On Soviet Mathematics of the 1950s and 1960s

D. B. FUCHS

When I received an invitation to write a kind of short mathematical autobiography for this volume, I had a strong impulse to refuse. But then I realized that it was a good occasion to describe some events of which I became partly a participant and partly a witness and which definitely deserve to be recorded. In what follows I shall try my best to avoid personal reminiscences.

Some words to begin with. I was brought up in a mathematical family. My father, B. A. Fuchs, was a well-known mathematician, S. Bergman's student and the author of numerous books and articles in several complex variables. Some famous Russian mathematicians, including N. V. Efimov and V. A. Rokhlin, were friends of our family. So it was quite natural that at an appropriate age I had a firm intention to become a mathematician, which was more or less equivalent[1] to the intention to enter Moscow State University (MSU). Nevertheless my father, while supporting the first of the two intentions, was strongly opposed to the second. He tried to convince me that I could not be admitted at MSU just because of my name. (I should explain here that the name Fuchs sounds Jewish in Russia; and my father actually was a Jew, at least biologically, while my mother originated from Russian peasants of the Middle Volga.) But I was not then of an age to listen to wise warnings. I applied to enter MSU, and was admitted there without any noticeable problems. It was the autumn of 1955.

I could not appreciate the overwhelming significance of the events at that time, but I can now.

The Jewish problem: does it exist?[2] I know that the question is discussed quite seriously in the West. I can hardly convince those who do not want

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[1] The pyramidal structure of our political organization reflects itself in pyramidal structures in many other aspects of our life. In particular, the level of Moscow University is uncomparably higher than that of Leningrad, Kiev, Novosibirsk, etc. Universities (and their level is uncomparably higher than that of, say, Saratov, Kazan, Odessa, etc. Universities).

[2] In what follows I discuss exclusively the situation in the Department of Mathematics of Moscow State University. The situation in the whole deserves more serious analysis; but basically it is the same everywhere in our country.

to believe, the more so because I have no reliable statistics. I simply want to say some trivial things known to everybody in our country. Before doing so I must stress that my own experience of this kind is far from dramatic. The three determining events of my life, namely entering MSU, becoming a graduate student at MSU, and getting a job there, coincided with three local minima of the anti-Semitic campaign; and the first of these minima was a global one, at least for the last 40 years.

It began with the struggle against the "cosmopolites" at the end of 1949. This struggle embraced all aspects of life—not only the entrance examinations. For example, my father had lost his job and was in various degrees of unemployment until the autumn of 1953, when he obtained a chair at a second-rate technical college. But this struggle did influence University entrance examinations—in a drastic way. Almost no Jews were taken into the Department of Mechanics and Mathematics (the usual abbreviation: Mekh-Mat) in 1950–1952 (though there were exceptions; for example, Ya. Sinai entered Mekh-Mat in 1952). I know of some ten Jews (or half-Jews) who entered in 1953 (some future superb mathematicians were among them, such as Yu. Manin, E. Golod, B. Mityagin and A. Dynin). I was told once that none of them was admitted to MSU according to the normal examination procedure (some were transferred from other Universities, while others were given the semilegal status of "a candidate to students"); probably this is only partially true. But in 1954, and particularly in 1955, the problem suddenly ceased to exist (or, rather, interrupted its existence). The about-turn was so sharp that many people (including my father) simply did not know of it, or could not believe it. During the next 15 years the situation took a definite turn for the worse. Nevertheless I believe that most of the people who examined prospective students in mathematics did it honestly (some mathematicians volunteered their efforts to support the racial purity of Mekh-Mat, but they were not numerous). The crucial job was done in those years by physicists (there were four examinations: written and oral mathematics, physics and a "composition"). But in the late 1960s and early 1970s the situation changed dramatically (I shall return to this period in my notes). There began a real war against Jews. All people with Jewish-sounding last names (some Germans and Estonians suffered without any guilt) or with Jewish patronymics or even with a parent with a Jewish patronymic were considered to be Jews, and every possible barrier was erected on their way to Mekh-Mat. The most effective weapons were special "Jewish problems" which were offered to "Jews" at the oral examinations in mathematics. Sometimes they were too difficult, sometimes they required a large amount of complicated calculations. Bad marks for the composition could be given to those who managed to escape failure at the oral examination. I know of many highly talented people who were not admitted to Mekh-Mat in the 1970s and later. It is enough to mention A. Beilinson, A. Givental and B. Tsygan (Beilinson was transferred to Mekh-Mat from the Pedagogical Institute when he was a third-year student,

Givental graduated from the Oil Institute, Tsygan studied in Kiev). I could name many less famous people who suffered the same fate. And how to count those who gave up?

As a rule in the years 1970–1988 there were some three to five Jews or half-Jews among the 500 students of Mekh-Mat each year. And usually they had to go through various Appellation Committees, complaints to the Ministry, etc.

I have the pleasant opportunity to confirm that in 1989 things were much better than in previous years; but the real trouble is that the examination commission consists mainly of the same people as before.

It would be interesting to understand the reason for this beastly unfair and pragmatically unwise campaign. I doubt that the Heroes of the Great Anti-Jew War were inspired by their pure Anti-Semitic feelings. According to my father, I. M. Vinogradov, who was always considered as the leader of Anti-Semitism in mathematics, surrounded himself with Jews in the late 1930s; in particular he never took any important decision as the Director of the Steklov Institute without a consultation with the local Party leader B. Segal; the last was actually Vinogradov's creature and protegé. Once (probably it was in 1968) I spoke to I. R. Shafarevich, and mentioned without any particular motive the year 1950 "when all the Jewish professors were driven from Mekh-Mat." "Who told you this?" Shafarevich asked with irritation. "Possibly it was Gelfand." I did not know what to say. Certainly, it was not Gelfand who had told me this, at least, for the first time; but it seemed to me that everybody knew it. "It is true that Gelfand had to leave Mekh-Mat then," Shafarevich continued, "but I had to do the same without being a Jew. They simply fired all the good mathematicians, Jews or not Jews." Now it seems to me that Shafarevich was more right than might seem at first glance. Mekh-Mat would never have been driven to its present miserable state if the policy of its authorities had been directed only against Jews. Russia is a very big country, and if you were not to admit the Jews to Mekh-Mat, or, say, left-handed, or blue-eyed ones, or apply some other arbitrary criterion, but were to honestly choose the best ones from the rest, then this would be highly unjust and immoral but would not have had such a terrible effect. The only way to deprive Russian mathematics of talent is to struggle against the talent, and this is exactly the struggle we were involved in. For example, those who graduated from the bet Moscow mathematical high schools were always regarded by Mekh-Mat's authorities as Jews, irrespectively of their actual origin. And this is a great (probably undeserved) honor to Jews that they were *a priori* included in the category of talented people.

To finish this subject I want to stress that in post-war Soviet Russia the notion of a Jew was by definition purely genetic, and Jewish genes were supposed absolutely dominant; you may be a quarter Jew and three quarters Russian (as Beilinson is), but you are regarded as a full-fledged Jew.

Mekh-Mat of the late 1950s. The "thaw" in the country gave rise to a

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"thaw" at the University. The level of (the best part of) the Mekh-Mat students was extremely high in those years. Anosov, Arnold, Golod, Kirillov, Manin, Novikov, Palamodov, Sinai, Vinberg, and many other first-class mathematicians studied there more or less simultaneously with me. Bari, Dynkin, Gelfand, Gelfond, Khinchin, Kolmogorov, Kurosh, Markov, Petrovskiĭ, Pontryagin, Postnikov, and Shafarevich were among the Professors. The Mekh-Mat schedule was oversaturated with first-rate lectures and seminars. Many undergraduate students wrote good scientific works (which is in general more common in the USSR than in the West), and Dima Arnold even solved Hilbert's Thirteenth Problem. According to Mekh-Mat's rules each second-year student had to choose an advisor from the staff of the Department of Mathematics (or outside of this staff: the times were liberal). My first adviser was B. N. Delone (he used to spell his name Delaunay). Many of us adored him, partly because of his independent and rather extravagant behavior. He organized a small seminar in Diophantine approximations which attracted a small group of second-year students. The seminar was quite unusual. You would just open your mouth, and Delone would exclaim, "How clever! We shall publish it in Uspekhi! No, better in Doklady! No, let it be Uspekhi after all." After several months there remained only two of us: Sasha Vinogradov (no relation to I. M. Vinogradov!) and I. And we did write a note in Doklady (jointly with Delone). I do not think anybody ever read it; in any case two lines were missing in the printed version of the main statement. Certainly we were pleased and proud (both Vinogradov and I were in our teens) but even then we did not take it seriously. In any case, besides Delone's seminar we attended Dynkin's seminar on Lie groups and homogeneous spaces. And after a year we decided to find an advisor in this seminar, but not Dynkin himself. This led us almost forcibly to A. S. Schwartz, who was then himself a last-year graduate student. Schwartz gave his okay and thus we became topologists.

Topology of late 1950s and 1960s in Moscow. In the 1930s and 1940s topology flourished in Moscow, and the center of it was L. S. Pontryagin. According to V. A. Rokhlin, Pontryagin knew much more than he published, and no article in topology published by anyone in the world contained much new for him. But in early 1950s the "French works" appeared. Many beautiful geometric results of Pontryagin's era were understood as mere trivialities. And Pontryagin abondoned topology for optimization theory. Different opinions on this dramatic episode were expressed by different people (including Pontryagin himself—see his autobiography in Uspekhi, 1981); I will not touch on this subject. But I cannot avoid reflecting on its terrible influence on topology at Mekh-Mat and consequently in the USSR.

The official head of Mekh-Mat's topology was P. S. Aleksandrov, whose interests were then restricted exclusively to "set-theoretical topology", or "general topology"; that is, the domain of mathematics which studies the notion of a topological space in maximal generality with special attention to various

pathologies. The staff of the Chair of Topology consisted almost completely of Aleksandrov's students (the only exception was made for me in 1963–1967). In addition to this there existed a strong public opinion (cultivated by Pontryagin and his colleagues) that topology had exhausted itself and was no longer a worthy subject to work in. Nevertheless a small company of topologists has formed around A. S. Schwartz and M. M. Postnikov. The seminar had seven permanent participants. These were S. Novikov, B. Averbukh (he calculated the groups of oriented bordisms independently of J. Milnor, but after Milnor published his work), L. Ivanovskiĭ (who did an enormous amount of calculations of the homotopy groups of spheres, and never changed the subject), Vinogradov, myself, and two more mathematicians who never were topologists but wisely studied topology as if it were their field: D. Anosov and G. Tyurina. (The latter became my wife in 1961 and died in an accident in 1970.)

Our seminar had no formal leader. Schwartz received his Ph.D. and had to leave Moscow. (He had no Moscow "*propiska*"; I suppose here that the reader is familiar with this notorious notion. Aleksandrov possibly could have obtained permission for him to stay, but he definitely did not want to.) Postnikov did not like teaching, and he had some strong interests outside mathematics. So we were left to ourselves. Unfortunately, Novikov was of our age group, and it was embarassing for him to assume the usual functions of a seminar leader (to tell us what to read, to offer problems, etc.). The same difficulty, by the way, has always been present in my relations with Arnold. I could not be his student, but nevertheless some years later I became a kind of "in-house topologist" in Arnold's seminar. I even published two articles with solutions of his problems; both of them are still referred to, especially "Cohomology of the braid groups modulo 2".

Our small group was under hard pressure from two sides. Firstly, our fellow students reproached us that we pursued some homology and homotopy groups instead of doing something really worthwhile, say, in PDE or classical function theory. Our reaction was fully inadequate: it resulted in a sheer ignorance of analysis (at least on my part). Later I tried to study some analysis but the time was lost. Unfortunately, even in topology we tried to avoid everything which did not involve homological algebra. I learned the notion of a spectral sequence a year earlier than that of a barycentric subdivision. And do not ask me when I learned that the Grassmannian $G_+(4, 2)$ was diffeomorphic to $S^2 \times S^2$.

Secondly, there was a bitter struggle between set-theoretic topology and algebraic topology. Aleksandrov hated algebraic topology (of which he was one of the creators) and tried to stop its development by any means. Good-humored Anosov invented comic "Rules for the Chair of Topology" which prohibited studying "commutative diagrams and exact sequences which actually were neither commutative, nor diagrams, nor sequences, and the more so not exact, but were in fact senseless combinations of zeroes and arrows".

Once Schwartz wrote in an advertisement of his lectures that "the main difference between set-theoretic and algebraic topology is that while the latter solves difficult problems with simple methods, the former solves simple problems with difficult methods". Such jokes were among the reasons that after getting his Ph.D., Schwartz had to leave Moscow for Voronezh; and we had lost our potential leader. The main battle of the war took place some years later when Novikov and Shafarevich tried (in vain) to prevent three major students of Aleksandrov from getting their Doctoral degrees (much higher than the American Ph.D.). All this seemed very important to us until 1968 when both armies found themselves on the same side in the front line in the "Esenin-Volpin case" (see below).

In the meantime the general attitude toward topology changed, and the main reason was the Atiyah-Singer index formula. The index problem was actually formulated by Gelfand, and our mathematicians were always interested in it. The trouble was not in the mere fact that the solution was found by somebody from the outside; but our PDE experts could not even understand what was written on the right-hand side of the formula! They became eager to study topology. The modest lectures which I used to give to several bored students became one of the most popular courses at Mekh-Mat. The lecture halls were overcrowded. people wanted to learn everything in topology, but especially the ingredients of the Atiyah-Singer formula: K, ch and Todd.

Certainly, my topology was useless to all these people because the Atiyah-Singer formula was as far from me as from those who were listening to me. At the time I thought that the most important problem in mathematics was the calculation of the homotopy groups of spheres, and the most useful device was the Adams spectral sequence. This spectral sequence was the ultimate purpose of my lectures. But gradually everything became more or less normal. The ignorance of most of our mathematicians in topology became less dramatic, the popularity of topology decreased to a reasonable level, and my lectures in topology became better.

The only one of us who did really bright work in topology was Sergei Novikov. When he was an undergraduate student he worked mainly in homological algebra; he invented a kind of Steenrod square operations in the cohomology of Steenrod-like algebras and applied it to the Adams spectral sequence. Then he began to work more geometrically and wrote a paper on cobordisms entitled "Some homotopy properties of Thom spaces". The most outstanding among his later works were about smooth structures on manifolds, codimension 1 foliations on 3-manifolds, the topological invariance of the rational Pontryagin classes, and cohomology operations in cobordisms (the Adams-Novikov spectral sequence). Novikov became a theoretical physicist in the late 1960s, but he still writes in topology from time to time.

My own scientific achievements in topology were modest. I did some reasonable things under the guidance of Schwartz, but then I had to choose

subjects myself. I invented a kind of a formalization of Eckmann-Hilton duality. Some people, including Peter Hilton, approved of it. Probably the work was not bad, but I could not follow the temptation to continue it. As a result of this, at the age of 27 or 28 I simply did not know what to do next. But then I made Gelfand's acquaintance, and it was a godsend for me.

Gelfand-Fuchs. I am sure that most readers who happen to know my name, know it in this combination. In 1967–1970 I worked jointly with Gelfand, and we published some 15 articles; some 5 articles more were published later. I had no idea that Gelfand knew about my existence until somebody (it was probably M. Agranovich) phoned and told me that Gelfand wanted to meet me. I came to his apartment where there were many people (I remember D. Kazhdan and G. Margulis), and the telephone rang every minute. Gelfand asked me what I was doing, and happily enough listed to my answer not very attentively. He said then that he always wanted to work in topology (*this was the truth*), and made me an offer to work jointly with him. And it turned out that he was quite serious; we began to work. Our first observation was that if one takes the quotient of $\mathbb{R}^\infty - 0$, where $\mathbb{R}^\infty$ is the Tychonov space, by the group of homotheties, then one gets a smooth ∞-dimensional manifold S (a "sphere") on which all globally defined continuous functions are constant. And the projection $\mathbb{R}^\infty - 0 \to S$ is a nontrivial principal $\mathbb{R}$-bundle (which was impossible over a paracompact base). Moreover, this $\mathbb{R}$-bundle turns out to be universal in some sense. We tried to generalize this construction to a general noncompact Lie group, and rediscovered the Van Est cohomology groups as groups of certain characteristic classes. Then we approached infinite-dimensional groups, and this led us to the cohomology of the corresponding Lie algebras. A tentative calculation gave a surprising result: the second cohomology group of the Lie algebra of vector fields on the circle was neither trivial nor infinite-dimensional: it had dimension 1. Thus we discovered the central extension of the Lie algebra of vector fields on the circle which nowadays is known to all theoretical physicists under the name of the Virasoro algebra. During the next two years we made a lot of successful cohomology calculations. All this time it surprised me a great deal that Gelfand, who had a huge number of first-class results in mathematics, greeted every theorem as if it were his first success. But certainly this is the best attitude for scientific work.

The case of Esenin-Volpin. To finish my notes I want to record some events of Spring 1968 which later proved to be so important for Mekh-Mat.

The Brezhnev era began with several political trials, and the general atmosphere in the country was dreadful. But Mekh-Mat remained relatively free and happy thanks to the efforts of I. G. Petrovskiĭ, who was then Rector of MSU. This situation was very unstable, for its preservation required the acceptance of all kinds of compromises by members of Mekh-Mat's staff.

One of the important features of our political life in the late 1960s was "signing letters". After Sinyavski and Daniel's trial, some groups of people

wrote collective letters to various power institutions with various kinds of protest (from rather mild to very strong). Of course, the authors of the letters were punished, but the letters kept on being written.

The letters concerned various things, mainly violations of the rights of various individuals and groups of people. To sign a letter became fashionable. There were so many letters that most of them caused no reaction from the authorities, and this fact encouraged various kinds of people (scientists, writers, artists, even schoolchildren) to write more and more letters. In order to stop the campaign, the authorities had to choose one of the letters for an exemplary punishment of its authors. And it seems that they chose the letter about Esenin-Volpin.

A. S. Esenin-Volpin was the son of the great Russian poet [Sergeiĭ Esenin], a good mathematical logician and a confirmed dissident. In January 1968 he was taken to a "*psikhushka*", a special psychiatric hospital for political deviants. Even now I do not know the real reason for this action, though I can guess that it was connected in some way with the approaching fifteenth anniversary of Stalin's death on March 5, 1968. After some unsuccessful private efforts to release him, a letter was written and signed by 99 mathematicians. Even now I do not dare list those of them whom I remember; I restrict myself to a neutral statement, that almost all well-known mathematicians who already had their Ph.D.'s in 1968 and who were seldom seen abroad later, were among those who signed the letter. The letter was very clever; it did not contain any general complaints or bitter accusations of the KGB (as was usual in those letters). It just stated the plain fact that there were some formal rules for putting people in asylums, and that these rules were definitely violated by the "emergency medical help" service which had taken Esenin-Volpin. The whole matter was briefly broadcast by the Voice of America. The violations were so evident that Esenin-Volpin was actually released immediately after the Voice of America broadcasting. (He was transferred to the Academy's hospital, where he was out of danger.) Later the authorities tried to prove that Esenin-Volpin had been released before the letter was written, and that the authors of the letter were aware of the fact. But that was too evidently wrong.

The signers began to be pursued. There were meetings at various places where the signers were vilified by their colleagues. But many of these meetings went wrong, partly because those who were to conduct them had only a vague knowledge of the events. Some signers were offered statements to sign such as "I certainly did not mean to say that... " or "I disapprove of the Voice of America's interfering... " or other such things. Some agreed but others refused.

Gelfand refused in the most persistent way. There were signs that the authorities wanted to represent him as the leader of the whole rebellion. There appeared nasty rumors that Gelfand had directed the whole campaign by phone and so on. It was definitely wrong. The Academy's authorities

tried to make him sign a letter to some American mathematicians stating that everything was fine with Esenin-Volpin, and that the American newspapers that highlighted the events were wrong. Gelfand even agreed to sign the statement that everything was fine with Esenin-Volpin at the moment, but they insisted on the phrase "Your newspapers have deceived you", which Gelfand refused to include. He was in bad condition; his blood pressure went up, but he was resolute not to yield.

Nobody knew what might happen. And we did a wise thing. A small group of mathematicians (including Shafarevich, Arnold, Tyurina, myself, and some others) went to a remote skiing place in the Caucasus. All of us took our formal leaves. We had no connection with the outer world and did not want to know what was going on in Moscow. And when we returned everything had been settled.

There had been discussions in some high spheres, and it was decided to act without extreme measures. Thank God, nobody was arrested. Two people lost their primary jobs, and some ten people lost their secondary jobs. Many people had difficulties with promotions. No one was allowed to go abroad; for example, no one of the 99 went to the Nice Congress. (Even Novikov could not receive his Fields medal in person.)

But it was not personal matters that really counted. A year later Efimov was replaced as the Dean of Mekh-Mat by Ogibalov, who was one of the Heroes of the 1937–1938 purges. Every side of Mekh-Mat's life was terribly affected: entrance examinations, selection of graduate students and new members of staff, the possibility to organize seminars, etc. Only now has Russian mathematics begun to recover from the blow; we shall know before long whether it is still possible to recover.