Étude on Life and Automorphic Forms in the Soviet Union

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This is a short account of my life and work in the Soviet Union. It is an incomplete account, omitting, for example, my contacts with D. Kazhdan, J. Bernstein, and S. I. Gelfand, which were very important for my future work on L-functions. But I hope that my story will be interesting for people who follow the mathematical developments in the USSR. This paper would not have been possible without the efforts and patience of my friends Jay Jorgenson and Tohru Uzawa. I thank them for their efforts to force me to clearly dictate the text and for the patience with which they accepted my seemingly endless changes. I thank my friend Roger Howe for his encouragement, as well as Atle Selberg and Jim Cogdell, who read the entire text and suggested many changes. I also thank my son Gregory, who edited the final version of this text, and many other members of the mathematical community for helpful remarks and encouragement.

I was born in 1929 in Moscow, the capital of the Soviet Union. My parents were both from traditional Jewish families. My father came from Berdichev, a small, heavily Jewish city in the Ukraine, and my mother came from Gomel, another small city with a large Jewish population. Both were from middle-class families, who became poor after the October revolution of 1917.

My father was an engineer who worked in research related to shoe production. His specialty was synthetic soles. He was not very successful, but I believe that he was a good engineer who knew his specialty very well. After World War II, when he was 50 years old, he defended a Ph.D. thesis. He was motivated by the salary increase for the Ph.D. degree, given to workers in research institutes. That increase was instituted by Stalin, who recognized the importance of science when the atomic bomb destroyed Hiroshima. However, the Ph.D. did not help my father very much, since soon after defending his thesis he was demoted and sent to work in a factory where having a Ph.D. degree produced only a little addition to his salary.

My father suffered from his lack of success. He considered that his failure was the result of his not being a member of the Communist Party. When I was in my twenties, he strongly advised me to join the Party. We had many arguments about it. Even now I am sad that I was not able to explain to him why I refused to join the Communist Party. I am happy, however, that I followed my way. My mother did not get a higher education and worked as a typist most of her life. She usually worked at home, on a small typewriter she kept there. I remember how excited she was when she helped type my translation of Siegel's lecture notes. Later, I dictated my thesis to her.

My parents basically forgot, or were afraid to follow Jewish traditions. However, they were not against them. Sometimes at Passover we did have matza. I remember that in 1940 my grandfather, then 80 years old, moved to our apartment, where he stayed till the end of his life. He kept Jewish traditions. He died in our apartment in October of 1941, when Hitler's armies were close to taking Moscow. After emigrating to Israel, I learned about his cousin A. Shenkar, who emigrated with his wife to Israel in the early 1920s. He was a successful businessman in Palestine, and was the first chairman of an organization of businessmen in Palestine. Not having any children, he donated a lot of money to various universities in Israel. I was happy to learn that one of the buildings at Tel Aviv University is named after Shenkar.

I remember myself in 1939, reading newspaper stories about great public trials. It was clear to me that the stories were fabricated, but I was afraid to talk about this, even with my parents. The purges of the 1930s touched my family as well. My uncle, a brother of my father, was one of the leading lawyers in Moscow when he was arrested in 1937 and sent to a labor camp. He died there but we never knew exactly when and how.

On June 22, 1941, Hitler attacked the Soviet Union. Even now I remember Molotov's speech in which he announced that Hitler had moved his army into the Soviet Union without a declaration of war. In 1942, our family was evacuated from Moscow to the town of Kirov, in northern Russia not far from the Ural mountains. The local population was hostile to relocated people. They had good reasons for this, since they had to divide scarce food and housing with the newcomers. The food supplies that were never very good became worse in the wartime. The local children were very anti-Semitic and often picked on me. Fortunately, after one year, we went back to Moscow, where things were a little better. I was too young for the army and so I went to a high school.

It was a difficult time for me. Food was rationed during the war and we never had enough of it. To get better rations, I went to a special technical high school which prepared engineers for the railways. In addition to the usual subjects we studied technical topics such as drafting. Despite better rations, I did not like the school and was not a good student. I was dismissed from the school and sent to work at the railway station. However, my father talked to the director of the school and persuaded him to excuse me from that work in order to continue my education. I went to an evening high school and graduated in 1946.

My interest in mathematics started when I was about ten, with my father showing me the negative numbers. I remember that I was struck by their charm and the feeling of something unusually beautiful. Later, while still in high school, I rediscovered the binomial coefficients. My interest in mathematics was developing mostly at home until the later years of high school, when I started to attend the math seminars for teenagers organized by students of Moscow University. I also took part in several Moscow Mathematical Olympiads, with moderate success. There, the participants were given several hours to solve difficult problems. Whoever could solve the most problems was the winner. I remember that I won one third prize and a couple of honorable mentions. In high school, I also became interested in number theory after learning about Fermat's Last Theorem. I remember that several times I tried to prove it (not quite successfully).

In 1946, after graduating from high school, I entered Moscow University. I was still mainly interested in number theory. Soon I started to attend the number theory seminar organized by A. O. Gelfond, who was famous for his results on transcendental numbers α^{β}. His seminar was attended by his students and ex-students, some of whom were already professors. I remember N. M. Korobov and A. G. Postnikov. At the seminar, I met my friend G. Freiman. He was also a student, two years ahead of me. I also met U. V. Linnik, a friend of Gelfond, and already a professor in Leningrad.

The seminar was devoted to diverse topics in analytic number theory, including additive number theory, the Riemann zeta function and so on. The center of interest in this seminar was the method of trigonometric sums and applications. Occasionally we had talks about transcendental numbers. We had already realized that it was impossible to solve fundamental problems like the Riemann hypothesis using old methods in analytic number theory. We started to explore other topics such as uniform distribution of fractional parts, where progress looked more achievable. Gelfond was very interested in the theory of the zeta function. When he died in 1966, I was present in the hospital. I remember that he was trying to write some formula and tell me something which was clearly related to the zeta function. He could not because he was already paralyzed.

Later on, when I was a third year student, I attended courses of N. K. Bari on problems of trigonometric series. I started to work on one of the problems, the uniqueness problem. A subset X of S^1 is called a set of uniqueness if any trigonometric series which converges outside of X to zero has zero Fourier coefficients. It is easy to prove that any countable set is a set of uniqueness. Later on, Salem proved that the standard Cantor set is a set of uniqueness. He constructed certain of closed sets of cardinality equal to the continuum called H-sets. He conjectured that all sets of uniqueness are H-sets. I found a new construction of sets of uniqueness which are not H-sets. I also found a criterion for a closed set to be a set of uniqueness.

For this result in 1952 I received a Moscow Mathematical Society award for young mathematicians. Let me recall that 1952 was a year of great anti-Semitism in the Soviet Union. So it was a great surprise that I received the award. I shared the prize with Prokhorov, a specialist in probability theory, who is now a member of the Soviet Academy of Sciences.

I graduated from Moscow University in 1951. My advisor at Moscow University was A. O. Gelfond. He was a very warm person, very humane and sensitive to me and to the other students. He was a member of the Communist Party. His father was personally acquainted with Lenin. This was widely known, since Lenin, in his only philosophical book (required reading for all students), criticized the philosophical beliefs of Gelfond's father. When I asked Gelfond about this, he said that his father and Lenin had disagreements in public life, but in private they were friends. Being a member of the Communist Party, Gelfond felt that he had some influence, and recommended that the Moscow University accept me as his graduate student. However, that year was one of great anti-Semitism in Russia. Let me recall that anti-Semitism became very strong in Russia after the end of World War II. It was a strange inheritance that Stalin got from Hitler.

And so, the recommendation from Gelfond did not help me enter the graduate school of Moscow University. I was denied admission by the party committee of the mathematics department. The reason given was that my grade in military training was only "C". I did not take military training seriously and used to play chess during class (something I never did in math classes). In fact, military training was not an important subject and my "C" grade was just an excuse to keep me out of graduate school.

A. O. Gelfond still wanted me to continue with mathematics and he suggested to his friend A. D. Buchstab to try to take me in the Moscow Pedagogical Institute. My friend A. Lavut (later a well-known dissident) made the joke that Moscow University and Moscow Pedagogical Institute are fighting for Piatetski-Shapiro. The University wanted the Pedagogical Institute to take him, and the Pedagogical Institute wanted the University to take him. Since Moscow University was stronger, I entered the graduate school of the Moscow Pedagogical Institute.

At that time, the department of mathematics of the Moscow Pedagogical Institute was very strong. It included the famous logician P. S. Novikov. My advisor A. D. Buchstab was known for his work in sieve methods. He was a very good advisor for me since he let me do what I wanted. At the same time, he helped me very much to enter the graduate school. The main obstacle was the oral examination in Marxism and Leninism. A. D. Buchstab was present at the exam. It was a funny examination. It was well known that for Jews there were only two grades: satisfactory ("C"), and unsatisfactory ("F"). I think that I answered all the of the questions; however, the person conducting the exam refused to give me a grade. He said that only the chairman of the department of Marxism and Leninism could give me a grade, and the chairman was not present for the exam. In the end, I got the highest grade for a Jew which was "C". The director of the Moscow Pedagogical Institute knew about the style of the exam. Sometimes he accepted Jews who scored "C" on that exam, something he never did for non-Jewish students. This was a funny reverse discrimination.

Graduate school was organized differently in the Soviet Union. Everyone accepted received a stipend for three years with no teaching obligation. However, there was no way to extend the stipend beyond three years. The stipend was very small, but our needs as graduate students were very modest.

Accepted with me was my friend Yu. Sorkin, also Jewish. He was working in general algebra, with his advisor A. Dizman. He was also from Moscow University. Together we went through a strange, Kafka-style, adventure during our first year in graduate school. After we were both accepted to the graduate school we both received letters informing us that we were supposed to go to teach in a high school in Karaganda, in the middle of Asia. The letter said that if we did not go, we would be ordered by the courts to go. My parents panicked and advised me to go. They said it would be better to go to Karaganda than to go to a camp. My reaction to the letter was not so strong and my friend Sorkin agreed with me. At that time, after a student graduated from college, he was supposed to be assigned by a special commission to work somewhere. The justification of this was that since the state educated us for free, the state in return could send us to work where it wants. Since both Sorkin and I were recommended but not accepted to the graduate school of Moscow University, we both were given this assignment. In the meantime, we were accepted to the graduate school of the Moscow Pedagogical Institute. We did not sign the agreement to accept the assignment of the commission. However, the decision of the assignment commission was compulsory, and, theoretically, we could have been forced to go by the court. In reality, such cases seldom went to court. Fortunately, Sorkin had a friend in Karaganda who wrote that they did not need us. It was true, the school did not need us. We went through this adventure safely, but it took about one year.

In 1954, after I defended my Ph.D thesis, I went to Kaluga for three years. Kaluga is about 100 miles away from Moscow; by train it took a few hours. The apartment of my parents in Moscow was very close to the train station, so it was very easy to commute to Moscow for weekends.

My work in the theory of automorphic functions started when I. R. Shafarevich suggested to me to translate Siegel's lecture notes from the Institute for Advanced Study on automorphic functions. As far as I remember, I met Shafarevich for the first time in 1949 when I was a student in my third year of undergraduate studies. I attended a course of B. N. Delone on the geometry of numbers. The course included some elementary material such as

Minkowski's Lemma and introduced algebraic numbers as a lattice in multidimensional Euclidean space. He talked about various problems including the famous tower problem of Hilbert. (If you construct a tower of unramified maximal abelian extensions, is it finite or not?) He was trying to say something about this problem using his geometric approach. He said he had a student named Shafarevich, a genius, who felt that this problem could not be solved in such an elementary way. Later on, Shafarevich and E. Golod solved this problem in a more sophisticated way. I do not remember any serious interaction with Shafarevich until later, when I became a graduate student and started to participate in his seminar.

Shafarevich was very interested in Siegel's theory of modular functions. He conducted a seminar to understand Siegel's lecture notes on automorphic forms.[1] He invited me to translate the lecture notes from English to Russian; I was happy to do this since I could earn a small amount of money. I remember that my mother, who was a typist, helped me very much since I dictated the text to her and she typed it right away. The lecture notes of Siegel were published in Russian—their only publication.

I remember the moment when we learned that not every complex torus has an algebraic structure. Shafarevich was very surprised by this and went to discuss it with Gelfand. It was later understood by several mathematicians that in higher dimensions, complex manifolds are not algebraic in general. The general criterion for Kähler manifolds to be algebraic was given by the celebrated theorem of K. Kodaira. By that time, I had started to work on the theory of automorphic functions. The seminar of Shafarevich was very inspiring for me. About that time, I obtained my first results about automorphic functions. I remember going to Shafarevich's apartment and discussing the difficulties I had encountered. That was always very helpful. He had a very good grasp of the general picture and of all technical details. At that time, it was typical to go to the apartment of your professor because in the universities there was not enough space to work.

I remember that our conversations were not restricted to mathematics, and after finishing our mathematical discussions we frequently turned to politics. Shafarevich, a son of a professor, was a well-educated man who knew French and German. Even then, he made it clear that he disliked the October Revolution. Of course, he did not say that explicitly, which would have been dangerous. At that time, during Stalin's rule, no one could dream of being a dissident. However, it was clear to me that Shafarevich had negative feelings for Communism. Of course, he never was a member of the Communist Party. More interestingly, he was against all revolutionary movements in principle. At that time, Dostoevskii was not easily available in Russian,

[1]These lectures were given at the Institute for Advanced Study in Princeton during World War II.

but Shafarevich quoted the very negative depiction of revolution from the famous novel "*Devils*".

Returning to mathematics, the central result of Siegel's book was the so-called theorem of algebraic relations for Siegel modular functions. In my Ph.D. thesis, I proved a generalization of this theorem for the case of Siegel-Hilbert modular functions. Actually, I introduced the terminology "Siegel-Hilbert modular functions" in my thesis, and it was accepted.

Siegel's book also contained explicit descriptions of symmetric domains; at that time, I did not know the general theory of Lie groups. So this description was very important for me. The realization of bounded domains as unbounded domains played a very important role in Siegel's methods. For an important class of bounded symmetric domains, these unbounded domains are called the Siegel half-plane. The general notion of Siegel half-plane can be described as follows. Let V be a convex cone in $\mathbb{R}^n$ which does not contain any line. Consider the following set of points:

$$H = \{x + iy;\ y \in V,\ x, y \in \mathbb{R}^n\} \subset \mathbb{C}^n$$

This is what I called a Siegel domain of the first kind. For example, the Siegel half-plane is given by taking V to be the set of positive definite $n \times n$ real symmetric matrices. The natural problem was to extend this description to other symmetric domains. The natural question was how to do this for other types of symmetric domains. The difficulties of this problem were manifest in the two-dimensional complex ball B^2. The two-dimensional complex ball is given by

$$\{(z_1, z_2);\ |z_1|^2 + |z_2|^2 < 1\}.$$

It is easy to prove that there is no realization of B^2 as a Siegel domain of the first kind. I found a realization in the following form:

$$\{(z, u);\ \Im z - |u|^2 > 0\} \subset \mathbb{C}^2.$$

This example led me to the general definition of a Siegel domain of the second kind. Let $V \subset \mathbb{R}^n$ be a convex cone with no lines inside. Let W be a complex vector space and let F be a map satisfying the following conditions.

(1) $F : W \otimes W \to \mathbb{C}^n$.
(2) F is linear in u and antilinear in v.
(3) $F(u, v) = \overline{F(v, u)}$.
(4) $F(u, u) \in \overline{V}$ and $F(u, u) = 0$ if and only if $u = 0$.

Then the Siegel domain associated to (F, V) is given by

$$H = \{(z, u);\ \Im z - F(u, u) \in V\}.$$

It was proved later in collaboration with S. G. Gindikin and E. Vinberg that any bounded homogeneous domain has a realization as a Siegel domain of the second kind with transitive action of linear transformations. There is a very nice exposition of the story of Siegel domains by S. G. Gindikin [1].

In those days, I was able to check by hand that all of Siegel's examples can be written in this form. Also by guessing, I could find realizations for two other exceptional symmetric domains. However, the most unexpected application was the discovery of nonsymmetric homogeneous domains in $\mathbb{C}^4$. In hindsight, you just have to realize that such examples should exist. It is a simple exercise that you have to take $n > 3$, because you have the first nontrivial cone in dimension 3:

$$y_1 y_2 - y_3^2 > 0,$$
$$y_1 > 0,$$

which is equivalent to

$$\begin{pmatrix} y_1 & y_3 \\ y_3 & y_2 \end{pmatrix} > 0,$$

where $A > 0$ for a symmetric matrix A means that A is positive definite. Then you consider

$$F(u, v) = \begin{pmatrix} u\bar{v} & 0 \\ 0 & 0 \end{pmatrix}.$$

Then the corresponding Siegel domain is

$$\begin{pmatrix} \Im z_1 - |u|^2 & \Im z_3 \\ \Im z_3 & \Im z_2 \end{pmatrix} > 0.$$

I remember that I published the definition of Siegel domain of the second kind a year before I realized that it can be used in order to construct examples. It is an interesting situation: if you knew the definition of Siegel domain of the second kind and knew that this definition led to an example of a nonsymmetric domains, then it would take at most an hour to find an example and the essential idea of the proof for showing that it is not symmetric. I believe that this situation is typical even now; nobody, even the author, reads articles with proper attention. I know other examples of this from my American life as well!

Let me explain how I came to the idea that nonsymmetric homogeneous domains should exist. I knew about this problem, but was not interested in it. I was under the general impression that such domains did not exist, but it would be difficult to prove the nonexistence. However, the study of automorphic forms naturally led me to the study of the geometry of symmetric domains. The following fibration is important.

Let D be a bounded domain. To each boundary component B_i, we construct a fibering by looking at all the geodesics that end in B_i and associating the end point to every point on the geodesic. In a typical situation, the fiber is not a symmetric domain. Thus I learned from the geometry of Siegel domains about the existence of nonsymmetric domains.

This fibering was very important for understanding Satake compactifications. It was also very important for generalizing Satake's construction to arbitrary arithmetic groups. At that time, the very important result of A. Borel and Harish-Chandra on the structure of fundamental domains for arbitrary arithmetic groups appeared. I remember that Shafarevich was invited to attend the International Congress of Mathematicians in Stockholm in 1962. I was invited to this Congress also. As usual, I was refused permission to go. I do not remember the formal explanation, but it was clear that I was refused because I was Jewish. Since I was invited to give a talk, I wrote the talk and asked Shafarevich to read it because I felt he was closer to the work than other people. He was willing to do this.

When Shafarevich returned, he told me that many people attended the presentation of my talk. I was very excited to hear this. Shafarevich passed to me the question of A. Borel whether I could prove the theorem of algebraic relations and the theorem of normal compactification of arbitrary symmetric domains and arithmetic subgroups. It was clear to me that we had the necessary tools: the generalized reduction theory of A. Borel and Harish-Chandra, and the geometry of symmetric domains by myself. I started to work on this topic and soon obtained the results. At about the same time, A. Borel and W. Baily proved the same results. I must confess that their exposition was more thorough than mine; everyone (including me) now calls it the Baily-Borel compactification.

Shafarevich introduced me to automorphic forms, the topic which became the main focus of my work. We shared a strong interest in number theory and later wrote a few papers together. One paper is on the tower of fields of automorphic forms. Let H denote the upper half-plane and let $\Gamma(p^n)$ be the modular group of level p^n. Let

$$X_{p^\infty} = \operatorname{proj}\lim H/\Gamma(p^n).$$

Then $\widetilde{\mathrm{SL}}_2(\mathbb{Q}_p)$ acts on X_{p^∞} considered as a complex manifold, but if you also take into consideration the fact that X_{p^∞} is defined over $\mathbb{Q}(\mu_{p^\infty})$ (you add all the p^n-roots of unity), then $\widetilde{\mathrm{GL}}_2(\mathbb{Q}_p)$ acts on X_{p^∞}. It is clear that this fact lies behind arithmetic applications of automorphic forms. We started to work on it to understand it better. Later on, I found that the Eichler-Shimura congruence relation has a very natural interpretation in this language. In my article [2] I explained the interrelation of the action of the group of adèles and the Eichler-Shimura congruence relation. I remember that V. Drinfeld helped me very much in the preparation of this article. Soon after, V. Drinfeld came to his wonderful discovery on Shtuka; he told me that he was influenced by my article. This was, as far as I remember, my first article to be written first in English and then translated into Russian to obtain permission to be sent abroad. This article was published later in the Proceedings of the International Conference on Modular Forms. I was invited to this conference. But I could not obtain permission of the authorities to participate in this conference. Another paper, quite well known, was on the Torelli theorem for K3 surfaces. We started to work on this topic entirely on Shafarevich's suggestion. Shafarevich was an ideal collaborator; he would look into all technical details. It was possible to discuss everything with him.

When I decided to leave Russia, I went to Shafarevich and told him about it. He was very negative about this. He was not openly anti-Semitic at that time. But, by that time, I had heard many remarks from him that sounded strange to me. For instance, he was very critical of the Jackson Amendment which denied the most favorable status to countries which restricted emigration. He criticized Jackson and the American Congress even more than the Soviet media. He tried to persuade me not to emigrate. Earlier, Shafarevich was tried to persuade B. Moishezon not to emigrate, also to no avail. Shafarevich gave me many different arguments against emigration, the most funny of which was the following. He said that I would never learn how to correctly pronounce the Hebrew letter "ain". He was right. I still cannot pronounce it correctly, but my daughter Shlomit can do it without a problem.

Recently, Shafarevich published an essay on "Russophobia", placing himself on the extreme right of anti-Semitic Russian literature. It was very unpleasant to see a man I respected so much become a leader of anti-Semitism. I am not going to discuss the contents of this book, but I must say that I completely disagree with his basic statement that Jews were responsible for the October revolution and the evil that came from it. It is true that some Jews participated in the October revolution, but it is clear that the October revolution was not a plot of Jews against the Russian people. The Jews, like the other participants, honestly believed that this revolution would bring a good life for all people. I quite understand Shafarevich's worries about the fate of the Russian people, and I agree with him that the October revolution was a catastrophe for the Russian people. But in the history of any people, there are similar or worse catastrophes. I think that if people would critically understand the reason for a catastrophe, then they could overcome it. Trying to make Jews the scapegoats for this catastrophe is very bad for the Russians themselves.

In 1958, at the end of my stay at Kaluga, Gelfand invited me to come to the Institute of Applied Mathematics in Moscow, which at that time was considered secret. Its director was the late M. V. Keldysh, one of the most important figures in Russian science, not only for the position he held (for a long time, the president of the Soviet Academy of Sciences), but also for his theoretical leadership of the Sputnik program. Let me digress for a moment and add some personal stories about Keldysh. In his memoir, A. D. Sakharov mentions that Keldysh was asked by Brezhnev how much time one needed to make Russian science Jew-free (Judenrein) and Keldysh replied that probably 15 to 20 years would be enough. In the same memoir, Sakharov writes that at the institutes Keldysh directed, M. V. Keldysh was not anti-Semitic. I worked at the Institute of Applied Mathematics for about 15 years and I share the same feeling that Keldysh was not anti-Semitic. After I resigned from the Institute in 1974 and applied for an exit-visa, Keldysh, as the director of the Institute, had to write a letter about me stating whether I was involved in classified research. After my application was refused, I asked him to confirm that I was not involved in any classified activity. Keldysh told me that he had already confirmed that. I believed him, but when I told my refusenik friends, they all laughed at my naïvete and one respected refusenik suggested that I tell American journalists that Keldysh was a hypocrite. I refused to follow this suggestion, and in fact, in less than a year I got permission to leave the Soviet Union. I consider this as proof that Keldysh was not lying.

Gelfand was the head of a theoretical department that was not involved in classified research. At that time, there were many mathematical departments involved in classified research, but researchers started to realize that they would be better off by staying away from classified research.

I remember that I had a chance to attend Gelfand's course in 1949, when Gelfand worked together with Naimark on unitary representation theory. About the same time I started to attend the Gelfand Seminar. The Gelfand Seminar was unusual in its breadth of topics covered—there could be talks on representation theory, functional analysis, hydrodynamics, sheaves, etc. It was Gelfand's intention to understand mathematics as a whole; no problem in mathematics was irrelevant to his seminar. The seminar was also flexible in its time schedule. Seminars started at 6 or 7 p.m. on Monday and went on to 10 p.m. or even midnight. One thing was certain: the seminar never started or ended on time.

Gelfand was very active. He would ask many questions and at the end replace the talker and present the talk in much better form. I remember giving a talk myself on representation theory of $\widetilde{\mathrm{GL}}(2, \mathbb{Q}_p)$. This was the very beginning of this work. The notion of smooth representation was not common at that time. Gelfand, together with M. I. Graev, already started to work on the classification of representations of $\widetilde{\mathrm{GL}}(2)$ over p-adic fields, but he only considered unitary representations. However, for a person working in automorphic forms, the natural notion was a smooth representation. My talk was about the Jacquet-Langlands theory of $\widetilde{\mathrm{GL}}(2)$. It covered smooth representations and Kirillov models, etc. Gelfand was not familiar with this theory but he immediately understood the importance of this notion. The point is that he was concerned with the notion of equivalence of representations in infinite-dimensional spaces. If you say that two Banach representations V_1, V_2 of a topological group G are equivalent if there exists a continuous isomorphism of these two spaces which commutes with the action of G, then you are in trouble. This is because representations which are naturally equivalent will not be equivalent under this definition. For example, consider the representation of $\widetilde{\mathrm{SL}}(2, \mathbb{R})$ on the space of holomorphic functions on the upper half plane. One can give two different norms on this space so that the two Banach spaces are not equivalent under the naïve definition. It is clear that the two representations should be called equivalent. Hence Gelfand was interested in the correct notion of equivalence. In the p-adic theory, if one considers smooth representations, then one can use the naïve definition. In the Archimedean case one can also define the notion of smooth representations, which were established much later by W. Casselman and N. Wallach.

Gelfand had a broad interest in representation theory. To him, representation theory is at the center of the whole of mathematics. He was not a specialist of the theory of automorphic forms at the beginning. The first important thing that started our collaboration was the notion of cuspidal automorphic forms. The definition of cuspidal automorphic forms was well known by that time. The definition of cuspidal Maaßwave form was also known; reformulation in terms of representation theory was very easy. When I discussed this notion with Gelfand, he became very enthusiastic. He said many times at that time that it was a very important definition. In the course of our discussions, Gelfand realized the connection between scattering theory and asymptotic properties of Eisenstein series (later on, L. D. Faddeev wrote a paper on this). Gelfand and I proved that the spectrum of cusp forms is discrete. Gelfand many times underlined the importance of this set of irreducible representations of the adèle group which can be realized as cuspidal. He said that the set should play an important role in arithmetic. We also introduced an important operator which is in some sense similar to the S-matrix. Unfortunately, I did not learn enough of scattering theory. At the end of our cooperation, we wrote the book *Representation theory and automorphic functions* with M. I. Graev.

Cooperation with Gelfand was very unusual. Typically, people who cooperate divide any type of work among themselves. But with Gelfand, it was different. One has to appreciate his deep understanding and knowledge of mathematics and wonderful ability to find unexpected relations. For instance, in the case of the theorem of discrete spectrum for cuspidal automorphic forms, Gelfand, from analogy with scattering theory, formulated the theorem, explained to me why it is true, and left me to work out the rest. Later on, cuspidal representations became known to Western mathematicians and they enthusiastically developed the ideas. The theorem of restricted tensor products, which describes representations of $G(\mathbb{A})$, was also first proved in our book. I remember that Gelfand explained to me the problem: how to relate representations of the adèle group $G(\mathbb{A})$ to representations of local groups. We started to work on this problem and soon we found the theorem about restricted tensor products.

I remember that the three of us, Gelfand, M. I. Graev and I, spent one vacation on the river Volga and worked on the book. Graev was also my friend, who started to work in the theoretical department of Gelfand at about the same time. Due to a shortage of office space in the Institute, we had to share our offices. Graev was a quiet person strongly devoted to mathematics, capable of doing a lot of hard mathematics. We owe it to him that the book finally came out. Unfortunately, after I left the Soviet Union, Gelfand and Graev stopped working on automorphic forms. I was very happy to meet

Gelfand very recently in the USA Dr. Klaus Peters suggested that we reprint our book as it is. Gelfand and I agreed immediately with this idea. We decided only to add a new introduction signed by the three of us.

Let me conclude these notes by mentioning my relation with Y. I. Manin. He was younger than me. He was a student of Shafarevich, but soon became completely independent. Our relationship was always warm and friendly. I remember especially the last few months in the Soviet Union, when I attended Manin's Seminar at Moscow State University about gp-adic L-functions. We had a number of mathematical discussions at that time. I remember one of them, where Manin told me that he expected the Mordell conjecture to be solved soon despite the fact that some very important tools were still not available. He said that he would not be surprised if an extremely talented and powerful mathematician came and solved this problem in a few years. Recently, I met Manin in the United States and in France and I found him the same warm, friendly person.

Even though I left the Soviet Union, I do not harbor ill feeling towards it. I recall my years there without regret. There was, and still is, an excellent mathematical school there. I am always happy to meet my friends from Moscow.

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