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**The Second Half of the 20th Century and its Conclusion:
Crisis in the Physics and Mathematics Community
in Russia and in the West**

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For me, the physics and mathematics community is the community of mathematicians and theoretical physicists. I grew up in it, worked in it, and work in it today. It is precisely to this community that the anxious thoughts I will try to describe here are related. A large part of them were born in me two-three decades ago and have been maturing ever since. However, at first I related the evolution of our science to the general decay and decomposition of communism, the growth of its incompatibility with a highly developed intellectual community, the deepening incompetence of the upper echelons of power, particularly increasing in the Brezhnev era. I believed that these processes were characteristic only of the scientific community in the USSR, whose disintegration was historically predetermined (although none of us expected that it would occur so soon). Today, after having worked in the West for several years and having observed the situation in the most developed countries, I can say this: my anxiety about the evolution and the fate of the physics and mathematics community continues to grow. I am referring to our community in the entire civilized world and not only in Russia; already for a decade this community has been going through a difficult period of transition, a process that will hardly end within the next ten years.

1. Evolution of mathematics in the 16-19th centuries

My generation of mathematicians and theoretical physicists did not expect to face such a crisis. In the 1950s, when we were at university, this community stood quite high. Some four-five centuries of uninterrupted development of our sciences were behind us. We felt this would always be so. At the time I imagined the evolution of mathematics and of mathematical thoughts about the laws of nature as follows.

16th century: development of the algebra of polynomials; solution of algebraic equations of degree three and four; as the main product, introduction and use of negative and complex numbers (the negative numbers were accepted at once; the struggle for complex numbers was long, lasting to the present day).

17th century: appearance of coordinates, which allowed for translation

of geometry to the language of algebraic formulas and enlarged its subject matter; development of calculus; statement of mathematical laws underlying natural phenomena: Fermat's variational principle for light rays, Galileo's relativity principle, Hooke's law, the universal law of gravitation, Newton's laws. The first significant instances of mathematical derivation of the laws of nature from fundamental principles, insufficiently appreciated at the time: the derivation of the law of refraction of light on the boundary layer between two media from Fermat's variational principle and the derivation of Kepler's laws by Newton, later to become the basis of the modern scientific method. The emergence of the basic ideas of probability.

18th century: the development of analysis becomes a powerful flow, including linear differential equations and the method of eigenoscillations, the calculus of variations and much else. Differential geometry and number theory appear, probability theory develops further. Theoretical mechanics, including celestial mechanics, becomes a mature and well-developed science. Hydrodynamics comes into being.

19th century: the flow of mathematics, including probability theory, continues to gain strength. Complex analysis arises; the solvability problem of algebraic equations gives rise to Riemann surface theory and group theory; linear algebra is created; the study of symmetry intensifies, Lie algebras make their appearance; geometry, number theory, Riemann surface theory, differential equations, Fourier series and other topics become powerful, well-developed disciplines. New branches of physics with their own mathematical laws appear, e.g. electricity and magnetism; technological developments give birth to thermodynamics, and then to statistical physics and kinetics. The end of the 19th century is marked by the appearance of the first germs of the abstract branches of mathematics, such as set theory and the theory of functions of a real variable. The qualitative-topological branches of mathematics (qualitative theory of dynamical systems and topology) emerge. The first ideas of mathematical logic are born. In the physics community, a deep understanding of the insufficiency and even self-contradictory character of classical physics based on Newtonian mechanics and classical electrodynamics becomes predominant. One should keep in mind that during this period huge advances in technological development took place. Undoubtedly, the growth of physics was largely one of its consequences. The mathematical comprehension of the laws of nature was preceded by experimental discoveries.

Such was our science at the turn of the 20th century. The lead-

ers of mathematics of that period—Poincaré, Hilbert, Weyl—personalized the watershed dividing the 19th and 20th century and the beginning of our history, the history of our time (“our” from the point of view of my generation, for whom mathematicians coming of age in the 1920s and 1930s were senior contemporaries, people we had the opportunity of contacting). Speaking of theoretical physics, its prehistory ends with Einstein and Bohr, i.e., with the creation of relativity and quantum physics. And their scientific children, so to speak, are those who directly taught people of my generation. I do not claim to have given here a thorough history of the development of science. The reader must excuse me for failing to name many important domains. My aim is quite different: to demonstrate how this development was a powerful increase in the level of knowledge; how achievements of previous generations were understood by the subsequent ones, were unified and simplified; how the old organically merged with the new.

2. Education up to the mid 20th century.

The powerful and increasing flow of knowledge in the exact mathematically based sciences constantly required the reassessment and modernization of education. Finally, by the beginning of the 20th century, a stable system was created; in it, the first essential stage was the school system, with the gymnasium (as it was called in Russia) giving a general education from the very beginning to the age of 17-18, ten or eleven years in all; this was followed by a specialized higher school—the university. In the 20th century it became necessary to supplement the university by post graduate studies, i.e., several years of even more specialized training, devoted to the acquisition of in-depth narrow mathematical specialization and the development of creativity, leading to the first research work. There were variations in this system in different countries, different words were used for the successive stages, but the number of years (8-9) for the whole course (undergraduate + graduate school) was the same.

In the first half of the 20th century, secondary gymnasium education was not yet compulsory, but the level of knowledge required “for everyone” was rising in the leading countries. In the second half of the 20th century, the last part of the gymnasium studies became more specialized, in order to ensure the acquisition of more knowledge in mathematics, in physics, etc.

The main trait of this system was the presence of a very strict system of examinations: in mathematics, for example, students from age 10 had to take exams every year. The early subjects—arithmetic, geometry, algebra—were studied very seriously. The study of all important subjects was crowned

by an examination, but mathematics was studied with special care, as was the ability of writing grammatically. A strong foundation was created; on it, further mathematical (and other) education could be built. It was especially important that this foundation was erected at a sufficiently early stage, leaving enough time later to learn higher mathematics as well as the sciences based on it, e.g. theoretical physics.

Allowing the right time to be lost, putting off learning, leads to essential losses. The older one grows, the harder it is to put things into one's head, and life takes its toll, making it impossible to study forever. Not least in importance is the early developed stable habit of intense work, of learning mathematics, of logical precision, of perseverance and concentration of one's mind on the goal. This habit does not come naturally to everyone, and is lost without early training. To facilitate this training, to help acquire these habits and learn to love mathematics and related sciences, mathematical circles (voluntary informal math study groups) and olympiads were organized at some time. They turned out to be extremely effective. This entire education complex is an achievement that we cannot reject without risking to lose all scientific education in mathematics.

3. Mathematics: the 20th century.

The first half of the 20th century is a period of unchallenged dominance of set theory in the ideology of mathematics. The development of set theory itself led to such general abstract concepts and constructions that their meaningfulness and consistency (absence of contradictions) began to be questioned. This led to the active development of mathematical logic, discussions of the consistency and completeness of set theory itself and of mathematics as a whole. The foundations of mathematics came to the forefront of mathematical research, as well as the justification and rigor of proofs, even in the interaction of mathematics with its applications and with the natural sciences. The mathematical community completely separated itself from the community of theoretical physicists. The study of higher mathematics became entirely oriented towards the rigor of exposition. This led to a significant reduction in the meaningful study of the branches of mathematics oriented to applications in the natural sciences.

This was particularly true of theoretical physics, which had not been understood by the mathematical community. In the USSR, classical mechanics remained within mathematics, while theoretical physics moved to other university departments. Something of the same kind occurred in the West in the 1920s, except that the experts in mechanics closer to the applications

moved further away from the mathematicians than in Russia: only those who "prove rigorous theorems" (at least as part of their work) stayed on with the mathematicians.

The USSR educational system under which my generation of mathematicians was brought up was conceived from the 1930s to the 1950s. General physics was still studied, but contemporary theoretical physics was practically ignored. In the final count, only some elements of special relativity were included in the last parts of the concluding physics course (at Moscow State University the progressive professors of mechanics finally added special relativity to the syllabus of mechanics undergraduates only thirty years later, in the 1970s); general relativity and quantum theory remained outside the sphere of university mathematical education. The first attempts to include them in the syllabus date from approximately 1970, and cannot be regarded as successful.

There are many subjective aspects to this story: already in the 1920s, conservative experts in mechanics such as Chaplygin ignored these new disciplines, regarding them as western foolishness. P. S. Alexandrov told me that Chaplygin would not allow P. Urysohn to include the then new general relativity in his graduate studies exam.

This is our specifically Russian trait—a tendency to conservatism, to isolation from world science. Even Chebyshev in the 19th century, with all his brilliant analytical talent, was pathologically conservative. A. F. Kagan told us that he, still a young privat-dotsent, tried to tell the aging Chebyshev about modern geometry and other fields, and the latter expressed his contempt to "fashionable disciplines" such as Riemannian geometry and complex analysis. The school created by Chebyshev was strong, but also had a strong tendency to provincialism.

The French school, after Poincaré', beginning with Lebesgue and E. Borel, followed an ultra-abstract direction and created in Paris (and then in the whole world) an unbridgeable chasm between mathematics and the natural sciences. Some of the mathematical stars of the time (such as the E. Cartan and Jean Leray) did not like this state of affairs, but, despite their personal influence, remained isolated. The brilliant groups of Parisian mathematicians, formed in the 20th century, cultivated and deepened this chasm, became the ideologues of a total and unified formalization of mathematical education, including the primary and secondary school level. We call this program "Bourbakism".

Fortunately, although the founders of the Moscow mathematical school

(D. F. Egorov and N. N. Luzin) imported set theory and the theory of functions from Paris in the early 20th century, some of their pupils were influenced (while contacts with the West were still possible) by the more powerful and ideologically richer school of David Hilbert. As a result, the Moscow-Leningrad school followed a more reasonable path than the Parisian one, and did not exclude, in fact encouraged, interaction with the scientific world outside of mathematics.

Although Hilbert did proclaim a program of unified axiomatization of mathematics and theoretical physics, his understanding of it was definitely out of the ordinary. For example, during the dawn of general relativity theory, he proved the deeply nontrivial result on the Lagrangian character of the Einstein gravitational equations, a theorem which remained insufficiently appreciated for long, but later played a very influential role. Thus Hilbert confirmed the might of the axiom that all fundamental physical theories must be Lagrangian. This had been quite unclear before then for the case of Einstein's theory. Any physicist will understand the value of this way of looking at "axiomatization and formalization", a far cry from proving existence and uniqueness theorems for hundreds of types of equations or writing up rigorous proofs of results already obtained by physicists or engineers.

Among Hilbert's pupils, Hermann Weyl avoided set theory and formalization; he actively interacted with physicists and introduced fundamental ideas, J. von Neumann was among the ideologues of formalization and axiomatization, but (like Emmy Noether) understood this in a nontrivial way, close to that of Hilbert. Noether and von Neumann made substantial and useful contributions to this program, and now all of us work with the notions that they introduced and specified. Hilbert's school put into practice the ideology of a unified mathematics, uniting it with theoretical physics, the ideology of "useful formalization" -the one that serves unification. There is no need to artificially make the simple complicated. For example, von Neumann's general theorem in the spectral theory of self-adjoint operators is a deep and complicated set-theoretic theorem; but it should not replace, in the educational process, the theory of the simplest classes of differential operators where one can do without it. Occasionally it turns out, especially in the case of singular coefficients, that one cannot manage without the general theory.

But to create the cumbersome set-theoretical axiomatization of analysis starting from the elements (like Bourbaki)-this is a real nonsense which can destroy the entire realistic calculus. However, this was part of the ideology

of the subsequent period.

4. Mathematics and physics: 1930 to 1960.

Unfortunately, the German mathematical school (including the Austro-Hungarian one) was dispersed by nazism. Its surviving stars left for the USA and brought up the brilliant post-war generation of American scientists. As I was told by French physicists in 1991 when I was working in Paris, the development of quantum theory in France was aborted by the Count Louis de Broglie, who played the role of Lysenko' in the French physics community, despite his personal contribution to this theory at its early stages (*'Editor's Note: T. D. Lysenko (1898-1976), notorious biologist, highly supported by Stalin and Khrushchev. He used his administrative position to fight genetics as a "phony bourgeois science"; this led to the rejection of Mendel's laws and genetics in general in the school and university syllabus and even to the arrest of numerous biologists in 1940s and 50s.*) It is said that he was remarkably stupid and remarkably steadfast in his stupidity. And this led to sad results.

Before World War 1, there was no serious theoretical physics school in Russia. The first Russian stars of international theoretical physics (Gamov, Landau, Fock) emerged in the 1920s and 1930s from direct contacts with the best ultramodern European school of quantum theory headed by Niels Bohr. Gamov soon chose to stay in the West, while Landau and Fock created strong schools in Moscow and Leningrad. I think that Landau derived his approach to creating a school and the style of his seminar from his contacts with Hilbert's circle. In the period from 1930 to 1950, he developed and put into practice his fundamental ideology: how and what should be taught to a theoretical physicist. In the USSR, the new schools of Landau and Fock were supplemented by "Russian autochthones", the community that grew out of the classical physics school of L. I. Mandelstam and others, which was particularly strong in applied fields; some of them also made significant contributions to contemporary quantum theory.

How the Russian community of pure mathematicians of the 1930s rejected such a brilliant person as Nikolai Bogolyubov is a curious story. Of course, the flaws of his joint publications with N. M. Krylov were serious, but the aggressive criticism of their papers by A. A. Markov in 1930 was excessive. After that no one believed Bogolyubov. He solved Luzin's problem concerning almost periodic functions; Men'shov, who was asked to check the paper, replaced a serious verification by pecking at details, always in a formalistic manner. Thus Men'shov pointed out several inessential shortcomings. This caused the work to be considered dubious. As a university student in the

late 1950s, I heard that there had been such a paper by Bogolyubov in the 1930s, but doubts about its validity remained. Later I learned that in the world literature on function theory the paper was regarded as verified long ago and classical, and told this to my father. He expressed his contempt for Men'shov's manner of replacing verification by henpecking. In any case, Bogolyubov with his intuitive, loose style in the presentation of proofs, was rejected. This turned out to be beneficial for him. He spent years studying quantum physics. Later, having produced brilliant work in the theory of superfluidity, he had serious problems trying to become part of the physics community: the realistic and sharp criticism, characteristic of Landau, was unexpected for him, and poisoned his first talks. He later overcame this criticism (although not too soon), and convinced Landau, but their relationship remained strained and tainted with jealousy.

Here an important role was played by the fact that people like Vinogradov and Lavrentiev used, not without success, Bogolyubov's tendency to support dubious people in their campaign against "Jewish physics". Later, in the 1970s, after his conflict with Vinogradov, Bogolyubov threw out all this ballast of disgusting notions from his mind. All these years he very carefully hid from his friends such as Lavrentiev what he thought of their pretense of considering themselves physicists without knowing what modern theoretical physics is about (although Lavrentiev was very talented). Bogolyubov told me, in the 1970s, that the mathematical community has no idea of how much must be learned to be able to understand what contemporary quantum physicists are talking about; in doing this, he used idiomatic expressions which I will not try to repeat here.

In the late 1930s, as my father told me, mathematicians invited Landau to the Steklov Institute to give them lectures on what quantum mechanics and statistical physics are all about. After the course, they were very annoyed; they had a strong dislike of that logical mix-up, as my father put it. Later, after the publication of von Neumann's book, two of them, Kolmogorov and my father, read it with great pleasure. A precise axiomatic style was what they needed. They wanted to understand the underlying logic, not quantum mechanics. A third listener, Israel Gelfand, decided to understand that part of physics as physicists did. He began to attend the Landau seminar, and continued to do so for ten years (or more). Gelfand was the only applied mathematician capable of talking to real physicists, and not only to experts in classical mechanics, during the period when the crucial classified research of the 1940s and 1950s took place in the USSR. He

acquired a great deal from physics for his own mathematics, initiating the theory of infinite-dimensional representations, whose elements he picked up from the world of physics, and solving the inverse scattering problem posed by physicists (M. A. Naimark, B. M. Levitan, and V. A. Marchenko also took part in that research). Gelfand's pupil A. Berezin also borrowed the problem of constructing the fermion analog of the integral from the Landau Seminar.

Except for the people mentioned above, none of the Moscow mathematicians of that generation studied any more physics. The contact with quantum physics was closed for them; true enough, the disinterested lover of science Men'shov continued to visit the physics seminar for many years, even without any understanding. I think that I have listed here all the famous members of the elder generation of Moscow mathematicians of the 1930s and 1940s who knew at least something about the quantum physics of the 20th century. Incidentally, A. Ya. Khinchin tried to study the foundations of statistical physics, but his attempts were met by physicists with deep contempt. M. A. Leontovich told my father that Khinchin understood nothing. Among the outstanding Leningrad mathematicians, A. A. Markov in his youth wrote a useful paper that put some order in the foundations of the theory of ideal plasticity, but later never returned to the natural sciences.

Such a brilliant geometrical talent as A. D. Alexandrov wrote some nonsense deduced from the axioms of Lorentz transformations; one feels ashamed to recall the work of his school on that topic; although he was educated as a physicist, here his feeling for axiomatization led to absurdities. Quantum physics came to the Leningrad mathematical school much later, in the 1960s, with Ludwig Faddeev, who in his youth was a Fock's student before he became a graduate student of Ladyzhenskaya and began to prove rigorous theorems. However, recognizable features of a physicist—holes in the proofs—would show up in his work. His best work was to come later, when he returned to the role of a quantum mathematical physicist close to the physics community.

For a long period, a very special role in Moscow mathematics was played by Andrei Kolmogorov. Being an ideologue of set theory, axiomatization of science, and the foundations of mathematics, he possessed, at the same time, the remarkable ability to solve difficult and important mathematical problems, and to do reasonable and efficient work in applications and the natural sciences as well as humanities. From axiomatization of probability theory on the basis of set theory, he could move on to the discovery of the law in isotropic turbulence; from mathematical logic and delicate counterexamples

in the theory of Fourier series, to ergodic theory and the analytic theory of Hamiltonian systems, solving old problems in a novel way. He made significant contributions even to algebraic topology. At the same time he had strange, I would say almost psychotic, aberrations: in education, both school and college, he fought against geometry courses at the university, tried to rid the high school syllabus of complex numbers and to implement set theory everywhere, sometimes quite awkwardly. Vladimir Boltyanskii once performed for me an amusing scene that occurred when Kolmogorov was "chasing" the complex numbers from the school syllabus. As ridiculous as it may sound, he had the same ideas as Bourbaki in educational questions, sometimes even more absurd. He did not know contemporary theoretical physics, and his attitude to the natural sciences was based on classical mechanics. Kolmogorov, however, possessed a remarkable gift: to find the crucial points, to discover what would subsequently become indispensable for many researchers. Observe how his discoveries in dynamical systems in the 1950s (and those of his pupils) became widespread in the second half of the 20th century.

Fortunately, the superprestigious Moscow University, with its new luxurious palace, was, by Stalin's order, administrated by an outstanding scientist and (which was particularly rare among the leading mathematicians- administrators) a very honest man, I. G. Petrovskii. The ideological administration of mathematical education was de facto entrusted to Kolmogorov. Particularly important was the fact that in the late 1950s, all the mathematicians of Moscow with any degree of creative potential would gather at seminars held at the mechanics and mathematics department (mekh-mat) in the evening, and at sessions of the Moscow Mathematical Society. Later, I have never encountered anywhere in the world such a concentrated and powerful community covering all the branches of mathematics. Such was mekhmat when I studied there. In that community, Kolmogorov's pupils Arnold and later Sinai shined; they grew out of set theory, the theory of functions of a real variable, and dynamical systems. I assessed the domains in which they worked as the last explosion of the ideas of set theory, Kolmogorov's swan song. This was then very fashionable, but I did not like set theory. I felt that it was the heritage of the 1930s, and there would not be many truly novel ideas forthcoming there.

5. My generation: the 1960s.

Anosov and I studied modern topology together, but I did that professionally, while for Anosov it was a hobby. He was drawn towards dynamical systems, and under the influence of Smale carried out a brilliant work. Arnold,

on the contrary, was clearly attracted by topology. Some new approaches to analysis originating in topology, such as the ideology of transversality and general position, which he learned from me, made a profound impression on him. With his help, I began to become acquainted with the ideas of geometry underlying Hamiltonian mechanics and the hydrodynamics of incompressible fluids; he led me to the problems of the theory of foliations. Soon afterwards, I began to attend the famous Gelfand seminar and had many conversations with him. His outlook on mathematics was the closest to me, and a mutual understanding arose between us.

I finished my postgraduate studies in 1963, already a well-known topologist. The authority of that field was growing rapidly. In the 1950s there was a great deal of talk about this new remarkable field, which Hilbert had not understood, and its sensational discoveries, where the breakthrough was mainly carried out in the early 1950s by the French school. It was considered that after Pontryagin there was a long interruption in the USSR: first class topological work, comparable to that in the West, did not appear for a decade. The influence of topology on algebra, on partial differential equations, algebraic and Riemannian geometry, dynamical systems was really impressive.

I saw my goal in filling this gap in Soviet mathematics. Until I reached international stature, I could not think of anything else, although I would readily listen to people from other fields, trying to understand the foundations. In 1960-1965 scientific fortune was on my side, and I succeeded in reaching my goals. Continuing to work in topology, I asked myself: What is the use of our activities? Where and when the ideas that we are developing will be applied? For a sane person this question is natural and even necessary. The love of mathematics does not annul this question. Already at that time I discerned a kind of inferiority complex in this connection in many pure mathematicians, a sickly fear of this question.

On the contrary, other mathematicians, earning their bread in applied scientific institutions, worked usefully there, but without enthusiasm, by rote, so to speak, in order to serve someone; they did not feel any inadequacy, but regarded only pure mathematics as the real science, and devoted to it all their free time.

In the early 1960s, the anti-mathematical aggressiveness of the new class of professional computational mathematicians increased. They began a campaign against pure mathematics, claiming that the true development of mathematics is only computational mathematics. Among the older generation of

mathematicians, this was definitely the opinion of A. N. Tikhonov and A. S. Kronrod. People working in computational mathematics would say that the community of pure mathematicians consists of practically insane people, speaking their own weird language, totally unintelligible to physicists and applied mathematicians, and they, the "pure ones", would soon become attractions in zoological gardens. Seeing all this, I reflected a lot and began to study nearby fields-mechanics and then theoretical physics. Other branches of mathematics, which were considered less abstract and more prone to applications than topology, did not give any answers to my questions: actually none of them had any serious relationship at the time with the natural sciences and the applications, as I discovered to my disappointment.

An even worse impression came from the problems of "theoretical applied mathematics", where terminology borrowed from the real world was used, rigorous theorems about things superficially resembling reality were proved, although these things were in fact infinitely far away from any reality. Only proving rigorous theorems was prestigious, and the more complicated the proof, the better; reasonable realism in the setting of the problem, as well as the result itself, were much less appreciated. Unfortunately, even Kolmogorov advertised "theoretical applied mathematics". In general he had a strange split personality: he would recommend the investigation of these questions to other people, while he himself studied the natural sciences-pushing a button in his brain, he would become a totally different person, far removed from pure mathematics, working on the basis of entirely different criteria.

I decided to spend some years studying theoretical physics. I began with quantum field theory, but soon understood that one should begin with the basics, not at the end. My decision was motivated by the great respect I had for physics. The lectures of Einstein, Feynman, Landau, and several other prominent physicists deeply impressed me. The clarity and simplicity of their exposition of mathematical methods were in sharp contrast with what one could read in texts written by mathematicians, with very few exceptions. I had first observed the natural way in which mathematical notions arise when in my youth I was studying topology at the peak of its development in the work of the most outstanding topologists; the complicated and deep algebraic apparatus seemed to appear easily and naturally from qualitative geometry and analysis, yielding a double intuition about the same objects. In physics similar traits became huge, incomparably more varied and dominant. It is not by accident that in the period of difficulties in foundations of theoretical physics in the 1980s and 1990s the quantum field community found a haven

precisely in topology. Besides the topologists, in my generation Arnold was one of the people who was leaning to this style of thought, and soon he was drawn to topology.

Remarkable mathematical beauty and an extreme level of abstraction were needed by physics to state the laws of nature; this level rose even further in the 20th century, but it is only now that physics has united all this with unbelievable practical efficiency resulted in technological revolution. I would say that in this period physics headed the progress of mankind, and mathematics followed alongside. The nuclear bomb, computers, the technological revolution, numerous technical miracles which have transformed the world around us, all this originated in the ideas and programs set forth by such leaders of the physical and mathematical sciences as Fermi, von Neumann, Bardin. Many physicists participated in this. Everyone knows A. D. Sakharov, whose contribution to the creation of the nuclear bomb became widely known after he became a dissident. In our country, M. V. Keldysh (my mother's brother) was one of the creators of the space program at its early stages. The Soviet authorities long kept the names of these people in deep secrecy, replacing them (not without the shortsighted participation of Keldysh) by the undeserving names of "pseudo-creators" for the West whenever one asked who was the leader during the world-wide hullabaloo of the 1950s and 1960s. Perhaps the authorities wished to mislead imperialists and conceal really important people from them, at least temporarily. Later the true names of the creators were stated publicly, but then it was too late—these names never became known to the international community; too many lies had been told before, and so many smoke screens had been produced that they no longer could be dispersed. Well, they have no one to blame: these lies were fabricated with their participation.

In our scientific community, however, these names were known then already, from conversations and rumors. Keldysh was highly respected. The Institute of Applied Mathematics (IAM) that he created had great authority in the USSR. In the 1960s the Steklov Institute, say, was regarded as a thing of the past, something redundant. Mathematicians should work together with scientists from other fields, and do pure mathematics in their free time. This was the point of view in that period of the most learned applied mathematicians, including Keldysh and Gelfand. And there was no anti-semitism at the IAM. The Steklov Institute appeared to be a loathsome monstrosity. Unlike the scientists from the mechanics community, those in the IAM kept a course bringing them to modern physics, perhaps not without Gelfand's

influence on the authorities.

All this was destroyed in the late 1960s by Brezhnev's political changes: the authorities were scared and angered by the "sins" of mathematicians. The IAM deteriorated completely. The Steklov Institute turned out to be more resilient: the administration there was also persistently quite odious, and the institute also depreciated, but has recently sprung back to life.

6. How is one to understand the mathematical beauty of Physics?

I was attracted by the beauty and power of physics. In 1965-1970 I systematically studied the entire course of physics textbooks. Except for two or three books (on statistical physics and quantum electrodynamics) I studied the books of the Landau-Lifshits series. Even earlier I had seen that the circle of physicists was not only richer than the circle of mathematicians in terms of science, but also more honest. This was the situation in the USSR, not in the West. L. I. Mandelstam's pupils A. A. Andronov, M. A. Leontovich, I. E. Tamm, and later his pupil A. D. Sakharov, despite their influence as leading physicists in applied theory, were regarded as models of decency in the country's physics and mathematics community, moreover, even in the entire scientific community in the USSR. Indeed, there was no one like P. L. Kapitsa among mathematicians. Subsequently, Sakharov became the model of decency in the whole world as well. The circle of Mandelstam's pupils was the circle of my parents' close friends since as far back as the 1920s.

In the same period the leading mathematicians of our country were talented but exceptionally amoral; I would even say that they lacked any conscience whatsoever. For example, the entire list of mathematicians who were full members of the Academy in the 1960s, whose honesty I would vouch for, consisted of my father P. S. Novikov, and also S. N. Bernshtein, L. V. Kantorovich, and I. C. Petrovskii, the latter being the only decent major administrator among mathematicians. People from Leningrad said that V. I. Smirnov was an absolutely decent person, but he was a mediocre mathematician and I didn't notice him. My brother, a well-known physicist in the field of solids Leonid Keldysh, letting out a cackle, told me in the early 1960s that formerly it was believed that mathematicians were divorced from life, but now it was said that a mathematician is surely a dishonest, first-rate crook. That was the kind of talk one could hear among physicists about mathematicians.

In the early 1960s, Leonid travelled abroad (to the USA) and upon returning told me confidentially that American physicists called the State Department in his presence, arranging for his trip somewhere in the USA, and were told: "We thought Keldysh was a lady." Apparently, they had in mind our mother Lyudmila Keldysh, a prominent expert in set theory and geometric topology, who had made a couple of trips abroad (not to the USA). The meaning of this remark, which amazed Leonid Keldysh, was obvious. He did not expect that Mstislav Keldysh, the president of the USSR Academy of Sciences and our uncle, would be absolutely unknown in the West as a scientist. The latter also discovered this subsequently, and it was a tragedy for him.

Returning to my main line of thought, I would say that in the first half of the 1960s the attacks on pure mathematics by applied mathematicians had not developed very far. One of the main reasons for this was the remarkable discovery of new particles by means of Lie group theory and related concepts. The whole new world of quarks, new concealed degrees of freedom in the micro world emerged. Considerable hope was pinned at the time on the theory of functions of several complex variables. In any case, physicists began to say once again that there were no laws of nature except for the laws of mathematics. They decided that it was necessary to step up the study of modern mathematical ideas. Applied mathematicians are like repair or construction workers; it is necessary to educate them so that they will become more literate in physics; whereas abstract contemporary mathematics is a real science and nothing can replace it. Heightened interest in Einstein's gravity and cosmology in the 1960s revived the need for Riemannian geometry; there was talk of making use of topology. All this postponed the crisis in society's view of mathematics for several decades. Mathematicians were appeased.

This was an important period for me. I saw it as an indication of the need to undertake efforts and study paths leading from mathematics to the natural sciences, and I began to study theoretical physics. The same efforts were undertaken by Ya. Sinai and Yu. Manin, and among topologists close to me, A. S. Schwarz. Each of us pursued his own aims and went his own way. It should be noted that none of the western mathematicians went along this way at the time (perhaps only I. Singer and later Alain Connes). In the West, ideology similar to "religious number theory" prevailed. Prominent and influential mathematicians in the western world of mathematics, for example, Andr e Weil, promoted the thesis that there is no need to turn to the natural sciences and applications, that it is possible to become a

great scientist without doing this, and that times had changed. This thesis certainly discouraged the part of the mathematical community that might have moved towards the natural sciences and applications. It is curious that such mathematicians as M. Atiyah, J. Milnor, and D. Mumford finally also completely rejected the ideology of religiously pure mathematics.

The community of pure mathematics in the West developed the following viewpoint: to earn money I teach mathematics at the university; this is my duty to society. The rest of the time I study my pure mathematics. They lived for a number of decades with that viewpoint. In our country it was not like that, this approach did not work: no one wanted to teach. Except for a small number of leading universities, conditions for those who were teaching were bad. The teaching load was too big, teachers could not even think of trips abroad, and there was no time for research. Be that as it may, the western community of mathematicians broke away from society further and deeper than ours did. Even in the brilliant centers of applied mathematics, say, at the Courant Institute in New York, with time the community increasingly came to understand applied mathematics as a set of rigorous proofs and questions of logical justification.

Gradually I adopted the following viewpoint: of course, mathematics or at least most of it, including contemporary abstract mathematics, is very valuable knowledge for humanity. But it is not so easy to put this value into practice. The leaders in mathematics must be literate in general science, must know the routes linking mathematics with the external world, must be able to seek new links and help younger mathematicians find their bearings. Otherwise I could not see how the internal achievements of mathematics would become useful for society. Mathematics should not be compared to music: the latter appeals directly to emotion and would be rejected if people were not to experience any emotions from it. One should understand that mathematics is a profession, not entertainment. In the past generations of mathematicians, there was always a community of leaders highly esteemed by the external world. Recall H. Poincaré, D. Hilbert, H. Weyl, J. von Neumann, A. Kolmogorov, N. Bogolyubov. Of the most prominent mathematicians of older generation, I had many conversations with I. M. Gelfand. He told me once: "In my youth I was worried whether the functional analysis that we developed was useful. Having worked in applications, I found the answer to this question and relaxed. But in dealing with physicists, do not delude yourself. Having discovered something valuable on the basis of your knowledge, which they do not have, you often find to your surprise that they

have reached the same conclusions on the basis of other considerations. The knowledge they possess should by no means be underestimated.”

In the process of study, I realized that theoretical physics, if learned systematically, from the fundamentals to contemporary quantum theory, is integral, extensive and profound mathematical knowledge, and is remarkably adapted to describing the laws of nature, working with them and effectively obtaining results. One cannot help but agree with Landau: to understand this, it is necessary to study his so-called ”theoretical minimum” entirely. This was the backbone determining one’s level of scientific culture. A person who has failed to learn it has a deficient idea of theoretical physics. Such people could turn out to be harmful for science; they should not be allowed into theoretical physics. Their influence would contribute to the downfall of education.

Unfortunately, the mathematicians’ community of the time did not acquire even the basic elements of this knowledge, and this was true even of those who called themselves applied mathematicians. For example, I quickly discovered that virtually none of the experts in partial differential equations knew precisely what the energy-momentum tensor was and could never clearly define the notion in mathematical terms. Among experts in mechanics, certain changes began earlier. A. Ya. Ishlinskii told me many years ago: ”Barenblatt and I made an error in our work in the 1950s, and a physicist pointed out the incorrect behavior of entropy on wave crests in our paper. Only after this did we seriously learn thermodynamics, the four potentials, Maxwell’s rules, and so on.” This means that the mechanics community had not studied such things previously. The best, most advanced experts in mechanics learned them in the 1950s and 1960s. But mathematicians did not learn anything of the kind even then.

I recently asked S. V. Iordanskii, M. A. Lavrentiev’s pupil, who subsequently became a fine quantum physicist: ”Sergei, please tell me what your teacher Lavrentiev, who considered himself a physicist but clearly did not know physics, thought of the Landau-Lifshitz textbook cycle. Did he also criticize them?” Iordanskii answered: ”No, he didn’t. He said that the authors knew special functions quite well.” Thus M. A. Lavrentiev, a talented mathematician trying to be objective, praised these textbooks, but failed to see any knowledge of theoretical physics in them. Or thought that nothing in them had any relationship to mathematics except special functions. Lavrentiev’s light-mindedness, disregard of the profound body of knowledge created by dozens of tremendous talents and repeatedly tested, the absence

of any awareness at all that this knowledge exists, had its consequences: his son, (*Editor's Note. The expression "Landau theoretical minimum", well known among Russian physicists, stands for the basic facts of physics anyone aspiring to do research under Landau was supposed to know quite well. These facts, roughly speaking, consist of the complete contents of the numerous volumes of the famous L. D. Landau-E. M. Lifshitz physics course. For more about this, see the interview with S. P. Novikov in this volume.*) for example, a decent administrator (as director of the Mathematics Institute in Novosibirsk) rejects the special relativity theory. Have no doubts about it, he grew up under the scientific influence of his father. Generally, although M. A. Lavrentiev had outstanding abilities, he was exceptionally irresponsible. Suffice it to recall how he accustomed everyone around him to heavy drinking, not realizing that they were much less alcohol-resistant and weaker than he was. After all, he could outdrink even the Communist boss Nikita Khrushchev. Lavrentiev's irresponsibility destroyed many fine things even in his own undertakings. I would like to say, however, that Lavrentiev and Petrovskii in 1960-1966 did enormously useful work in revealing Soviet mathematics to the world, and my generation owes this to them.

In any case, the 1960s were the period of the rise of my generation, in its first phase, when the older brilliant generation was still alive; many of the latter still functioned as scientists or administrators, while we had embarked on the first tour in the development of mathematics with great energy, and were preparing for the next one. As I mentioned before, some of us (Ya. Sinai, Yu. Manin, A. Schwarz, and myself) began to study different sections of theoretical physics independently of each other. At the same time, various groups of theoretical physicists began to move towards mathematics in different ways. An axiomatic trend appeared in quantum field theory, aimed at building a mathematically rigorous theory free of contradictions on the basis of contemporary functional analysis. It failed to achieve its aim, of course, but there did emerge a mathematically nontrivial cycle of rigorous studies in functional analysis with a beautiful algebraic and quantum field aspect. A number of experts in statistical mechanics (originating from physics) began to prove mathematical theorems. The case of E. Lieb was interesting: as we know, he began with brilliant papers, acknowledged by physicists, providing precise solutions of problems in statistical physics; he was always quite cognizant of physics research and made important contributions himself. Nevertheless, he chose the profession of rigorous mathematical physicist, and he was not alone. The community of contemporary mathematical physicists proving rig-

orous theorems came into being. Most of them had an initial education in physics. Essentially they became mathematicians. Ya. G. Sinai was heading precisely for this field when he studied theoretical physics-as a new field of mathematics, where rigorous theorems were proved. Except for Lieb, none of them studied exactly solvable models even in the past-this was another, different mathematical physics.

My own program was based on the deep-set desire to contribute to the borderline between contemporary mathematics and theoretical physics, founded on the ideas of contemporary mathematics-geometry and topology (including the geometry of dynamical systems), algebraic geometry and so forth. Could they be really useful, so to say, in practice? The growing computer boom was then obvious to everyone, gradually filling the natural sciences, applications and even pure mathematics, providing them with huge new possibilities, particularly in applications. But I could change nothing in this field, as I saw it: it would develop without me, becoming part of technology. As to introducing ideas from topology or algebraic geometry into physics, here I could produce ideas in such a way that no one could replace me. And physicists began to show a keen interest for contemporary mathematics at the end of the 1960s. Interaction with physicists at the Landau Institute (I. Khalatnikov, L. Gorkov, I. Dzyaloshinsky, A. Polyakov, V. Zakharov, L. Pitaevskii, G. Volovik, S. Brazovskii, A. Migdal...) proved to be fruitful. I acquired a great deal for my program as a result of this interaction, and also helped them in some ways. My pupils grew up in the atmosphere of that interaction (except for the first generation, which did not follow me in the study of basic theoretical physics, although some of the best, e.g., V. Buchstaber, contributed to applications). I think that A. S. Schwarz's goals and his program were not too far from mine, although we later "settled" in different fields. Schwarz did a lot for the development of quantum field theory as a new branch of mathematics, often not too rigorous, and close to geometry and topology. I sought to develop nontraditional methods (unfortunately, their assimilation involved difficulties for physicists), to solve some problems emerging in general relativity theory and quantum mechanics, contemporary physics of nonlinear oscillations, condensed media, the theory of galvano-magnetic phenomena, often competing against physicists. In some, albeit rare, instances, the new mathematics that arose in the 20th century was really useful. Hence, so were the new tasks of mathematics itself.

As to Yu. Manin, his program, as I see it, was completely different: without doubt, he was especially interested in the mathematical language and

the logic of theoretical physics. Generally he strived to make a contribution to the formalization of science. The tendency to study many varied things was generally his strong point—he liked to do it and knew how. We will deal with the formalization of mathematics below. It seems to me that it has an aspect that played an important part in provoking the crisis of the mathematical community.

7. Second half of the 20th century: excessive formalization of mathematics.

When I read the works of the 1920s and 1930s in set theory, I noticed that, despite the abstract subject, these works were written clearly and lucidly. The authors tried to explain their thoughts and do it as simply as possible. Studying topology in the 1950s, I saw that the best books and articles by famous topologists (Seifert-Trelfall, Lefschetz, Morse, Whitney, Pontryagin, Serre, Thom, Borel, Milnor, Adams, Atiyah, Hirzebruch, Smale, and others) were written very clearly. The subject itself was not simple, but no one wanted to confuse you further. The subject was expounded as simply as possible to help the reader understand it.

But then a different kind of source began to appear—for example, in my early youth, I saw that in the monograph of my teacher M. M. Postnikov, where his best works were presented, the content became burdened by unnecessary formalization, which made it difficult to understand. With time the quantity of such texts increased. This process went ahead especially rapidly if there was a great deal of algebra and category theory in them. For this reason the formalization of algebraic geometry proceeded at a higher rate. Topology held out till the end of the 1960s, when algebra and algebraic geometry were swamped by this kind of style. Then, in the 1970s, topology succumbed to this trend as well. And this coincided with a period of its sharp decline and loss of the propensity for general mathematical contacts.

Formal language is not transparent, it is always narrow and protects one's field from understanding by its neighbors and from the mutual influence of ideas visible to all. If one manages to borrow ideas from an adjacent field, it is possible to formalize them so that their source is no longer apparent. For some reason, there are many mathematicians interested in developing formal language, separating even very close areas of study and making them mutually incomprehensible. Perhaps many would like to be "first in their village" and shut themselves off with curtains from their neighbors, although this is probably not the only reason for which the mathematical community

at large has come to like formal language so much.

I do not have a complete understanding of the nature of this process, its motivating forces, and the causes of its broad social success. I think that this is an affliction accompanying unilateral, excessively expanded algebraic aspects: there should be a cautious, balanced approach to the process, without hiding the essence of the matter under it, so that it would be useful, and it is not easy to do this. On this point, my approach differed strongly from Manin's: in a number of instances, he acted in the new sections of mathematical physics as an ideologist introducing algebraic elements a la Dieudonne', making understanding artificially more difficult.

For example, in the 1970s, I began to actively promote different elements of theoretical physics at the mechanics and mathematics department of Moscow University. I discovered that simple, natural expounding of elements involved great difficulty: the capable audience of pure mathematicians of the department refused to even look at uncomplicated specific formulas of the classical type. For this reason, it was very difficult to explain the fundamentals, say, of general relativity or electrodynamics, or in general, elementary field theory: I had to force my students, who had to pass the course, to work through the fundamentals and get used to them. After that, things went more easily, but the rest of the students often left without listening to the end of the basic elements. Only a few managed to pass the course and understand. I was interested in the experience of my colleagues; inquiring, I learned that Manin had a different approach. He presented something extremely formal to the listeners and then told them, for example, that they had learned the Dirac equation. The public success was indisputable, students' eyes shone eagerly; but I did not want to follow this example: as experience showed, after such a beginning, it would be much more difficult for the students to learn what the Dirac equation really was.

There were a number of successes in soliton theory in the mid-1970s, in which I took part from the very outset in the role of initiator and then developed them with my best pupils (particularly B. Dubrovin and I. Krichever). At the time, contemporary mathematical physics began for the first time to use methods of algebraic geometry-algebro-geometric (periodic) solutions of KdV and its analogs were constructed. Manin soon wrote very formalized instructional surveys on the subject. Many young mathematicians inclined to algebra eagerly read them. The papers and books of those who created the field were written in a simple easily understandable language, the aim of which was the transparent exposition of the subject based on mathematics

nontraditional for applications in order to study and use the subject. But for younger people oriented to algebra, this seemed difficult and alien, and there were no clear criteria as to what was needed and what is the essence of the matter. They liked formalized texts where substance is not discussed, reading them like texts from their own field, abstract algebra. Actually, they learned nothing from these texts, as I see it, although they would think that they have learned everything useful. This is probably true of all young mathematicians who did not learn the fundamentals of contemporary mathematical physics. A Bourbaki-like text on mathematical physics is a double absurdity, since such a text also makes it difficult for physicists to penetrate these methods, creating the illusion among them that these branches of mathematics, which they have never studied before, are extremely complicated and inaccessible. As I noticed later, Manin wrote incomparably more clearly when he thought that he had done something important and wanted physicists to understand this as well; so I don't know if he remained an adept of formalization. However, at that time, such views held by certain authoritative scientists contributed to the spreading of this affliction.

It would seem that our field of science, contemporary mathematics, is already complicated enough. It is difficult to learn it. It would be natural to facilitate the study, making the exposition as transparent as possible. Formalization of a science's language in the Bourbaki style is not useful: it is not like Hilbert's formalization, which makes understanding easier. It is a parasitic formalization, making understanding more complicated, hindering mathematical unity and its unity with the applications. I believe that over-formalized literature appeared, in particular, because it was possible to predict its success among broad sections of algebra-oriented pure mathematicians. It is necessary to go against the current in order to retain a transparent general scientific style, which could secure the unity of mathematics and unite mathematics with physics and applications. This can only be done by a few mathematicians today. Today's community does not understand. Moreover, it refuses to listen to voices warning us about the need to overcome certain obstacles, if authoritative people appear who say that nothing of the sort is needed. "Give them what they want; they are incapable of anything else," is the optimal strategy to which the democratic evolution of abstract science and education leads, when people do not know if there is an aim for their research, and they refuse to discuss this question. All criteria are easily replaced if there is no aim to be achieved. Public success remains the only criterion. However, I would say to those few who could

overcome these obstacles that Bourbaki's writings prevent from finding the correct route and misinform them in today's chaos. Useless algebraic formalization of the language of mathematics, which complicates everything and obscures the essence of matters and relations between fields, is an illness that has become too widespread, and even if I have not referred to the best examples, it is a manifestation of the crisis leading to a certain meaningless functioning of abstract mathematics, turning it into a body whose limbs jerk haphazardly without any rhyme or reason. It was said that in order to stop the building of the Tower of Babel, God gave people different languages, and they stopped understanding each other. The construction halted.

Excessively complicated formal abstract language enveloped not only algebra, geometry and topology, but also considerable part of probability theory and functional analysis. Analysis, differential equations, and dynamical systems proved to be less susceptible to it. Several fine things were done here in the 1950s and 1960s, which subsequently became widespread and generally useful. But other absurdities plagued this entire community: expert mathematicians in these fields still continue a program which accepts only hundred-percent-rigorous proofs of theorems, proofs whose length has become unthinkable. Only a small percentage of them spent any efforts on self-education and learned to establish any contact with the world of natural sciences, where specific research is being carried on, without any regard for mathematical rigor. But even those mathematicians who do establish such contacts, as a rule, pursue only one aim: to learn some results obtained by physicists and engineers in order to begin to look for their rigorous justification. This is called "analysis", "applied mathematics", or "mathematical physics".

The passion for formalized rigor has gradually turned into mythology and faith, in which there is a considerable degree of self-deception: after all, who reads those proofs when they are too complicated? In recent years many cases were found when solutions of famous mathematical problems in topology, dynamical systems, various branches of algebra and analysis, as it turned out, had not been checked by anyone for many years. Then it would be demonstrated that some proofs were incomplete (see my paper in the volume of the journal "Geometric Analysis and Functional Analysis"-GAFA- devoted to the conference "Vision in Mathematics-2000", Tel Aviv, August 1999). And not in all instances can the gaps be eliminated now. If no one reads these "famous" works, what is the situation with complicated proofs in more ordinary papers? It is clear that the majority of them are

simply not read by anyone. I can understand that Fermat's last theorem and the four color problem, solved in the same period, are worth a long proof and have been checked. But it is simply preposterous to live in a world of exceedingly long proofs which no one reads. This is a road to nowhere, a bizarre end of the Hilbert program.

There is also another aspect of the question of rigorous mathematical verifications. In the natural sciences, rigorous mathematics requires a specification of the model that leads much further away from reality than non-rigorous physics; therefore, it leads to a less soundly verified result in terms of general science. This is another argument, in addition to the loss of control over proofs. Surely Hilbert himself would have already said, a long time ago, that there is no reason to continue this line.

The existence of a crisis in the mathematicians' community, with its educational system and approach to science, should be distinguished from the question: Is there a crisis of mathematics as a science? Perhaps there is no crisis; simply the best works in a number of fields are now done by other people, coming from physics?

In the 1970s and 1980s quite large groups of theoretical physicists, including applied physicists, became basically mathematicians. They did this to develop contemporary mathematics, providing it with a strong impulse. I will mention several such waves.

(1) The capture of computational mathematics by physicists. This natural process had been underway for a long time. It is clear to everyone now that a physicist will compute better in problems whose meaning he understands than a computational mathematician.

(2) The assimilation by physicists of certain fundamental set-theoretical ideas of the theory of dynamical systems which arose mostly before the 1960s, but have now become common property. The development of computer-based creativity on this basis.

(3) The fundamental role of physicists in creating a cycle of ideologically rich and new branches of mathematics, such as the theory of exactly solvable systems, classical as well as quantum: the theory of solitons and completely integrable Hamiltonian systems, exactly solvable models of statistical physics and quantum field theory, matrix models, conformal theories, super-symmetry and exactly solvable models of gauge fields.

(4) The influx of quantum physicists (first regarded as temporary) in such domains as algebraic geometry and topology, due to the pause in the development of the physics of fundamental interactions. The joint contribution

by physicists and mathematicians to this field in the last 20 years was quite considerable. If supersymmetry in the real world of elementary particles or something of the kind is confirmed, some of these people will return to real physics, as they believe.

(5) The influx of a large wave of quantum physicists into problems of mathematically rigorous justification of physical results. It is curious that this wave, which calls only the members of its own community "mathematical physicists", is separate from the communities where topology really develops or models are solved exactly. It includes people who have come to firmly believe in the ideally rigorous approach, Hilbert's program. Ideologically, these waves differ considerably: those doing nonrigorous pure mathematics call themselves physicists, while those who prove theorems call themselves mathematical physicists. This wave lies in the wake of the development of what mathematicians call "analysis". No doubt, the wealth of knowledge brought to mathematics by these people, as I see it, should be ranked higher than the analysis used by pure mathematicians who did not know contemporary physics. Nevertheless, spiritually, I am in favor of rigorous mathematical physics.

Frankly speaking, I should say this about my program: I have spent many years studying theoretical physics in the search for new situations where topological ideas could be useful in applications and natural sciences. The new topology that physicists created is a wonderful thing, but I have studied theoretical physics enough to know that this field is not a branch of physics; let those who studied nothing believe it is. Physics is the science of the phenomena of nature, phenomena that can be observed in reality. The Platonic physics is a set of ideal concepts separate from reality. A large group of talented theoretical physicists was fascinated by Platonic physics and imperceptibly moved far away from reality. In the last quarter of the 20th century, their belief that real physics, following the experience of the previous 75 years, would move on and confirm the most beautiful theories, ceased to be justified. Confirmation of supersymmetry in physics of elementary particles bogged down for 25-30 years. It still does not exist, although the supersymmetry hypothesis strongly improves the mathematical theory. Quantum gravitation and all its manifestations-strings and so on-are extremely far from any possible real world confirmation. At the same time, these theories proved to be so beautiful that they gave rise to many results and ideas in pure mathematics. The departure from real physics of such a talented theoretical community exposes physics and deprives it of the stratum capa-

ble of combining the realism of physics and state of the art contemporary mathematics.

In real physics itself, a number of fields has presently begun to be oriented not to the study of the laws of nature, but increasingly to developments of the engineering type. It seems to me that such a tendency also exists among the realistically minded mathematicians. This is not so bad in itself. Every age has its goals and aims. It would be important to make the overall achievements of the 20th century mathematics as accessible as possible, computerized to the utmost, including classical algebraic topology: this would help revive normal exposition, put an end to the presentation of this wonderful field as an abstract nonsense, which even mathematicians no longer understand and can no longer work with.

In referring to contemporary engineering-oriented trends, I would like to point out that society's desire to achieve success here leads to the rise of curious social phenomena. What are quantum computers? The possibility of developing the theory of quantum simulation of the computational process is interesting in itself, as a branch of the abstract mathematical logic of quantum systems. When we refer to creating computers, the first question that arises is: can we indicate a possible physical realization, or roughly estimate the values of numerical parameters that must be overcome for actual realization, estimate its possibilities, its speed. Without this, such an object exists only in Platonic physics. So far it is only possible to write novels on the subject *à la* Jules Verne. Pretentious talk about the omnipotence of technology in the future lacks substance: let us leave the future to future people; so far we simply don't know anything. No one knows if it is possible to build in reality a sufficiently large completely coherent quantum system capable of realizing classically controlled quantum processes according to the given rather complicated algorithm. The physics of such processes must be studied thoroughly. And if it turns out to be possible, will the calculation model based on these principles work better than an ordinary one in the real world? Do not get carried away comparing the number of steps—they are not the same here as those in ordinary Turing and Post machines. So far there is no visible engineering or physical idea here. There is only abstract quantum logic. Post and Turing machines were created at the same time as real computers; they are not like quantum computers, which do not exist. In this situation I cannot understand the raptures over problems allegedly already solved by means of quantum computers, such as decoding ciphers, something which both private companies and structures of the KGB and CIA

type need. I fear that KGB-like organizations will have to wait. Perhaps, this is related to the logic of promotion: why not raise a hue and cry and get money for research from these organizations? They have a lot of money, they even pay psychics. In any cases, in this field we do not see geniuses such as Fermi, who proposed the atomic bomb project that would eventually be supported even by Einstein. Things of this kind cannot be created without geniuses; people have forgotten about this. But much ado about nothing has become commonplace in the community of the late 20th century. However, to tell you the truth, I like this theory. This is a case when the ado might turn out to be useful, forcing mathematicians to finally learn quantum mechanics. And it is impossible to get money without making a lot of noise first. So let us hope that at least some success is achieved here.

In recent years, completely absurd anti-scientific phantoms have emerged. They have made (and continue making) a lot of noise. One of these phantoms is the story of the so-called biblical codes: some mathematics professors have "proved" with the help of computers that the Bible was not written by a person. Deeply believing that the Bible is holy, I insist on the viewpoint that every mathematical work, pure or applied, must be checked and analyzed in mathematical terms, regardless of its subject. The second phantom, also created by pure mathematicians, is A. T. Fomenko's pseudo-history developed at Moscow University. Here, world history and Russian history of antiquity and the middle ages was "rejected" also by means of applied mathematical statistics. These stories have the following in common:

- (1) The authors belong to the circle of respected mathematicians.
- (2) Their works are supported by a number of leading authoritative mathematicians.
- (3) The authors are incompetent in applied mathematics.

In both cases the mistakes are absolutely standard. No doubt these phantoms will inflict damage on the mathematical profession, ruining the reputation of mathematics itself in contemporary society. These phantoms and such like are indications of the profound crisis of mathematics, of its upper strata, an in-depth social misunderstanding of the interaction between applied mathematics and the real world, and the failure to realize the dangers inherent to the situation.

Earlier, in my younger years, I adopted the following viewpoint from my elders: activity in pure science does not free the scientist from his public duty to science; on the contrary, being financially and politically independent, leading mathematicians must defend scientific values from new versions of

Lysenko and all sorts of insane and ignorant people. The defense of scientific values is their duty to society.

Applied scientists have become too involved in material problems. If the upper strata of mathematicians are incapable to fulfil their duty, they are worthless. Fortunately, western mathematicians (including religious people) have finally spoken out concerning computer theorems on biblical codes. In Russia, I have not seen a single public expression of opinion, except for my own, by a mathematician concerning this pseudo-mathematical-historical nonsense. In the West, too, the relevant defense was organized by the scientists of an older generation, B. Simon and S. Sternberg, both closely associated with the ideology of mathematical and theoretical physics.

The natural desire to teach quantum field theory to mathematicians arose among physicists who came to study pure mathematics. Edward Witten organized something like "courses" at Princeton, which I think lasted for about a year. An excellent aim; I also tried to do this and even taught several of my pupils something; this was already mentioned above. Apparently, a few people have learned something thanks to Witten. One of my old friends, D. Kazhdan, a very good mathematician, only a few years younger than I am, learned, in particular, the fundamentals of field theory. He liked them so much that he subsequently began to popularize them; he delivered several lectures on the subject, including some in Maryland, where I work. Unfortunately, however, he gave the lectures in the formalized Harvard language. This made it more difficult for a broader circle of mathematicians to understand him, but that was not the only trouble. Even in his younger years, my friend had an extraordinary ability for learning complicated things, and he did it again now, in his older age. I suppose that two-three more first-rate mathematicians of the older generation also learned something from him. But where are the younger mathematicians? It would be a good thing if mathematicians learned the fundamentals of field theory up to quantum theory. Isn't it time, even in topology, to stop concentrating on results non-rigorously obtained by physicists and rigorously proving them? Isn't it time for us to grasp that collection of ideas and to guess new results ourselves? It is hopeless to try to do this in a formalized language.

The new analysis created by physics in the second half of the 20th century, analysis which still isn't rigorous in principle, must be accepted such as it is. It would be beneficial to write transparent, simplified textbooks mostly oriented to mathematicians, but a non-formal exposition must be agreed upon. There are no such textbooks yet, and the course to be taught should

be wider than field theory. How can this be done? Is contemporary western education suitable for the purpose?

8. Disintegration of education and the crisis of the physics and mathematics community.

Here we approach the key question, the main cause of the crisis in physics and mathematics, namely the disintegration of education. Will the still existing generations of competent mathematicians and theoretical physicists be able to teach equally competent young heirs for the 21st century? The key to everything lies in education, and the difficulties of the problem, symptoms of disintegration, begin in elementary and secondary school and continue at the university.

Back in the 1960s, both in the USSR and in the West, there was growing public criticism of the difficulty of school mathematics curricula, and the number of examinations began to be reduced. This probably had to do with the introduction of compulsory 10-11-year education. It then turned out that "for everyone" this was too hard to do, particularly to pass examinations every year from the age of 10 and to study mathematics. And of course there was a shortage of competent teachers "for everyone". Moreover, mathematical ideologues in a number of countries (in the USSR it was Kolmogorov) began to carelessly destroy established schemes of phased teaching of mathematics and introduced ideas of set theory "for everyone". Kolmogorov did a lot of useful work training the most capable children in special elite schools, but he introduced a lot of rubbish into general mathematical education. Be that as it may, society demanded that education be reduced and regulated, and there was a great uproar.

The situation in the USSR was aggravated by political complications and state antisemitism, as it were, particularly under Brezhnev. Education was made much easier, most examinations were eliminated. The gradual fall in standards began. At the same time, the educational level declined in mathematics and physics departments. This happened everywhere, but in the USSR there was also antisemitism, the rise of dishonesty among personnel (particularly at entrance exams) and the growing influence of certain dishonest "professors" little known in the world of science, as well as the advent of a new type of administrator: a person with high scientific honors despite the fact that he did not even write his own Ph.D. thesis, i.e., was actually never a scientist. Such was the process of disintegration of education and science in the USSR, and colleges and universities deteriorated much faster than

the Academy of Sciences, which retained its scientific image much longer. I would like to note that the world of science outside the former socialist camp is unfamiliar with the concept of 100 in the late USSR. All the former Soviet scientists are aware of this, can list a number of names in a private conversation, but, as I repeatedly observed, once in the West, everyone remains silent on the subject, even if they are working there permanently. I am also reluctant to mention names in writing-in fear of being sued, since nobody is going to subject these people to a test to find out their level. It is amazing what a high percentage of upper administrators in science and, to a greater extent, in education were actually like this in the late USSR. And these "phony prominent scientists" occupied posts that by right should have gone to serious scientists. Due to this, when the Iron Curtain fell, a broad section of capable and competent people, who had long felt uncomfortable, like a disinherited knight, left the country and lost all contacts with it. Colleges and universities inside Russia, as opposed to the Academy, hindered these contacts, so that the loss of this section for future Russia simply reflects the situation of disintegration that emerged in the late USSR. It would be possible to overcome the difficulties related to low wages: one could work in the West and return when the conditions are tolerable. What happened was much worse: from the very beginning it became clear that there was nowhere to return: nobody was waiting for you in Russia, all the positions having been taken by false scientists. Such was the process of disintegration in the USSR/Russia.

In the West, however, there was also an abrupt fall in the level of college and school education in physics and mathematics in the last 20-25 years, and in the USA the decline in school education was apparently particularly steep. I can clearly see that contemporary education cannot produce a theoretical physicist capable of passing Landau's theoretical minimum. The departure of a large group of talented theoretical physicists to mathematics will hardly be compensated by anyone. In mathematics itself, education provides much less knowledge than 30 years ago. The experts coming from the best universities of the West are narrowly specialized and know mathematics and theoretical physics haphazardly and much less than in the past. They have no chance of becoming great scientists such as Kolmogorov, Landau, Feynman, and others.

I would not like to discuss the details of the process that led to this result. In those years I could not observe the details of life in the West. In any case, democratic progress in education led to the same results in physics

and mathematics as the Brezhnev regime did. The conclusion is very simple: we are undergoing a profound crisis. Keep in mind that mathematicians and theoretical physicists also control the level of training of engineers in mathematics and physics-this is the basis for the competence of engineers. This means that disintegration is moving into the field of engineering as well. The fall in the level of mathematics and physics education in various computer based sciences is also obvious to everyone. These sciences are being reoriented to provide services for business and commerce. This in itself is not bad: if business goes up, the younger people will follow, this is where the big money is. But how is it possible today to educate a versatile mathematician and theoretical physicist? Even if it is true that these fields have become somewhat overdeveloped and can wait, the loss of the circle of people knowing them might turn out to be dangerous for humanity. Once this stratum is lost, it is very difficult and will take a long time to restore it; when the need comes, it might even be impossible. Under a certain turn of events, this may seriously undermine humanity's possibilities in technology, which may become vitally important in certain scenarios of evolution. Something has to be done. The purely democratic evolution of education, when people freely choose courses, works poorly in these sciences: the next level of knowledge must rest on carefully prepared previous floor, and there are many such floors. The entire building must be erected, not just stories piled up in disorder: the evolution that has occurred is like a natural thermodynamic process with an increase in entropy, with decaying quality of information in society. Centralized actions must be undertaken under the control of highly competent people. Physics and mathematics education is not a democratic structure in nature; it is not like a free economy.

It is believed that these fields will be revived if there are large-scale military projects. But this is only half the truth, and is not sufficient (if it will occur at all). When there is no longer a sufficient number of competent people, no amount of money will help. Thus, we are entering the 21st century in a state of profound crisis. It is not clear how to come out of it: the natural measures, which seem obvious, are practically very difficult or nearly impossible to carry out in the modern democratic world. Of course, we have entered the age of biology, which performs miracles. But biologists cannot replace mathematicians and theoretical physicists; this is a totally different profession. It only remains to hope that serious measures will be taken to save education in the mathematical sciences.

translated by A. SOSSINSKY