Ilya Piatetski-Shapiro, In Memoriam

Jim Cogdell, Steve Gelbart, and Peter Sarnak, Coordinating Editors

P-S as he was widely known, even in Russia it seems, was born in Moscow on 30 March 1929. He became a student at Moscow State University at age seventeen. Among his teachers there were N. Bari and A. O. Gelfond; much of his early work in analytic number theory was done while he was at Moscow State University, including the work that resulted in the solution of a problem of Salem on sets of uniqueness for trigonometric series and his work on what is now known as the "Piatetski-Shapiro prime number theorem". He graduated from Moscow State University in 1951. He then became a student at the Moscow Pedagogical Institute, where he received his Candidate of Sciences degree in 1954 under the direction of A. Buchstab. After leaving the Moscow Pedagogical Institute, he spent three years in a teaching position in Kaluga. He then returned to Moscow, taking up the position of "Senior Research Scientist" at the Institute for Applied Mathematics. He defended his Doctor of Sciences degree there soon thereafter.

P-S's interest in automorphic forms dated from his participation in a seminar of I. Shafarevich on Siegel's Institute notes on automorphic forms and Shafarevich's subsequent invitation for P-S to translate Siegel's notes into Russian. While P-S's relationship with Shafarevich was never an official one, it was surely one of great influence. This interaction broadened his mathematical outlook and directed his attention toward automorphic forms and discrete groups, what we might call modern analytic number theory or even "analytic arithmetic". One thing that this led to was his consideration of and solution of a problem of E. Cartan on the existence of non-symmetric bounded homogeneous domains. This was related to the theory of domains of definition for automorphic forms. It led to P-S's first invited ICM address at the 1962 International Congress in Stockholm.

Throughout his career, P-S was also influenced by I. M. Gelfand. In the late 1950s he collaborated with Gelfand on introducing representation theory into the theory of automorphic forms. The influence of this work cannot be overestimated. It brought the powerful analytic techniques of representation theory (nonabelian harmonic analysis) to bear on automorphic forms and related arithmetic questions. It is the lingua franca of the subject as we know it today. His work in this area garnered his second invited ICM lecture, a plenary address, at the 1966 International Congress in Moscow. In this address he also outlined his thoughts on the structure of discrete subgroups of Lie groups, i.e, those groups with respect to which you define automorphic functions. In particular, it contains his belief that, essentially, discrete cofinite subgroups of Lie groups of rank at least two are all arithmetic. This problem was later solved by Margulis.

In the late 1960s and early 1970s, P-S turned to the theory of automorphic L-functions and converse theorems. His first papers in both these areas date from 1971. L-functions are functions similar to the Riemann zeta function attached to automorphic forms that are one incarnation of their arithmetic information. The converse theorem says that these invariants characterize the automorphic form. These topics dominated P-S's thought for the rest of his career, through his long

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collaboration with Jacquet and Shalika. His work in this area garnered him an invitation to give an address on it at the 1978 International Congress in Helsinki, his third ICM address.

Sandwiched in his work on automorphic forms was a period of collaboration with Shafarevich on some questions in algebraic geometry, solving with him the Torelli problem for $K3$ surfaces. In addition, during his years in the Soviet Union he worked on a series of problems in applied mathematics, including development of mathematical models in crystallography, seismology, neurophysiology, morphology, and cell biology.

P-S emigrated from the Soviet Union in 1976. He became a professor at Tel Aviv in that year and at Yale the following year. Here he continued his work on automorphic representations and L-functions, as well as interests in analytic number theory and theta series. His work on the converse theorem and functoriality finally earned him a fourth ICM address, this time at the 2002 ICM in Beijing.

P-S was elected to the Israel Academy of Science in 1978. He received the Israel Prize in Mathematics in 1981 and the Wolf Prize in Mathematics in 1990. [He shared it with De Giorgi. As far as I know, P-S was the first Israeli mathematician to receive the prize.] While he never received similar accolades in the States, he was recognized by all as one of the primary leaders in the field of automorphic functions.

P-S passed away in Tel Aviv on 21 February 2009, just over a month shy of his eightieth birthday.

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I became Ilya's student in 1977. He had just arrived at Yale that fall semester. While I was a student, and for many years after, Ilya held positions at both Yale and Tel Aviv, usually spending the fall semester in New Haven and the spring and summer in Tel Aviv. So, in the beginning, David Soudry and I shared him, David in Tel Aviv and I at Yale, although we both traveled to visit the other's home base during our student days.

I became Ilya's collaborator in 1983. After a talk he gave at the IAS, I pointed out a mistake he had made. Of course, he was essentially correct—his intuition was amazing—but there were a few cases "at the edges" where things failed. His response was "Wonderful, come and we will write the paper together." And when that paper was finished, he said "Good, here is what we need to do now." And so it went for many years. We wrote a series of papers on cases of base change that could be analyzed via theta. We wrote a book on the Kuznetsov trace formula for arbitrary Fuchsian groups of the first kind. We had/have a book in progress on automorphic forms on $GL(n)$.

Beginning in the early 1990s all our efforts were directed toward the converse theorem for $GL(n)$ and applying it to obtain cases of Langlands's functoriality conjecture. In the special year at the IAS in 1999/2000, we were lucky enough in collaboration with Kim and Shahidi to use the theory of L-functions and the converse theorem developed with Ilya to solve large families of Langlands's functoriality conjectures. This had always been a long term goal of Ilya's, and I was glad we could finally obtain it. We finished our last paper on this topic only months before Ilya died. This work did result in a fourth invitation to give an address at the 2002 ICM in Beijing. Ilya did not attend the ICM (they had already been to Luminy and Moscow that summer, and he did not have the energy to travel to Beijing), and I gave the address (it was a joint invitation). In addition, that year at the Institute, with Sarnak we solved the last remaining case of Hilbert's eleventh problem on integral ternary quadratic forms over number fields (sums of three squares, for example). Mathematically, it was a very good year for Ilya.

P-S as a young man in Russia, around 1948–1950.

Ilya was very productive till the end. He first told me about his Parkinson's disease when he visited me in Stillwater not long after I moved there. This would have been in 1988 or 1989. There had been rumors about his health for several years before that. In 1992 he quit traveling to meetings. We were scheduled to attend a meeting at Ohio State in March of that year. I was visiting Yale then. Instead, I went alone and gave both talks. This no-traveling policy did not last long, maybe a year or two. Then I think Ilya decided that whether he traveled or not had no effect on how he felt, and he no longer wanted to be away from the mathematics community. He then started traveling as much as ever, if not more, always accompanied by his wife Edith, without whom his travel would have been impossible. In 1997 he retired from Tel Aviv and became full time at Yale. This was primarily a health decision, I believe. He became emeritus at Yale only a few years before his death but retained the title of senior research scientist.

Even as his health continued to decline, we worked. Often it is was late-night telephone calls by either him or Edith giving some ideas and asking if I wanted to work with him on them. These were then followed by a flurry of phone calls over the next few days, always made by him when he could, but then followed by periods of silence. I visited him last in April of 2008 at Yale, and we spent a couple of days working on a project that resulted from one of these calls. In fact, that project is still

in progress. Working with him at that time was unconventional. He was often frozen, and I would talk and write at the board, with no response from him. But the next day, he would have questions about what I had done. So, even though there was no response on his part while I was talking, he was always listening and thinking.

My last conversation with him was by phone on November 30 of 2008. Again a new idea. And as usual over the last few years, still thinking about automorphic forms and variations on the converse theorem.

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For more on the breadth and depth of P-S's scientific work, one can consult his *Selected Works*, published by the AMS in 2000, which contains a complete bibliography up till that time, and particularly the commentaries on the papers written by many of his colleagues and collaborators. For more information of a personal nature, see his paper "'Etude on life and automorphic forms in the Soviet Union", which can be found in his *Selected Works*, as well as the personal reminiscences "I. I. Piatetski-Shapiro, My Advisor and Friend" by L. N. Vaserstein and "Siegel Domains, etc." by S. Gindikin, both found in the *Festschrift in Honor of I. I. Piatetski-Shapiro on the Occasion of his Sixtieth Birthday*, published by the Weizmann Science Press of Israel in 1990.

Simon Gindikin

Moscow Years

More than fifty years of my life are connected with Ilya: he taught me mathematics, and this in a decisive way determined my mathematical life; the few years of our intense collaboration on complex homogeneous domains were some of the happiest in it. Our connection went much beyond being professional: Ilya was a wonderful and dedicated friend. I remember well how it all began. 1958 was the second year of the seminar on Lie groups, which was organized for undergraduate students by E. B. Dynkin (A. Kirillov and E. Vinberg were among the participants). The first year was solely educational, and then the seminar turned into a research one, in which a decisive role was played by the appearance of the participants of the seminar from earlier years, including F. Berezin and F. Karpelevich. They were already well-established mathematicians. A little later, Ilya appeared, having just come back to Moscow from Kaluga. The more senior participants described their current works, which often were in the early stages, and formulated new problems (at this time, Dynkin himself had already stopped working on Lie groups). Junior participants turned out to be invited into their private mathematical kitchens. We found especially useful their frank discussions, impromptus, willingness to share ideas, complete candor, and mutual trust. All this was important to us not only from the scientific but also the ethical point of view. We found that even comic situations can be instructive. One particular dialogue, which was very popular with our group, occurred after Friedrich Karpelevich found an omission in one of Ilya's proofs. Ilya slowly, after a few moments of silence: "Ah, here one can play on. . . ". Friedrich quickly blocks: "No, one can't!" Ilya after some thought:"Yes, one can! I once already played such a way. . . ". This was the end of the dialogue, and what the subject of the discussion was remained unknown to the rest of the seminar attendees. It's hard to find a better illustration of the complete mutual understanding.

Ilya was especially active and often presented. It took some time for me to get used to his style of speaking—without interrupting intense thinking. This was a very successful period of his mathematical life. He was close to realizing the informal Siegel's program—describe the algebraic structure of fields of automorphic functions on arbitrary Hermitian symmetric spaces (symmetric bounded complex domains). Siegel had done it in the special case of modular functions, which now bear his name. Ilya had worked in this in graduate school (beginning 1952). In the beginning he wanted to understand which modular groups exist besides Siegel's modular group. He began work with the translation in Russian of Siegel's Princeton lectures and under the influence and advice of I. R. Shafarevich. His exceptional Ph.D. thesis was on this subject. Ilya considered both of the known ways to prove that fields of modular functions are algebraic: through the compactification of fundamental domains and through estimates of dimensions of spaces of modular forms. Under both approaches it was necessary to understand certain core concepts of the geometry of symmetric spaces.

It was this that Ilya was concerned with when he first appeared at the seminar. Siegel's recipe for work with automorphic forms was composed of the realization of complex symmetric domains as tube domains, which allowed expansion of forms in Fourier series. This realization was possible not only in the case considered by Siegel but also for several other symmetric domains, and at the beginning, Ilya adapted elements of Siegel's construction to these examples. However, the tube realization does not exist for some symmetric domains, and Ilya's most important discovery is connected with this: he found that all symmetric domains allow a certain remarkable realization (a generalization of tube domains), which Ilya called Siegel's domain of the second kind. Ilya discovered that this realization was perfectly suited for his problems: automorphic forms can be expanded

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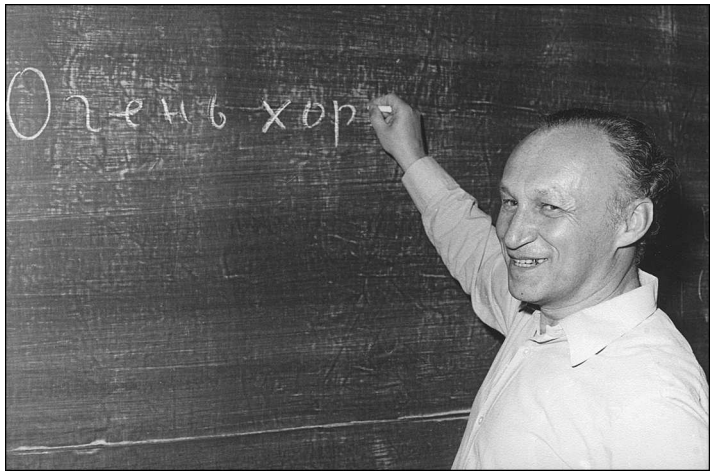
in Fourier-Jacobi series, and this is sufficient for required estimates of the dimensions. This is what Ilya started talking about in Dynkin's seminar. Interaction with other mathematicians was always important to him, and in the seminar's participants he found well-wishing, and often useful, listeners. Soon Erik Vinberg and I were fully engaged in this project. Erik began working on the theory of homogeneous convex cones, which is a central component of the construction of Siegel's domains, and which also are interesting in their own right. I began work on analytic aspects of Siegel's domains: multidimensional special functions and integral formulas.

To realize the second way to prove the algebraic nature of automorphic functions—the compactification of fundamental domains—Ilya was obliged to explore the boundary of symmetric domains and underlying geometry. Here the advice of Karpelevich, whose understanding of the geometry of symmetric spaces was total, proved especially invaluable. The benefit was mutual: these conversations were an important stimulus for the construction of Karpelevich's boundary of Riemannian symmetric spaces. For the compactification the fibering of domains over components of the boundary was very important. Here, a surprise awaited Ilya, which became one of his most remarkable discoveries: already in the simplest of cases the fibers were homogeneous Siegel domains, but not symmetric ones. Such examples existed already in dimension 4, but E. Cartan proved that all bounded homogeneous complex domains in dimensions 2 and 3 are symmetric. There was a consensus among experts that this must be true in any dimension (and this was proven under some restrictions, which seemed temporary). The appearance of Siegel's domains inevitably had to lead to construction of homogeneous, but nonsymmetric, domains, and there are direct, simpler ways to do this than Ilya's first construction; however, initial directions in science often prove indirect. Complex homogeneous domains turned out as a rule to be nonsymmetric. Understanding the geometrical nature of these mysterious manifolds became Ilya's primary pursuit for the next four to five years. This was the longest departure from the primary area of his mathematical life—automorphic functions—which he loyally served for almost sixty years. Progress was very quick. Vinberg and I were more and more involved in the work, and all three of us joined forces for the proof of the central result, that an arbitrary complex bounded homogeneous domain admits a realization as a Siegel's domain of the second kind (published in 1963). With this Ilya fully stopped with his work on complex homogeneous manifolds and returned to automorphic functions.



Tata Institute, January 1979, International Conference on Automorphic Forms, Representation Theory, and Arithmetic.

Today, fifty years later, it must be remarked, with some regret, that though complex homogeneous domains are a class of homogeneous manifolds with a beautiful and a well-understood geometry, their applications turned out to be limited and poorer than those of symmetric domains. Initially, certain interesting explicit formulas were derived, but further results were constrained, as groups of automorphisms, as a rule, are too small. In the nonsymmetric case there are no discrete groups with fundamental domains of finite volume and this constantly worried Ilya. In the last years of his life he hoped that nevertheless there was a possibility of considering interesting discrete groups in the general case, but this must have not been confirmed.

While I was working with Ilya on the joint project, we spent more and more time together and gradually became friends. We easily found a common language, and the difference in ages (eight years) felt nearly imperceptible. Ilya got into hiking later than I, but his habitual focus and drive colored this new pursuit. We were together on several trips in the mountains of Central Asia, difficult for him. Ilya overcame these difficulties just as he overcame all hardships during his life. Many remember with what passion he continued going on hiking trips when he was already seriously ill. Ilya's emigration changed something substantial not only in his life but also in the lives of his friends who remained. These partings, for friends and relatives, had a somewhat funereal quality to them: those left behind did not have a chance to see the departed again. Yet life in Soviet Russia occasionally gifted people with genuine miracles, events which could not happen in a normal life. Stalin died and some of the detainees in his camps, fated to die there, returned home. Soviet power, approaching its death, weakened restrictions on travel to other countries, and people could meet the emigrants,

whom under Soviet rules they should not have ever seen again. In April 1989 I met Ilya in Paris. My first impression was that he had not changed in the intervening thirteen years, but suddenly his condition worsened right before my eyes. It turned out he forgot to take his medicine on time, and I did not yet know anything about his illness. With his help I received a "hanging" visa (not one glued to the passport) to Israel, not apprising the Soviet authorities of my plans: such a trip still appeared too odious, even with the weakening of the restrictions. Attending the conference in honor of Ilya's sixtieth birthday was an indescribable gift.

A half of Ilya's life was connected with Russia in terrible conditions of communistic regime. Fortunately, our time was relatively "tame" (contrasted with the time of our fathers, with Stalin's mass murders and prisons), and it was possible, after accepting certain compromises, to live an interesting life and to preserve human dignity. We were young, and in conditions of total tyranny we found happiness in work, family, friends. Ilya was allotted thirty more happy years of life in Israel and the United States. Mathematics continued to be the center of his life. He was happy with new coauthors, students, colleagues. Once Ilya told me about two extraordinary mathematicians who were close to him, that even though they both produced remarkable results, one of them did not fully realize his potential, and one did so maximally. I understood that Ilya aspired to maximally realize his possibilities, and I'm certain that he managed to do so.

Gregory Margulis

In my part of this article I will call Piatetski-Shapiro Ilya, though I never addressed him this way during his lifetime because of the age difference (usually I called him Ilya Iosiphovich but it sounds awkward to people in the West).

Ilya became a student at Moscow State University in 1946. The Moscow State University was and still is divided into faculties (*facultets*) that are to a large extent independent. Each faculty has its own admission process. Ilya became a student in the faculty of mechanics and mathematics. In 1946 the official anti-Semitism in the Soviet Union was not strong. It became much stronger in subsequent years, starting in 1948–1949 and culminating in the year before Stalin's death in March of 1953. Ilya graduated in 1951, but he was not recommended to the graduate school of Moscow University, undoubtedly for anti-Semitic reasons but with a formal excuse that he got only C in military preparation (*voennaya podgotovka*). Nevertheless (Alexander) Gelfond, who was one of Ilya's advisors during his undergraduate years and who highly respected Ilya, was able to arrange Ilya's admission to the graduate school of Moscow Pedagogical Institute. Ilya was a graduate student there from 1951 to 1954 (three years was the usual time for being a graduate student in the Soviet Union).

Already during his undergraduate years Ilya obtained outstanding results in trigonometric series (solution of a Salem problem) and in analytic number theory (analog of the prime number theorem in sequences $[n^a]$ where $11/12 < a < 1$). For the solution of the Salem problem Ilya was awarded the prize of the Moscow Mathematical Society for young mathematicians in 1952. I also remember being present at a meeting years later in Moscow University, where Ilya was nominated to become a corresponding member of the Soviet Academy of Sciences, and hearing his work on trigonometric series highly praised by Menshov, one of the most distinguished experts in the field.

Soon after becoming a graduate student Ilya switched his mathematical interests to the theory of automorphic forms and related topics, mostly under the influence of Shafarevich. Ilya translated a book by Siegel on automorphic functions in several complex variables. The translation was published in 1954, and Shafarevich was the editor of the translation. Ilya and Shafarevich wrote a joint paper on $K3$ surfaces that has been very influential. Ilya highly respected Shafarevich all his life, even in later years when Shafarevich became involved in well-known controversial activities.

After finishing graduate school in 1954, Ilya was not able to get a job in Moscow, and he had to move to Kaluga which is about 200 kilometers from Moscow. In 1958 Ilya got a position in the Moscow Institute of Applied Mathematics in the department headed by (Israel) Gelfand. That institute was a part of the Soviet Academy of Sciences and at that time was formally considered to be a part of the Steklov Institute, though in reality it was completely independent. Ilya's position in the institute was "senior research scientist" (*starshii nauchnyi sotrudnik*).

There were two advanced scientific degrees in the Soviet Union: candidate of sciences (roughly equivalent to Ph.D.) and doctor of sciences, which was considered to be a much more prestigious degree. Ilya became a candidate of sciences in 1954 and a doctor of sciences in 1959 at the unusually young age of thirty.

After returning to Moscow, Ilya began a longtime collaboration with Gelfand on relations between representation theory and automorphic forms. He published several joint papers with Gelfand on this subject. In 1966 Gelfand, Graev, and Ilya published a book, *Representation Theory and Automorphic Functions*. Ilya collaborated with Gelfand also on various topics of applied mathematics and on

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mathematical methods in biology. Ilya revered Gelfand and highly valued his opinions.

By the late 1950s and early 1960s Ilya was internationally recognized as one of the top mathematicians in the Soviet Union. He was invited in 1962 to speak at the International Congress of Mathematicians (ICM) in Stockholm, but he was not able to get permission to attend the congress, and his invited address was presented by Shafarevich. Later Ilya was an invited speaker at ICMs three more times: in Moscow in 1966 (plenary talk), in Helsinki in 1978, and in Beijing in 2002 (jointly with Cogdell). Thus Ilya was an invited speaker at ICMs four times, which is quite exceptional (there are only two or three other mathematicians in the world who were invited to speak at ICMs that many times).

Ilya's formal position in the Soviet Union was quite good. Besides his position at the Moscow Institute of Applied Mathematics of the Academy of Sciences, in 1965 he was appointed a professor at Moscow State University. (This was a half-time paid position that had to be renewed every year, and it was not renewed in 1968 after Ilya signed a letter in the defense of Esenin-Volpin.) Approximately at the same time he became the (unpaid) head of the mathematical group in Gelfand's laboratory of mathematical methods in biology, which was a part of the Moscow State University. Nevertheless his recognition in the Soviet Union, both formal and informal, did not match his high status in the international mathematical community. I believe that this was one of the main reasons that Ilya decided to emigrate. Another significant reason was difficulties in communication with foreign mathematicians. I remember that, in 1990, when Ilya gave an acceptance speech at the Wolf Prize ceremony, which according to the rules should be no more than three minutes long, he specifically addressed the issue of communication.

As far as I remember, before 1967, I met Ilya only occasionally and do not have recollections of talking to him. But in 1967, after Kazhdan and I proved Selberg's conjecture on the existence of unipotent elements in noncocompact lattices, I started to work on the problem of arithmeticity of discrete subgroups of Lie groups, due in large part to the encouragement from Ilya. This problem was publicized by Ilya beginning in the late 1950s, and a substantial part of his plenary talk at the Moscow congress in 1966 was related to it. After 1967 I met and talked to Ilya regularly until his emigration in 1976. Eventually I was able to prove the arithmeticity of irreducible lattices in higher rank groups. It should be noted that, in the case of noncocompact lattices, the strategy of the proof of arithmeticity had been envisioned by Ilya (and Selberg). In particular, he envisioned the importance of the study of unipotent elements in a noncocompact lattice and of a statement about nondivergence of orbits of unipotent flows in the space of lattices. It has turned out that various generalizations of the latter statement played an important role in applications of homogeneous dynamics to number theory.

After Ilya's emigration in 1976, I received occasional telephone calls from him, but we did not meet again in person until 1987 at the Selberg seventieth birthday conference in Oslo. At that time he was very active, but one could notice something strange in his gait and coordination of movements. We met next in 1990 when I visited Israel for the first time. As mentioned above, in 1990 Ilya received the Wolf Prize, shared with De Giorgi, and he delivered a very nice acceptance speech. During my 1990 visit to Israel Ilya took me to various places in Israel and, in particular, in Tel Aviv. His physical condition in 1990 was noticeably worse than in 1987, and at some point he asked me if I knew what disease he had. After I answered "yes," Ilya said that he had had that disease for eleven years.

In 1991 I became Ilya's colleague at Yale. He continued to have graduate students until 1996–1997. But even after that Ilya continued to teach and to do mathematical research despite his deteriorating physical condition. He had an NSF grant until his retirement in 2004 and even for a couple of years after that. His career at Yale was certainly amazing, and it demonstrated Ilya's extraordinary determination and will.

Hervé Jacquet

Piatetski-Shapiro was one of the nicest mathematicians that I ever met. He was easy to talk to, modest, open to new ideas or new points of view, and always supportive. It was a pleasure to discuss mathematics with him. I appreciated his philosophy of mathematics. Once, when we were discussing the interest of some new developments, he said to me: *Well, mathematicians are happy when they have something to do.*

My first contact with Piatetski-Shapiro was a charming note he sent me after the publication of *Zeta Functions of Simple Algebras*, my book with Roger Godement ([GJ72]): *Could you send me a copy of your marvelous book?* Piatetski-Shapiro told me later that he thought from the very beginning that the work would generalize to all classical groups. Indeed, it does (doubling method). In contrast, after the publication of the book, Harish-Chandra told me that he could not see why I bothered, because the work applied only to the general linear group. My second contact with him was somewhat indirect. After the work of Gelfand and Kazdhan ([GK75]) and Piatetski-Shapiro (pp.

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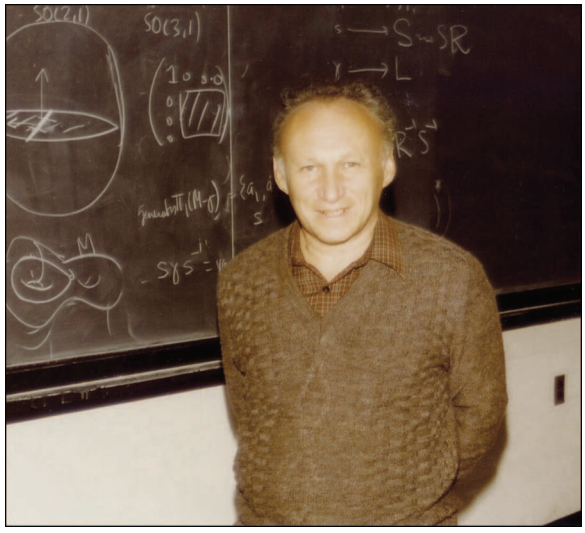

P-S at Yale University around 1979.

373–396 in [PS00]), completed by the work of Shalika ([Sha74]) on the uniqueness of the Whittaker model and the Whittaker expansion of a cusp form for $GL(n)$, it was clear that one could expect a theory of the L-function $L(s, \pi \times \pi')$, attached to a pair (π, π'), where π is a cuspidal automorphic representation of $GL(n)$ and π' a cuspidal automorphic representation of $GL(n-1)$. The L-function would be entire with a simple functional equation. To obtain a converse theorem, one would need to consider, more generally, pairs (π, π'), where π is a cuspidal automorphic representation of $GL(n)$ and π' a cuspidal automorphic representation of $GL(m)$, with arbitrary integers (n, m), and again attach to such a pair an L-function with functional equation. There was first of all a local problem, namely, to associate to a pair (π, π'), where π is an irreducible local representation of $GL(n)$ and π' an irreducible local representation of $GL(m)$, an L-factor and an ϵ-factor. The main point was to determine how these factors behave with respect to parabolic induction. Shalika and I embarked on such a project for $n = 3$, considering the equations for the pairs $(3,2), (3,3), (3,1)$. We asked ourselves if the functional equation $(3,1)$ could lead to a converse theorem but had no idea how to prove it. One day I received a phone call from Mark Novodvorsky that Piatetski-Shapiro had proved, at least in principle, the converse theorem for $GL(3)$ using only the functional equation $(3,1)$ (I also received a letter forwarded by Novodvorsky and dated June 18, 1975). This was very surprising to us. I remember a long telephone conversation with Shalika on the subject. We managed to understand the proof, but I would like to stress that this is one of the most original ideas of Piatetski-Shapiro. Later, we proposed, and he generously agreed, to write a joint paper on the converse theorem for $GL(3)$ ([JPSS79a], [JPSS79b]).

I finally met Piatetski-Shapiro after he emigrated from the Soviet Union. I was taken by his unassuming and gentle demeanor. The three of us embarked on a joint project to write up systematically what was needed to obtain a converse theorem for $GL(n)$. This was a somewhat tedious enterprise, especially the Archimedean theory. Moreover, the mathematical community was not very supportive because the project deviated from the accepted canon and was regarded as perverse. I once mentioned in front of Armand Borel that I would write up the Archimedean theory and send it to the *Canadian Journal of Mathematics*. Borel immediately declared: *Poor guys! They were trying to improve the quality of their journal*. I found this so discouraging that I set aside the project. Later, I coauthored with Shalika a first version of the Archimedean theory ([JS90]). More recently I wrote another, more detailed, version ([J09]). Separately, Piatetski-Shapiro and Cogdell worked on the global converse theorem. Again, this work contains wonderful ideas, for example, the brilliant treatment of the places where the datum is ramified.

Piatetski-Shapiro also encouraged me to reprove results of Waldspurger on toric period integrals in terms of a relative trace formula.

We will all miss his wisdom and enthusiasm for mathematics.

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Steve Gelbart

Recollections of Ilya

To me, Ilya was not just a friend; he was a hero, personally as well as mathematically.

I shall perhaps comment more on his personality, leaving a detailed discussion of his mathematical skills to others.

I first met Ilya in June 1976 in Bonn, Germany. But my personal memories of him go back still further.

In graduate school at Princeton, I remember the day when the Russian book by Gelfand, Graev, and Piatetski-Shapiro appeared in English translation in 1969. I don't remember if I had heard of Ilya before then, but I certainly did thereafter. In the old Fine Hall Common Room, my advisor Eli Stein sat down to look at the preface of that book, with me as a pair of attentive ears. Stein had just a few years earlier found counterexamples to the supposed complete list of irreducible unitary representations of $SL(N,\mathbb{C})$ by Gelfand and Naimark, and I had just finished some of the relevant computations that went on in the case of "real" harmonic analysis. Gelfand's name I knew since undergraduate days, but Piatetski's joint work on representations of adele groups immediately piqued my interest. What Ilya (and Gelfand and Graev) reported on was the separation of the continuous spectrum from the discrete one, part of the program developed afterward by Langlands. From that day on, I began working in automorphic forms as opposed to harmonic analysis, guided by that book and by the running commentary of Assistant Professor Joe Shalika on the unraveling tales of the birth of Jacquet-Langlands.

From 1970 through 1975, the spectacular results of Langlands in the West were intermingled with those of Piatetski in the East. Ilya, however, was also a victim of the anti-dissident forces of the Soviet regime and was dismissed from one of his jobs. In 1974 he applied for permission to emigrate to Israel and was refused and dismissed from all work; he became one of Russia's famed "refuseniks". During this time, Ilya started a collaboration with Jacquet and Shalika when he sent them his famous converse theorem for $GL(3)$. At the same time, several of us mathematicians were asked to write Ilya. The hope was that this would demonstrate to the Soviet authorities what an important international figure he was and thus would help protect him. I did write, and much to my surprise, Ilya answered, thereby starting a correspondence of letters telling each other exactly what we were working on. In particular, he was most interested in my metaplectic work and what it could do for the symmetric square. By February 1976, when he was finally able to leave the Soviet Union, I had teamed up with Jacquet, and we were able to prove our theorem of cusp forms on $GL(3)$ coming from $GL(2)$ using Piatetski's converse theorem. He and I corresponded several times after he came to America as a visiting professor, and finally I was able to meet him in Bonn's summer on automorphic forms.

Our meeting was memorable. We shook hands, and Ilya did not let go until we had exchanged several sentences that began a joint collaboration that would last more than ten years and a friendship that would last for life.

I often think of that meeting as saying a lot about Ilya. Although he had begun countless collaborations, many with the greatest mathematicians of our time (Gelfand, Shafarevich, Gromov, etc.), each word spoken by him to me made it seem that he was thinking 100 percent only of my work with him. Later, each time he found a solution to a nagging problem, he would announce it in a way that made it seem that "my" ideas led to the way out! There was never any "I", just "we". His mind poured out ideas at a phenomenal rate. If one idea didn't work, another would appear moments later. All in all, we began with more papers on the metaplectic group, then the unitary group, and finally many classical reductive groups.

In 1977 I received a Sloan Fellowship and decided to use it to spend a half year in Israel with my wife Mary and our two young children. Part of the reason for going to Israel was personal, but Ilya's presence in Israel was also a strong factor. Ilya was on the faculty of Tel Aviv University as well as at Yale. He knew I was interested in Israel and persuaded me to give a course on $SL(2)$ at Tel Aviv and to teach a seminar at Hebrew University as well. The result was an intense and wonderful semester in Israel during the spring of 1978. After our return to Ithaca later that year, we remained intrigued by Israel and began planning a full year's sabbatical in Jerusalem, which we took in 1981–82. During that stay I applied for regular positions in Israel at three different places, and we made our permanent move to Israel in 1983. Looking back, Ilya made it all possible. We could never have contemplated such a serious move without him.

In the fall of 1978, Ilya returned to Yale, and I visited him there right away. At the end of a day of calculations, Ilya insisted that I come over to his apartment for dinner. When we arrived, a

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Piatetski-Shapiro shaking hands with Prime Minister Shamir at the 1990 Awarding of the Wolf Prize at Israel's Knesset.

smiling woman opened the door and said, "Hi, I'm Edith and you must be Steve." This was my first introduction to the wife who became an enduring source of love and support to Ilya and a good friend to Mary and me.

That year I also got to meet Gregory, Ilya's son, and Vera, Edith's daughter. In 1979 Ilya and Edith's daughter Shelly was born, just less than a year before my youngest son Dan. Ilya visited me in Ithaca after being somewhere else and got off the airplane carrying a baby bigwheel tricycle to deliver to New Haven! Ilya enjoyed his three children immensely; they all grew up to be wonderful human beings, devoted to and proud of their father.

Incidentally, Ilya may have had the early signs of Parkinson's then, but his activities were those of a vigorous man doing everything possible. In 1979 I remember him stopping from mathematics long enough to do—in his office—a set of thirty push-ups! Also in that year he went to the Tata Institute's Program on Automorphic Forms and had a great time (see picture).

Let me comment more on Ilya's "western" collaborations from the 1970s until his passing away. One of his earlier "pipe dreams" was to find a workable converse theorem for $GL(n)$. From the earlier days of the $GL(3)$ success, it "subsumed" him—how to state it and have it be the tool in (generic) lifting of classical forms to $GL(n)$: that "theorem" lay always at the back of his mind. Of course, his dream was fulfilled with the many years of collaboration with Jim Cogdell (see Cogdell's contribution to this article). Since David Ginzburg, Steve Rallis, David Soudry, and I had failed in our collaboration with Ilya to reach that height, Ilya's thanking us for our efforts in the 1990 and 1994 papers was especially kind. The functorial lifting of generic cusp forms for classical groups was taken up in 1999; by teaming up with Shahidi and Kim, it was finished through the marriage of the converse theorem with new methods from Langlands-Shahidi theory (see Shahidi's contribution to this article).

The second characteristic of Ilya's later work was that—in addition to being driven by personal questions, or maybe because of them—he was happy to team up with other people. He especially liked working with people whose strengths were different from his. In the last thirty-five years, his list of publications includes almost all of the people working on various sides of automorphic representations. I think that those people, several of whom are in the group photo from Luminy in 1999, were proud to have had the chance to work with him.

In the last ten years, Ilya's speech and motion became more and more restricted. Any other mathematician would have given up completely. In addition to tenacity, Ilya had a loving wife who was completely devoted to him and understood his needs. Edith, we thank you many times for sharing him with us.

I think often of the people who went through what Ilya did in World War II and the anti-Semitic years following. It is difficult to imagine anything positive coming out of their struggle. But Ilya had exactly the opposite point of view. His smile and even temper is what greeted any news. If he had bad things to say about a person or thing, he usually kept them to himself. And about the good people, he was never too shy to promote them or to influence them for the better.

In the month before he died, a difficult stay at a hospital in Israel actually allowed Ilya to occasionally speak a few words. Sitting with his good friend Grisha Freiman, Ilya mentioned my name, saying he would like to see me.

Soon after that, we were able to meet at the Weizmann Institute home of my colleague Vladimir Berkovich and his wife Lena, who is Grisha Freiman's daughter. Vera brought Ilya, Edith, and Edith's mother to a luncheon at the Berkowitz's to which Mary and I were invited. This very pleasant gathering also included Grisha and Nina Freiman and Alexei Panchishkin and his wife Marina.

After lunch on the Berkowitz's patio, I spoke a lot about Ilya's mathematics, and for a long time Ilya said nothing. As I said goodbye and was at the door leaving, Edith came running after me saying Ilya now could say something. I went back inside to him and he actually—with difficulty—said "Steve, next time I will speak mathematics."

Ilya affected me in a way that no other mathematician could. He will continue to "speak mathematics" to me because my life as a mathematician

was spanned by his, and his memories will last inside me forever.

George D. Mostow

During World War II the quadrennial Congresses of the International Mathematical Union (IMU) had to be suspended, and they were not resumed until the 1950 Congress in Cambridge, Massachusetts, USA. Because of the enormous mutual interest of the western and USSR mathematicians in each other's achievements, there was disappointment that all the Soviet invitees did not attend. After several congresses in the ensuing decades, it became clear, especially in the 1960s, that the USSR National Committee on Mathematics would not allow Jews to accept IMU invitations. It was occasionally possible for American mathematicians to visit the USSR, but visits in the opposite direction were rare.

Travel restrictions from the USSR to communist countries were less severe. This made it possible for K. Malyusz, a Hungarian student of Gelfand, to arrange a conference in Budapest in 1970. The conference was designed to bring together, for the first time, leading mathematicians from the USSR with their counterparts from the West. Such meetings had not occurred since World War II. The USSR mathematicians were euphoric in Budapest. Not only were the scientific interactions stimulating, but all of the "capitalist" amenities were available. The hotels sold the *International Herald Tribune*, stores sold food not seen in Russia, and there were no lines at any of the stores.

I first met Ilya at this conference. One evening, Ilya invited as many of his countrymen and westerners as could fit into his small room. We shared the bottle of amber-colored old Wodka that he had brought from Moscow. I still get a high when I recall the conviviality of that get-together—a far cry from the discriminatory anti-Jewish policies of the USSR National Committee for Mathematics at that time.

During the early 1970s a growing number of Soviet Jews were permitted to emigrate to Israel. The anti-Jewish behavior in the Soviet Union, however, was not enough to make Ilya want to leave his country. What shook him to the core was the difficulty of maintaining a Jewish identity and the enforced conformity to communism around him in the scientific community. He didn't wish this future for his son, sixteen at the time. Ilya's professorship at Moscow State University was not renewed in 1968 when he signed a letter in support of the dissident mathematician Esenin-Volpin. In 1974 Ilya applied for an exit visa to Israel, and as a result he lost his research position at the Moscow Institute of Applied Mathematics. Authorities also refused to grant Ilya an exit visa, claiming that he was too valuable a scientist to be allowed to leave. He continued his researches nevertheless.

Deprived of income, his plight attracted much attention in the United States and Europe. In 1976 a presentation was made to the Council of the U.S. National Academy of Sciences urging the use of their good offices to get Ilya an exit visa. Later that year, Ilya obtained an exit visa for emigration to Israel. He was welcomed warmly upon arrival in Israel and accepted a professorship at Tel Aviv University. In subsequent years, he visited colleagues all over the world who had signed petitions and fought for his freedom.

On Ilya's first visit to Yale, in 1976, he was given accommodations at Connecticut Hall, the oldest building on the old campus, which is used for distinguished visitors. As Ilya was leaving his room on the first morning, so was the visitor in the next room. As they were both heading for breakfast, they fell into step together. The visitor said to Ilya, "I am a professor of English from Oxford." Ilya responded, "I am a professor of mathematics from the Soviet Union—from Moscow." "Oh," said his companion, "I also am a Communist!" Whereupon, Ilya asked pointedly, "So vot means Communism?"

Starting in 1977 Ilya divided his time between Tel Aviv University and Yale, directing doctoral dissertations in both places.

One of his major works at Yale dealt with the "converse theorem", which establishes a key link between automorphic forms on n by n matrix groups and zeta functions.

For $n = 1$ this theorem is classical. The assertion for $n = 2$ was proved by Andre Weil, and the unexpected and novel version for $n = 3$ was conceived by Piatetski-Shapiro while he was still a refusenik in the Soviet Union. It took another twenty-five years and works with other collaborators, in particular his student James Cogdell, before the suitably flexible and powerful general case was completed. The converse theorem has played a crucial role in many of the most striking results known toward the "principle of functoriality" of Langlands, which Sarnak calls the "holy grail of modern number theory".

When translations of Shafarevich's "Russophobia" began circulation in the United States, a diatribe asserting the harmful role of Jews in the Soviet Union, Ilya discussed with me the desirability of inviting Shafarevich to give a series of lectures at Yale. This would give Ilya an opportunity to show Shafarevich that he was mistaken in his ideas. I think that Ilya was motivated less by naivete than by loyalty to his friend, whom he wanted to rescue from disgrace.

Ilya took friendship seriously, whether with student, colleague, or mentor. His concept of friendship was wholehearted and generous, and meant saying "yes" to any request from a friend.

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Conversely, when Ilya made a request of a friend, he expected the answer to be yes.

About ten years after Ilya joined our department, one could discern a change in his facial expression and in his gait. No one spoke about his symptoms. One day Ilya asked me abruptly, "Do you know what disease I have?" I replied, "Parkinson's." Then he said, "I have now asked three friends what I have and all three said 'Parkinson's', so I am not going to keep it a secret any longer."

The changes brought on by his disease came slowly but steadily. When he needed to attend a seminar at Leet Oliver Hall, his wife Edith would move him up the stairs by grasping his right arm tightly, placing his right foot onto her left foot, and lifting him to the next step.

Despite these crippling handicaps, Ilya continued to teach his classes with the help of his former student and collaborator, Jim Cogdell, and with a junior faculty member when Cogdell was not visiting. There was a referendum of students in his course on whether to remain in Ilya's class or transfer to another class. Overwhelmingly, the students elected to stay in Ilya's class. Many volunteered the comment, "we enjoy having contact with a great mind."

When we could not understand Ilya's barely audible speech nor read his writing, to our amazement, he still was able to collaborate with Jim Cogdell, both in person and by phone, making progress on their ongoing research projects.

With his last bit of strength, Ilya attended mathematical conferences and seminars almost up to the day of his death. Such persistence was possible only, in Edith's words, because "mathematics is his life."

Remembering his life will never cease to inspire us.

Roger Howe

Walking with Ilya

It is not simple for me to express properly my affection and admiration for Ilya Piatetski-Shapiro. It was easy to salute his mathematical vision and the tenacity with which he followed it, but I also felt an exceptional affection for him as a person. He did not push himself forward and he did not talk a lot; but when he did, I was pretty sure I wanted to listen. And I felt a rare freedom in talking with him. He had a talent for connecting deeply with people.

We had a lot of time to talk because we went on many walks together. When he was in New Haven in the 1980s, we would pretty religiously go for a hike on Saturdays, often in Sleeping Giant Park a few miles north of New Haven and on West Rock Ridge, which formed the western boundary of the New Haven area, and further afield on Mount Higby and many of the other basalt ridges that provide texture for the Connecticut landscape. Sometimes his wife Edith, daughter Shelly, and her brother Gregory or sister Vera, when they were in New Haven, would join us. Jim Cogdell and Ilya's other students, and his many colleagues from Russia, would come along if they were in New Haven for a visit. Often we would start in the morning, enjoy a "small lunch" prepared by Edith, and stay out until mid- or even late afternoon. We walked in deep winter when it was hard to keep hands warm even inside heavy gloves, in spring with streams rushing with melting snow and dogtooth violets pushing up, and in fall when the sunlight turned the woods into a 3D stained glass window; less in summer, because Ilya spent summers in Israel. A couple of years, I visited him there, and he organized walks then too, under the summer sun, in the Jerusalem hills and in the wadis going down to the Dead Sea, above Ein Gedi, and hiking down to the St. George's Monastery in Wadi Qilt, usually not just the two of us, but some group of mathematicians and friends.

In the fall a frequent destination was West Woods in Guilford. This is a maze of trails traversing ridges separated by bogs. The crisscrossing trails can be quite confusing, but if you know your way around, as eventually we did, you can find a path that leads to a vista of a "hidden lake", an estuary that was home to many water birds and that provided a scenic backdrop for our small lunch. The first time Edith visited New Haven, in the late 1970s, several colleagues took Ilya and Edith to West Woods. Edith brought with her the Russian love of mushrooms and was excited to find a treasure trove of them growing among the rocks and fallen trees. She made a large collection and took them home to make soup. The rest of the group shared the typical American ignorance and distrust of mushrooms and worried that Edith was courting danger. The wife of one colleague called the next morning to check that Edith and Ilya were still alive.

In the late 1980s Ilya's daughter Shelly was of an age when she could walk well and was still willing to tolerate adults, and she often came along on our walks. Several times during trips to Guilford in the fall, Shelly and our children would come along, and we would pick apples off the trees at Bishop's Orchards.

What did we talk about? Of course, a lot of it was mathematics. Our paper [1] giving a counterexample to the generalized Ramanujan conjecture was outlined during walks in Corvallis, Oregon, during the AMS Summer Research Symposium in 1977. What might make a reasonable substitute for the Ramanujan conjecture was a topic that

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recurred many times. Over a period of years, Ilya sketched preliminary outlines of his program for establishing liftings for automorphic forms on classical groups using converse theorems. In the early 1990s, he invited me to deliver a set of Schur lectures in Tel Aviv, and we discussed my plans for them.

On the other hand, the paper [2] was worked out not in the great outdoors but in the confines of Ilya's office at the University of Tel Aviv, with shades drawn to shut out the brilliant Israeli summer sun. In the summer of 1980 I commuted there three days a week from Rehovot, where we were staying as guests of the Weizmann Institute. After a transfer at the Jerusalem main bus station, I arrived in midmorning. We talked until lunch, went out briefly to eat, then returned and continued talking until it was time for me to return to Rehovot. Our discussions revolved around Waldspurger's recent reworking of the Shimura correspondence, which Ilya was studying. Our paper was an attempt to see how it might look in another context, in particular, on Sp_4. Ilya's interest in special families of automorphic forms, especially on Sp_4, continued, and they were the subjects of several more papers [3], [4].

When nonmathematicians were along, topics would be more varied. Shelly liked puzzles, both solving them and posing them. Here is one of hers that stumped me:

In a room there are three light bulbs. They are controlled by three switches, located in the hall outside the room. How can you determine which switch controls which bulb, if you can only enter the room once?

Politics was not a favorable topic. Like many Russian emigrés in the 1980s, Ilya admired Ronald Reagan for his strong anti-Soviet stance, an enthusiasm I had trouble sharing.

For nearly as long as I knew him, Ilya suffered from Parkinson's disease. He first came to Yale in the late 1970s. The first symptoms of the condition—difficulties in writing on the blackboard, etc.—became noticeable in the early 1980s. Ilya's case progressed slowly, perhaps because he resisted it with the same determination that he brought to doing mathematics. Among other things, our walks were exercise therapy. Ilya was able to fight off the progressive disability and continue to walk into the 1990s, but his steps gradually became less certain, more and more labored. Our walks covered less and less ground. Eventually, he had to stop. However, whatever his physical condition, he continued to do magnificent mathematics. Some of his most outstanding achievements came in his seventies, after he had retired.

I regretted that I saw less of Ilya after we stopped our walks. I got busier, became chair, got involved in mathematics education. However, during the 1990s, we kept talking, and we shared a student, Ju-Lee Kim, of whom I am sure Ilya was proud [5]. I know I am one among many who feel that having Ilya as colleague and friend has made life richer.

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David Soudry

My Teacher

In the summer of 1977, when I finished my masters studies at Tel Aviv University, I met one day with Shmuel Rosset, from whom I took a course in algebraic K-theory. I wanted his advice about the possibility of carrying my Ph.D. studies abroad. He told me, with great excitement, that the great Professor Piatetski-Shapiro was allowed to leave the Soviet Union, after being a refusenik for several years, and now he is coming to Tel Aviv University. Shmuel suggested that I study with Piatetski-Shapiro and said that this would be a great opportunity for me, and, of course, this also meant that I would not have to leave my country. I had never heard of Piatetski-Shapiro before, and his long name terrified me a little. He was scheduled to arrive at Tel Aviv at the beginning of January 1978. In order to prepare for his arrival, Vitali Milman gave a beautiful introductory course on group representations. The idea was to prepare a group of students who could attend a course by Piatetski-Shapiro. At the same time, a group of people in Tel Aviv University ran a seminar on Jacquet-Langlands theory. There was a great commotion over Piatetski's arrival. I first saw him in the last week of that semester. He entered the class accompanied by Vitali and took over. By that time we had already learned the first seven chapters of Serre's book. Piatetski-Shapiro spoke about local fields and focused on the nonarchimedean ones; he described the additive characters as well as the multiplicative characters,

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1999 Luminy Conference in France. Left to right: Freydoon Shahidi, Herve Jacquet, Erez Lapid, David Soudry, Steve Gelbart, P-S, Jim Cogdell, Steve Rallis, Sol Freidberg, and Zhengyu Mao.

and, finally showed us the local gamma factor of a multiplicative character χ,

$$\int \chi(x)\psi^{-1}(x)d^*x.$$

He lectured in a shaky English, with a heavy Russian accent, and he looked to us, young Israeli students, like a professor from the old European tradition.

A couple of weeks later I knocked on Piatetski's door. Two other people, unknown to me, were there talking to him in an undertone. I stood at the doorstep, feeling that I was interrupting, but he invited me in with a gesture of his hand. I introduced myself and expressed my interest in studying with him. He replied that he wanted to test me first. He wrote on the board

$$Ind_B^{GL_2(\mathbb{F}_q)}\mu_1 \otimes \mu_2,$$

the representation of $GL_2(\mathbb{F}_q)$, induced from a character of the standard Borel subgroup, and asked me to determine when it is irreducible and to describe its decomposition when it is reducible. "Come next week with the solution," he said. The following week, I came with the solution. He told me to go to the board and explain the answer in detail. While I stood there, lecturing to him, I noted, for the first time, his deep, blue, penetrating eyes and the strong lines of his face. Before my little lecture was over, he sprang up from his sofa and told me, with a smile, that I passed.

From that moment I became his student. He initiated me into representation theory of reductive p-adic groups and Lie groups, through the similar theory, where the local field is replaced by a finite field. Within finite groups (preferably a low-rank classical group) he could explain the essence of an idea and get directly to the heart of the matter. Oftentimes he treated issues of convergence, irreducible subquotients versus irreducible summands, eigenfunctionals (smooth, nonsmooth) versus eigenvectors, etc. as mere "technical complications". I appreciated this way of thinking when, in the following semester, he gave a course on the representations of the finite group $GL_2(\mathbb{F}_q)$, explaining the notions of cuspidal representations, Whittaker models, Bessel functions, and gamma functions, while I read, in parallel, the book of Jacquet-Langlands. The lecture notes of that course were taken by Moshe Jarden and can be found in Contemporary Mathematics, Vol. 16, AMS, 1983. Now I do the same and train my students to think first of finite field analogs.

Soon I started calling Piatetski-Shapiro by his first name, Ilya. Although not a man of many words, he conveyed his ideas and his intuition in a very clear way; he would explain a topic that he wanted me to learn about, or think of, and, sometimes, it could be related to a problem that he was thinking of at that time. The next time we met he would welcome me by "do you have progress?" (in his Hebrew). Then, whatever my answer was, he would tell me to go to the blackboard, write (say the integral), and start the discussion from the beginning. This could ramify to other channels of thought, and many times it would come with a bonus of a new fact or a new theorem that Ilya would explain to me. Representations of p-adic groups and automorphic forms grew a little more familiar as time went by.

Ilya immediately became a center of rich mathematical activity. Many famous mathematicians would come and visit him, give lectures, seminars, and even courses; as a student, I remember visits by J. Bernstein, A. Borel, P. Deligne, S. Gelbart, R. Howe, D. Kazhdan, R. Langlands, D. Mostow, S. Rallis, N. Wallach, A. Weil, and, surely, many more. Of course, I benefited a lot from these visits, and, in particular, I remember a course by Kazhdan following the papers of Bernstein and Zelevinsky, a course of Gelbart on representations of $SL_2(\mathbb{R})$, and a series of lectures by Howe on the Weil representation and reductive dual pairs (Howe duality). Occasionally, Ilya would tell me about his current joint work with one of his visitors. This is how I learned about his intensive work with Gelbart, with Howe, and with Rallis.

Ilya spent each fall semester at Yale University. He twice invited me to join him (fall of 1979 and 1980). Thus I became familiar with the Yale mathematical community and its activity. Naturally, I spent a lot of time with my fellow graduate students, Jim Cogdell, whom I can call my

mathematical brother, Jeff Adams, and David Manderscheid. In two to three years, Ilya introduced me to the community of automorphic forms and representation theory of p-adic groups and of real reductive groups, great masters and fellow graduate students alike.

In various meetings, at Tel Aviv, at New Haven, or elsewhere, Ilya would convene some of us and explain his vision, where a converse theorem would be used to establish Langlands functorial lifting to general linear groups, and this is why he pushed the efforts to find new global integrals that would represent L-functions. Listening to him and then discussing his ideas, I marveled at his depth of thought, and I sensed the presence and charisma of a giant; I felt privileged to be his student, to learn from him and be guided by him.

During the years, his illness progressed, and his physical powers slowly left his body to the point that the only contact I could make with him was through the eyes. Even in those moments, I felt clearly that in that weak and paralyzed body lay a giant mind; a giant mind with deep blue eyes.

Freydoon Shahidi

I first learned about Ilya as a graduate student of Joe Shalika's at Johns Hopkins upon reading his well-known and impressive book with Gelfand and Graev. It was soon after that that Ilya came up with his remarkable converse theorem for $GL(3)$ that required only one twist. I remember vividly how excited Shalika was about it, and I still have the red-covered preprint of it from the University of Maryland. This was later published as part of the *Annals* paper jointly with Jacquet and Shalika. It played a central role in establishing some low-rank cases of functoriality, notably the cubic non–normal base change for $GL(2)$, by the three of them, as well as the symmetric square lift from $GL(2)$ to $GL(3)$ by Gelbart and Jacquet.

I cannot recall if I ever met Ilya while I was a student or even the first few years after that, but I was very much aware of the difficulties and hardships he had leaving Russia, which one can attribute in part to his greatness! I recall a conversation with Langlands soon after Kazhdan visited IAS. If you recall, he got out of Russia with a lot less trouble. Langlands felt this was due to the fact that Kazhdan was younger and kept a lower profile than Ilya did. No matter what, we were all delighted that he now was in the United States, doing mathematics the way he wanted.

There were two special years in the early 1980s in automorphic forms. The first one was at the University of Maryland in 1982–83 in Automorphic Forms and Representation Theory, which included several conferences, one of which was focused on automorphic forms in which Ilya spoke about Waldspurger's work on the Shimura correspondence. As far as I recall, this was the first time that I was present in one of his lectures and probably the first time I met him. But it wasn't until the summer of 1983 that I got to really talk to him in any extended fashion. This was during the one month summer program organized by Paul Sally, Phil Kutzko, and I at the University of Chicago that was designed around Roger Howe's CBMS lectures. It was there that I noticed how kind he was as a person. He told me how interested he was in my work and how much he would like me to visit him, which happened a number of times, as I explain later. He also showed how good a pool player he was while relaxing during evenings.

The next special year was that of IAS in 1983–84. Many of us participated in that one, and I met many new faces, including Iwaniec and Soudry, for the first time. I was there with my family the whole year. This was also the first time I met Edith. My son, Alireza, was then 2 years old and played with Ilya's daughter Shelly. They both had tricycles and would follow each other, of which there is a picture. I recall how Ilya was watching over both of them and how my wife Guity and I were impressed with how good a father he was.

It was during this year that Ilya's work with Rallis on the doubling method was presented by both of them. He kindly asked me to give a number of lectures in his seminar during the year—two of them I devoted to a presentation of the classification papers of Jacquet and Shalika, which were based on the Rankin–Selberg product L–functions developed by the three of them.

My first visit to Yale was upon his invitation to give a colloquium in October of 1989. I recall I arrived just before a talk by him in the Lie groups seminar in which he was talking about the transfer or lift of automorphic forms from classical groups to $GL(n)$, using his converse theorems with Cogdell, which at that time were still far from being usable in any practical manner. These practical forms of the converse theorem were not completed until 1999 [1, 2].

The original plan that Ilya was talking about was to develop appropriate integral representations for certain product L–functions, from which he and his collaborators at the time—Gelbart, Ginzburg, and Rallis—were hoping to deduce their necessary analytic properties to which converse theorems can be applied. But, as I explain later, this was not how this project finally succeeded, and here is where my own work is used to complete this project, when it is combined with his converse theorems with Cogdell.

Anyway, I still remember his talk. He mentioned Arthur's approach to the same problem, but using the trace formula. He said: "Arthur's approach is

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more general, but this approach is much easier!" Considering how difficult the fundamental lemma, now a theorem by Ngo, on which Arthur's approach relies, turned out to be, this was surely a correct assessment.

I again visited Yale in April of 1991, this time upon invitation by Dan Mostow to give a colloquium and a seminar. I must say how much I enjoyed visiting Yale every time. It was in my second visit to Yale that I felt Ilya's illness had become more pronounced.

Ilya and I attended many meetings together from that point on, including Langland's sixtieth birthday conference in 1996 and the Luminy meeting of 1999. It was during these meetings that I noted how kind and remarkably strong a person Edith is and how much, sometimes with help from Cogdell, she had made it possible for Ilya to participate in these meetings, trips, and social gatherings. I recall, in particular, our visit to Château d'If, a very complicated place to visit, even for a perfectly healthy person.

All this said, it has been the past ten years that have played a very special role in my relationship with Ilya. The 1999–2000 Special Year at IAS, organized by Iwaniec, Langlands, and Sarnak, cultivated a very special environment that allowed serious collaboration between us. In fact, the IAS gathering led to important collaborations between Cogdell, Kim, Ilya, and me and led to proofs of some long-standing and striking cases of Langlands's functoriality conjecture. This included Ilya's twenty-year project to prove the transfer of generic automorphic representations from the classical groups to general linear groups, which I explained earlier [3, 4, 7, 8]. As I explained, this was done by combining our two approaches, his converse theorems with Cogdell [1, 2] and the required theory of L-functions deduced from my work [9] with contributions from Kim.

After that we still wrote three other papers jointly with Cogdell to answer some of the issues related to functoriality, the last of which is still unpublished and covers the remaining cases [5, 6].

Ilya was a great mathematician, a leader, and a wonderful man. He was the leader in many of his collaborations, while he never lost his generous and gentle side. My collaboration with him has been the highlight of my career and a culmination of many years of work. I will never forget his kindness and I am missing him already.

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Peter Sarnak

I first heard of Ilya from Paul Cohen when I was a graduate student. Paul had the greatest admiration for Ilya's mathematics and with good reason. Paul's thesis was written under the guidance of Zygmund at the University of Chicago. It was concerned with trigonometric series and, in particular, sets of uniqueness, these being closed subsets of the circle for which, if a series converges to zero on the set, then all the coefficients must be zero. On finishing his thesis Paul learned of a young Russian mathematician who had worked on similar things but had seen quite a bit further (invented H-R sets and solved an old problem of Salem. . .). That Russian was Ilya, and from then on Paul was in awe of Ilya. It was a big setback for Paul, but as we all know, he recovered rather well. I remember a hiring case at Stanford many years later. Paul wasn't convinced by the candidate that we were considering. However, as soon as he heard that the candidate had done something that Ilya considered difficult and important, he immediately changed his mind. Apparently Ilya's standards were similar to his own.

I first met Ilya at Yale in 1980. I was visiting Serge Lang, and he told me that I should stop by Ilya's office, as he was interested in meeting me. I remember entering the office and finding Ilya sharing it with his student David Soudry. Seeing this indicated to me that Ilya was not the

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usual "famous professor"; here he was making a sacrifice so that his student, whom I assume he had brought on a long-term visit from Tel Aviv, was properly taken care of. After introducing himself and Soudry, he looked at me for a while and asked directly, "Are you Jewish?" I replied that I was, and I think this pleased him as he added proudly, "so am I."

Not much more happened at this first meeting. It was a few years later in Durham, England, that we became much closer and started what turned out to be a long collaboration. I had come to this conference by chance, and Ilya quickly sought me out. He was interested in a recent paper that I had written that explained something about the zeta function and the things connected with the Lindelöf hypothesis and automorphic forms on $GL(2)$ and speculations about $GL(n)$ (about which I knew little). The fact that the world leader on the $GL(n)$ theory took this (and me) seriously meant a lot to me. This meeting was a defining moment in my mathematical life, because, through his direct and very effective method of teaching as we researched together, I was able to learn the modern representation-theoretic approach to automorphic forms (which of course Ilya and others had founded). Ilya was very generous with his ideas and time, and thanks to him my mathematical horizons and appreciation were greatly expanded. Our dreams of attacking the Lindelöf hypothesis by variations of this approach, which we later developed together with Jim Cogdell, have never borne such dramatic fruit. The difficulties that we encountered have been rediscovered many times by others following this route. We were able to establish interesting things in this direction, including the final step in the solution of Hilbert's eleventh problem in 2000. However, whenever we got together to work after a break, Ilya would always want to begin by returning to Lindelöf, bringing new ideas and insights. Most mathematicians are single-minded and persistent—after all, these are qualities that are needed to crack a hard problem—but Ilya had this quality in spades. He had what I call mathematical courage. Many mathematicians will only attack problems that they have a reasonable idea how to solve before they start. Spending years on a problem and having little to show for the effort at the end was never an issue for Ilya.

In 1987 Ilya and Steve Gelbart organized a special year at IAS in Jerusalem. I spent the year there with Ilya and many others working in the theory of automorphic forms. For me this was one of the best "special year events" that I have witnessed. They lined up the leaders in the field as short- and long-term visitors, many reporting on their exciting recent breakthroughs. Again for me this was a fantastic learning experience, and I have done my best to pass on what I learned there, especially from Ilya, to my students.

Ilya's family, Edith, Vera, and Shelly, made us feel at home in Jerusalem, and since then our families have been close. We did many things together over the years, hikes in Israel and California. . . and more recently Ilya and Edith rarely missed my daughter's soccer matches if they happened to be in town.

In 1999–2000 there was a special year at the IAS in Princeton organized by Henryk Iwaniec and me. In view of the developments by Jim and Ilya on a new flexible version of the converse theorem and also by Henry Kim and Freydoon Shahidi in the theory of Eisenstein series (specifically the Langlands–Shahidi method), it was clear that something big might emerge if one could combine these works. I remember having to convince some of my colleagues at IAS that it made sense to spend four full memberships on them. They agreed, though perhaps not quite entirely convinced. The striking series of papers that came out of this four-man collaboration, yielding some of the most impressive and useful cases of functoriality known to date, is well known among experts.That year was one of the best special programs at IAS that we have had in recent years, and Ilya and his team played a big part in making it so.

When I spoke of Ilya and mathematical courage and generosity, these were only an epsilon of his generosity and courage as a person. He endured over a long period a very debilitating disease. I never saw him complain or feel sorry for himself. Thanks to the superhuman support of Edith, he continued to work under these conditions till the end, and his bravery paid off in all ways. Mathematically he continued well into his seventies to produce at the highest level. This was recognized, for example, in his invitation to give his fourth ICM invited address in 2002. His continued success under difficult circumstances has inspired many to do better. His generosity with his ideas and style of teaching is greatly appreciated by all of us who were his students in the broader sense.

I had no idea that his health had declined rapidly in November 2008, and it came as quite a shock when I learned from Edith that he passed away in February 2009. We will miss him and can't replace him, but we can rejoice in having been lucky enough to have known such a good man.

Note: Photographs in this article are courtesy of Gregory Piatetski-Shapiro (Piatetski-Shapiro as a young man, P-S at Yale, and P-S at the Wolf Prize ceremony), Steve Gelbart (Piatetski-Shapiro at the 1999 Luminy conference), and the Tata Institute (Piatetski-Shapiro at the Tata Institute in 1979).