechanics was taught at first by Nekrasov---and I didn't listen---then Chetaev took over, and I found it rather interesting. And in third year I was also discriminating as to what lectures I attended. I attended Kolmogorov's third-year analysis course, and that was very interesting.

E. B.: An outsider would judge him to be ineffective as lecturer.

E. M.: Yes, but it was like this: From the beginning only a few were really listening to him. As I subsequently understood, only two really understood him: Lyosha Filippov and I. The thing is, that at some moment while he was lecturing he would begin to walk back and forth rapidly in front of the blackboard and mutter---and that was the most interesting part, because he was then telling us what he was thinking. Evidently, when he was doing this---maybe I made this ascription later on, but I think that this is the way it was in fact---he was thinking of the structure of the subject-matter. For example, once when he was behaving this way, he was thinking about how to arrange sets so that they were similar in every part, that is, arrange matters so that if you take a small part of

a set and look at it through a microscope, then it looks just like the whole set. I was able to understand this completely because I knew set theory and measure theory well. But the rest were unable to understand him. I don't know if on those occasions he was talking for his own sake or for ours.

Each time this happened, it emerged out of the material of the lecture---when he came to a certain word, it would act as a trigger to start him talking about related ideas of his own. And for this reason I listened to those lectures with pleasure. I took notes of his lectures, and still have them. I still have lecture notes of two courses---those of Petrovsky and Kolmogorov.

E. B.: Those shouldn't be lost. You should publish something about them.

E. M.: Yes, I want to write two papers: one about Kronrod, and the other about what, generally speaking, my conception of mathematics was back then, and how I related to the mathematicians ... at least up to the mid-1970s, when a new generation was in the ascendant---the generation of Arnold, Manin, Kirillov (all of practically the same age), Novikov (a little younger)---a high-powered generation. But after them only isolated people---there are powerful mathematicians both within the university, and outside it, but they no longer form such a dense growth.

E. B.: And who supervised your graduate studies?

E. M.: Nobody.

E. B.: You were never a graduate student?

E. M.: No. It was like this: I completed my undergraduate degree in 1950. From my third year I worked on differential equations under Petrovsky, and two departments---the department of functions of a real variable, or rather the department of function theory---that was its general name---and the department of differential equations---recommended me for graduate work. At that time I had a high estimate of myself---when I completed my undergraduate program I already had five published papers....

E. B.: And good papers they were....

E. M.: So I did not for a moment doubt that I would be accepted into the graduate program. I sat for my finals, obtaining five<sup>footnote</sup>{The marking scheme in the USSR, as also in tsarist Russia, was out of five, with three representing a mere pass, and two or less a fail.} in mathematics and five in my chosen foreign language. Incidentally, I understood later that I had made a stupid mistake: at that time the choice was between French and German---there was no English. So one had to choose between French and German.

E. B.: But I was before you....

E. M.: And you could choose English?

E. B.: It was a possible choice as a second language, I think.

E. M.: Well, for us there was no choosing two languages, only one, and groups were then formed according to the language chosen. One had to choose

between one of two languages. I had studied French as a child, and then forgotten it, but---as it later became apparent---not entirely: I can, of course, read French mathematical texts, but I didn't think I could speak French or read French fiction. But it's true that when there is an interview

on television with a Frenchman and at first they let him say something in French and wait before beginning to translate it, then I more or less understand. And then later, when my daughter Lena was studying French as the foreign language chosen in connection with her graduate program, I found myself correcting her. But all the same, I would say that I had basically forgotten French. Incidentally, that was the first foreign language I studied, starting when I was five years old, and was the foreign language that I knew best. And then German. I have been studying German my whole life from some point on. I don't have a talent for languages. I had English also at school. If anyone else had studied languages as much as me ... since after all I came from an educated family where it was considered that every educated person should know three languages, so I had to put a lot into them, but without much to show for it.

I knew German better than other languages, but only passively. But then when I arrived in Germany towards the end of the war, I began to speak it. Up till then I could only read it.

And then in university, when they saw that I spoke German quite fluently, they suggested that I sit the exam immediately, and so dispose of the language requirement once and for all. So I did that, but later regretted it, although I don't really know if French would have been much use to me, while English wasn't a choice.

To come back to the topic, in the graduate entrance exams I got five for German, five for mathematics, but was given a four for Marxism.

E. B.: A four? Well, that's not so bad.

E. M.: Not bad. I knew that this was the only subject where they grill you, but I knew the "Short Course" off by heart.

So I reckoned that since I had a score of five, five, four---and the competition was nil---the question of acceptance must be a mere formality. I decided to learn English in the meantime, and began attending English lessons. But then, although the faculty accepted me, the ministry did not confirm the acceptance. I had intended to do my graduate work with Petrovsky as supervisor, and he was then....

E. B.: Dean?

E. M.: Not Dean, but sectional academic secretary---this was in 1950. In 1951 he became rector of the university. After a while Petrovsky told me that for the time being nothing could be done. He had made representations on my behalf, but in vain. He then told me that the deputy minister for higher education wanted to see me---Mikhailov, I think. And he said that I could pursue graduate studies in any town except Moscow. I replied that I didn't want to. So I didn't become a graduate student. The only thing that Petrovsky did manage to do was to obtain permission for me to sit for the basic exams<sup>{Qualifying the student to proceed to the defense of his dissertation.}</sup> for the candidate's degree<sup>{Roughly equivalent to the Ph. D.}</sup>.

For a whole year they tried to find appropriate work for me. First, they tried to arrange a position for me in the Geophysics Institute, where initially Shmidt was director and then Gamburtsev. So I began work in the laboratory run by Alexey Andreevich Lyapunov, and he also tried to help me. I set to work on the processing of signals from earthquakes, which was interesting enough, in its way. So I went every day to the institute, even started a time sheet, but since I was not yet officially confirmed there I was not paid. However since I was already getting on in years---in view of the six-year hiatus in my life---I felt I should do something, so began doing editorial work. I edited quite a lot of books, including all of the postwar editions of Luzin's textbooks. I edited the volume *{it The Integral and Trigonometric Series}* under my own name, and I also edited his collected works, but---as it somehow turned out---not under my own name; I edited them and received some money, but someone else's name appeared as editor of the finished product.

E. B.: Was that at the same time?

E. M.: That was at the same time, or a little later. Thus I was earning a living by editing, and working in the Geophysical Institute without pay. But in the end nothing came of that, and I had to quit the institute---that is, stop turning up for work and keeping a time sheet---and then I tried the School for the Working Young<sup>{Schools providing evening classes to young working people.}</sup>.

E. B.: At that time several of my former students, including Khasminskii, were in a similar position. Karpelevich was sent to Novochoerkassk, Berezin taught at the School for the Working Young.

E. M. The School for Working Young was the only place where you could get work. The branch where I worked was in Perovo. Now Perovo is considered part of Moscow, you can get there by the Metro, but then it was outside the city, and you got there by electric train. It had the advantage that it was an evening school, so I was free during the day, and could attend seminars. And in the evening I went to the school---in those days I didn't need much sleep. In this way two years passed, during which time I satisfied the minimum requirements for the candidate's degree, worked a lot on mathematics, and published quite a few papers. Then in 1953, after Stalin's death....

E. B.: Things became easier....

E. M.: Yes. He died in March, and sometime in May, I think, Petrovsky said to me: ``Well, now you can apply." So I submitted a dissertation on the spot, and defended it in June.

E. B.: And he hired you. You first worked as a lab assistant, I believe?

E. M.: Petrovsky hired me, and I successfully defended my dissertation, but they wouldn't release me from my job at the school. Then again in autumn Efimov, the head of mathematics at the Forestry Institute, offered me a position. I would, of course, have gladly accepted, but they wouldn't release me from my position at the school. There was then a law that one could not quit a job of one's own volition. So for some time I worked in both places---both outside the city and on different train lines. In the daytime I was at the Forestry Institute, and went to the school in the evening, and that was rather hard on me. At some point I applied to the school to leave, and the director said that he would release me if I found someone to take my place. I found someone, but he still wouldn't release me. I found another replacement, and again he refused to let me go. So then I simply stopped going to the school---and I received an official summons. (This was sometime in 1954.) This caused me a little anxiety, and I shared my concerns with Petrovsky, who told me not to worry because after a little he would be offering me a position in the university. He was then rector. He initiated discussions with the Ministry of Communications, which at that time had jurisdiction over the school. I was then summoned to the Ministry, to the office of the deputy minister, who asked me if I would like to work in the Moscow Institute of Communications Engineers. I said no, I don't want to, and he said: ``What a pity, what a pity,"---and immediately set his imprimatur on my application---which I had been told to bring with me. Thus it was that in March, 1954, Petrovsky successfully hired me to work in the university.

E. B.: That was the first thaw. My own career trajectory was surer,

because I had already completed by graduate degree, and Kolmogorov had hired me to work in the university. But when in 1951 I successfully defended my doctorate<sup>footnote</sup>{A more advanced degree than the candidate's degree, requiring substantial and continuing research and opening the way to appointment to a professorship.} and Kolmogorov tried to have me made a professor, there was absolutely no chance. He went to Prokofiev, the deputy minister of higher education, and Prokofiev said to him: "Let him sit still and not make such a fuss." Only in 1954, after Stalin had died, was I chosen from among the usual competition.

E. M.: Well, I got a university position, and am there to this day. But I should also mention that prior to that Kronrod, who had been working for some time in the Kurchatov institute---that is, the Laboratory of Metrical Instrumentation---and had then transferred to Alikhanov's institute---then the Institute of Heat Technology, and subsequently the Institute of Theoretical and Experimental Physics---, had tried to get me hired there, but nothing came of it. And then in 1953, around the same time as Petrovsky hired me, Kronrod was also permitted to hire me. So I had a choice as to where I might go. I decided I'd go both there and there---although Kronrod was against this variant.

E. B.: He was a somewhat jealous person, I would say. He likes people, but tends to be jealous of them.

E. M.: No, I'm wrong, it was later. I was already working at the university when Kronrod got permission, and he was thinking that I would quit the university and go and work with him. However, just at that time I got married, and then in 1957 my daughter Lena was born and the university gave me a room. That was decisive for me. But for some time I worked in a full-time capacity at the university and half-time at the Alikhanov institute, and then the other way round when I was given an apartment in the worker residence of the Alikhanov institute---where I live to this day. After a little, I resumed full-time status at the university. But I fulfilled my obligations absolutely in both places. I didn't favor one over the other---it was just a question of where the record of my work was kept.

E. B.: Did you continue to work with Petrovsky?

E. M.: Yes, I continued to collaborate with him. And in the institute, I worked on rather interesting problems related to the processing of the observations being made there. I left there in 1968, but I might mention that the programs that I participated in writing are still being used after all these years. Well, all right, they have been modified somewhat, but their basic structure has been preserved. The fact is that I also enjoyed programming. Kronrod quite suddenly stopped working on classical mathematics, and went over to computational mathematics.

E. B.: Yes, that was his passion. I recall how he even announced to the

Society<sup>{The Moscow Mathematical Society}</sup> that mathematicians are behind the times, that a new kind of mathematics had arisen, and that it was time to bury the old kind.

E. M.: In those days they had great hopes for computational mathematics---not only computational mathematics, but machine mathematics<sup>{That is, mathematics done by computers.}</sup>---very great hopes, since its potential was then unknown. And then they worked with tremendous enthusiasm on what is now called ``artificial intelligence." I also got carried away by it. The computers were miserable then, and unreliable. The first computer that I worked with belonged to the Institute of Energetics---not the educational one, but the one that's now on Lenin Avenue, and used to be on Kaluga Street. There stood the machine, M-2, constructed by Bruk<sup>{Isaak Semenovich Bruk (1902-1974), pioneer in the design and construction of computers in the USSR.}</sup> and the two talented engineers Kartsev and Matyukhin---both deceased, alas. Although measures were taken to keep the machine level, all the same one could only really work with it during the night. When a trolley-bus passed the machine would get off-kilter. But at night, when there was no traffic, it worked. That's how I developed my insomnia. And we used it for our own problems. In particular, we programmed it to play challenge <sup>{it Durak}</sup><sup>{That is, <sup>{it Challenge Fool}</sup>, one version of a very popular card game in Russia.}</sup> Now that's a very interesting game, but only played locally: it's played in Russia but hardly anywhere else, so you won't be able to organize international competitions---but it's a very interesting game. Kronrod proposed that we try programming chess. Our first chess-playing programs were pretty good and robust.

E. B.: Gera Adelson<sup>{Georgy Maximovich Adelson-Velskyb. 1922}</sup>, Soviet mathematician and computer scientist. Headed the development of a computer chess program at the Institute for Theoretical and Experimental Physics in Moscow. Now lives in Israel.} was also doing that.

E. M.: Gera Adelson also. And he continued to work on related things. I didn't work directly on the chess-playing project, but I did work on the <sup>{it Durak}</sup> program. In addition, I worked on problems that the physicists had come up against. Incidentally, Kronrod was rather brusque in his dealings with the physicists. There were excellent physicists there---for example, Pomeranchuk---with whom Kronrod had interests in common. And Gera and I found it interesting working with them. But most of the physicists were only of rather middling quality. In that connection, there was a definite pecking order: the weaker the physicist the more he used the computer, or demanded that computing be done for him. There was only one person who never had any use for the computer: that was Landau<sup>{Lev Davidovich Landau (1908-1968), prominent Soviet physicist. Made fundamental contributions to many areas of theoretical physics. Nobel laureate in 1962.}</sup>. Pomeranchuk used the computer, but only in connection with a few very clearly formulated problems.

E. B.: By the way, did you know Yuri Orlov<sup>{Yuri</sup>

Fedorovich

Orlov (b. 1924), formerly of the Moscow Institute for Theoretical and Experimental Physics, from which he was fired in 1956 for his human rights activism. Now Professor of Physics and Government at Cornell University.}

E. M.: I knew Yuri Orlov only very slightly.

E. B.: He is now my colleague at Cornell University. We meet now and then and he tells me about how he worked with Kronrod. He also talks about his speech in 1956.

E. M.: A whole group of young, talented physicists demonstrated then. They were shaken by Khrushchev's pronouncement.

E. B.: Orlov was then, after all, still a convinced communist.

E. M.: Of course, of course.... Those young people began to think about what should be done to avoid all that occurring again. There were various proposals---for example, arming the people ... there you are. But then they began to investigate them all.

E. B.: Who else was there?

E. M.: Tretyakov, for example, who remained at the institute. A very talented experimentalist---and theoretician, too. I didn't have anything to do with him directly, but he used to drop into the laboratory. Once a new, quite difficult problem arose in our group, of olympiad type. We were applying ourselves to it, and he came in and solved it on the spot.

E. B.: You've already spoken a little about Kolmogorov. I would like you to say something about Petrovskii. After all, you worked with him for quite a long time.

E. M.: I was in close communication with Petrovsky for a long time indeed. In my second year, I gave a talk to the Moscow Mathematical Society. Afterwards, Petrovsky took the initiative, and asked me if I was interested in differential equations. In fact, that was one of my interests; I had worked on them with Kronrod, if you remember.

E. B.: Kronrod and Gera Adelson-Velsky worked together on a problem proposed by Bernstein.

E. M.: Of course---investigating the level surfaces of functions, the condition on a surface cap---that's all very close to differential equations. Well, Petrovsky invited me to work under him, and from the next year---my third---I began attending his seminars: the small one for his students---which he ran together with Kreines---and the big one which he ran with Sobolev and Tikhonov. I remember how in third year I was given a paper to referee quite close to what I was working on. It was then that I began to work with Petrovsky. At the same time, I attended

the big seminar on the theory of functions of a real variable led by Menshov, Novikov, and Bari<sup>{Nina Karlovna Bari (1901-1961), Soviet mathematician. Worked on trigonometric series. Killed by a train in the Moscow Metro.}</sup>. The seminars were very similar in style: they tried to make sure that everyone understood everything. In Petrovsky's seminar, it was usually Petrovsky himself who was the last to understand.

E. B.: Yes, he didn't excel in quickness of comprehension.

E. M.: And it was only after the very last participant in the seminar had understood a point, that they began to explain it to Petrovsky. But do you know what sometimes happened? Everyone had understood the point incorrectly [<sup>{it laughs}</sup>], so that it was not for nothing that he had not understood.

E. B.: I'll interrupt you just to tell you that I was a student participant in Kolmogorov's and Gelfand's seminars---and also in Petrovsky's. I was once reading and attempting to understand his work on lacunae, still in manuscript form. There were places where I was unable to understand the argument, and went to him for help. He always said: "I'll think about it and tell you tomorrow or the day after." Occasionally, there was indeed something that needed clearing up.

E. M.: He used to say of himself that he was a slow thinker. He did indeed think slowly---but deeply. The quickest thinker of everyone I know is Mstislav Vsevolodovich Keldysh, and after him, Kolmogorov. I remember once when I was sitting a graduate examination<sup>{The exams had both a written and an oral part.}</sup> with Kolmogorov as examiner, I included references to new results that I knew---in particular, Whitney's result on the extension of a function from a subset to the whole space. The proof may well have represented the first application of partitions of unity. Kolmogorov understood immediately---in fact, once he had heard it there was no need at all for any further elaboration as far as he was concerned. That was perhaps the first time I realized that Kolmogorov had very rapid comprehension. Lyapunov also had rapid comprehension, but not always accurate. In Menshov's seminar, Lyapunov was always the first to get the point. Menshov was also a rather slow thinker, and at that time Bari already ... but it's wrong of me to talk of her like that. She was a very good woman and a very good mathematician, but at that time she was already having difficulties. At some point, she tuned out. In second year, I attended her seminar on almost periodic functions, so I am indebted to her also for my education.

E. B.: Of course, Sasha Kronrod was noted for a certain provocativeness towards his seniors. A kind of specific nihilism with respect to all authority.

E. M.: Yes, but everyone got used to it. When he defended his doctoral dissertation, the list of references was missing from one copy. His

examiners\footnote{Or, literally, ``opponents."} were Keldysh, Kolmogorov, and Menshov. In two of the copies the list of references was included, but in the third there were blank pages. Kolmogorov, who had a very high estimate of the dissertation, thought that Kronrod had simply decided not to make any references to the literature\footnote{So presumably Kolmogorov received the copy with the references missing.}. Apparently, he had no doubts whatever concerning Sasha's character....

So in Petrovsky's seminar, he himself was always the last to understand, but then often with good reason. When I began to work under him, he proposed a set of problems, including some that I was quite familiar with, Working with him was interesting.

E. B.: Now, when that's all in the distant past, there's no need to be overly cautious. Of course, Ivan Georgievich [Petrovsky] was outstanding not only as a mathematician. And, naturally his administrative activities must have sapped his strength---including the spiritual sort. Given that, in what shape was he to work on science?

E. M.: I began working with him in 1947, and not long afterwards he became secretary to the Academy. At that time, he had already stopped actively thinking about mathematics. I don't know if he was still capable of research or not. All one can say is that, objectively speaking, he didn't have enough time for it. Later he published collaborative work with Chudov and Oleinik. He would express certain ideas and listen to the reactions they prompted, but left it to others to develop them to the full.

E. B.: It cannot have been a simple matter, I think, in terms of lack of time and opportunity for concentrating on mathematics.

E. M.: Perhaps. And apart from that, he genuinely took his societal activities extremely seriously. He used to say that it was very interesting to him that he himself had thoughts on a topic, and that he could possibly be of use, and he got a great deal of pleasure out of the work. And when he was appointed rector---in the new building\footnote{Of Moscow State University.}---he spent full days there: arrived early in the morning, and left in the evening. And usually, if it was a day when he held one of his seminars or if some of his students just happened to be present, he would invite them to the rector's office on the ninth floor towards evening, and after he'd finished up his work, a general conversation would ensue, after which they would accompany him home. He lived at 13 Kaluga Street. They would all walk together there talking mathematics, or about mathematics, or about science. He continued to be interested in a variety of things. He began to learn English, read various books and textbooks, and became interested in physics. In his last years he organized a seminar with Ilya Lifshitz---the elder Lifshitz.

E. B.: The Lifshitz of Landau-Lifshitz?

E. M.: No, that's Evgeni Lifshitz; this was his brother, a significantly more powerful physicist. Petrovsky was interested in physics problems. But his interests were broad. I remember that in Fizfac they repeated the Michelson experiment<sup>{Probably the Michelson-Morley experiment, as it is usually known.}</sup> for his sake. I recall that he invited me....

He very often took me with him when something special was going on. When Kapitsa<sup>{Pyotr Leonidovich Kapitsa (1894-1984), Soviet physicist. From 1921 worked in Cambridge with Rutherford. Returned to Russia in 1934, but was forbidden to leave the USSR again. Nobel laureate in 1978 for much earlier work on low-temperature physics.}</sup> returned, he talked about the work he had been doing in his laboratory<sup>{In Cambridge?}</sup>. One problem he had been investigating was the following: if you take a rod [positioned vertically] and attach its lower end to a hinge and make the lower end vibrate up and down, then the [vertical] equilibrium position of its upper end will be stable. He explained how he had come to the problem, wrote down a differential equation, and derived the solution. Then he gave another explanation, calling it "an explanation for the ladies", as to why this occurs: when the rod is moving away from you and the lower end is moving downwards, then the upper end traces out an arc of a circle concave downwards, so that a centrifugal force is set up tending to move the upper end back towards you---and if the lower end is moving upwards then the argument is similar. This he called the explanation for the ladies. And only

then wrote down the equations? This problem later became well known and popular. I

don't know if it was known before he discovered it or not, but it was part of his nature to verify everything experimentally. He was giving a talk on it at his institute, and Petrovsky took me along to listen.

Petrovsky had been thinking a little about the foundations of quantum mechanics, and found the axiomatics unconvincing in one place. However, professional physicists didn't seem to want to talk to him about it. Well, they thought in a different way. He was very glad to hear that Einstein had encountered an obstacle in exactly the same place.

He became interested not only in physics. Being rector of the university, he would intersect with various of the sciences. In particular, he developed an interest in the biological work of Lorentz. Have you heard of the branch of biology called ethology? It's the science of the behavior of animals, and at that time it was in the pioneering stage. Petrovsky helped found a laboratory dedicated to that science, and he himself often used to discuss it with the biologists. Then, when the birch bark writings<sup>{Birch bark documents, dating back to around 1400, were first unearthed in Novgorod in 1951.}</sup> became fashionable,

he went on an expedition to excavate them. Thus at that time he was, in fact, less occupied with mathematics itself, and more with natural science

in its wide sense---but perhaps not very profoundly.

Most of his basic work had been completed prior to my meeting him. The last project---a big one---that he completed was that on lacunae, the diffusion of waves in lacunae. Although it had been completed before I arrived on the scene, he continued to mull over it. When he was teaching us the general course on differential equations, and he came to hyperbolic equations, he would tell us something of that project and other things from his serious work. Later on, some of his students obtained significant results and developed his ideas further, but he himself no longer had the time or the inclination to pursue them. He would listen with pleasure, but no longer engaged in serious prolonged thought over several days---even in summer. He spent his summers at his cottage in Abramtsev, where I often visited him. After I had told him about all the latest results that had been obtained, or not obtained, he would suggest going for a walk, and we would set off on a long hike---to Muratovo, where Tyutchev's\footnote{Fyodor Ivanovich Tyutchev (1803-1873), considered the last of Russia's three great romantic poets, after Pushkin and Lermontov.} house is---and the conversation quickly turned from mathematics to new things that he had learned about.

E. B.: Tell me, did you ever touch on socio-political topics?

E. M.: Yes, with the closest of his circle of acquaintance he was completely open.

E. B.: What did he say about such things? I would say that this has all been relegated to history by now.

E. M.: He spoke of the past. He said that he accepts what is as it is. In fact, it would seem that he had resigned himself to this way of thinking already in his youth---after all, he came from the family of a very wealthy merchant.

E. B.: I didn't know that. That fact was not widely advertised, I would say.

E. M.: No, it wasn't. His grandfather was a merchant of the first guild\footnote{Merchants in tsarist Russia were divided into three classes or guilds.}. It seems that he did not have a very high opinion of his son, so put his faith in Ivan Georgievich to continue the business. So he completed studies at a technical school in Sevsk, where it seems the mathematics teacher did indeed influence him. And then came the revolution, and he insisted that they all flee as quickly as possible---and a good thing that they did, otherwise I don't know how they would have ended up. Nobody else from the circle of acquaintance of the family survived. He himself went to Novocherkassk and enrolled in the Polytechnical Institute there.

E. B.: What happened to his father?

E. M.: The whole family went away.

E. B.: But not out of the country?

E. M.: No, not out of Russia. They went down south where they weren't known, and got themselves lost there, so to speak. Later Ivan Georgievich returned to Moscow, and began working as a janitor in an orphanage.

E. B.: This part is better known, it would seem.

E. M.: Yes, that is known. He then married the---the term was then "head"---of the orphanage. And got enrolled in the university. Formally he was a student of Egorov. I must say he started studying mathematics late.

E. B.: Yes, that's well known.

E. M.: As for the rest, that's part of the official biography. It corresponds more or less to what happened in fact.

There is an aphorism due to Kurt Vonnegut, I think: "May God grant me the wisdom and strength to change what I can change, and accept what I cannot change"<sup>1</sup>. That was his motto.

He

told me many times that he had been very often reproached for not "slamming the door", but that if he had "slammed the door", they would just say "Thanks very much", but while---he would say---"in the university

I can't do anything, I can bring my influence to bear, and really influence things; and in this way I'm useful." That was his position, fixed in

his youth, and he maintained it. He kept his distance from everything, but did all that was in his power to do, and was of tremendous use to people.

E. B.: In those cases where there was some definite instability, when a light touch was enough to turn the balance in the right direction, he did it. And sometimes more than a light touch.

E. M.: Yes, he did a great deal of good. In fact, without him, of course, the university would not have become what it did become. He hired a great number of people.

E. B.: For example, Arnold, Kirillov, Berezin, Minlos,....

E. M.: Yes, that was to his merit.

E. B.: And he brought his influence to bear on other faculties. And the talentless party hacks didn't get through so easily---they had in any case to be confirmed by him.

E. M.: He used to say that he could not use the power of his position to hire or refuse to hire directly, but that he could bring his influence to bear in order to arrange things the way he wanted them.

He would sometimes talk, often very humorously, about what it had been like earlier.

E. B.: Earlier in what sense?

E. M.: In mathematics, for example. As a matter of fact, many mathematicians changed a great deal as time went on. In particular, Sergei Lvovich Sobolev. Petrovsky showed me some newspapers he had kept from those times....

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