

Interview with Sergey P. Novikov  
*Interviewer: Victor M. Buchstaber\**

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**- Tell us, please, how you became a scientist. What was the role of your famous Novikov-Keldysh family?**

You are right, the family played a great role. My father, Petr Sergeevich Novikov, is a famous mathematician. All mathematicians know his works on the theory of algorithms and combinatorial group theory (including the so-called "word problem" and solution of the Burnside problem for periodic groups). He was one of the best experts in 30s in the so-called Descriptive Set Theory and in Mathematical Logic in 40s; he also started in 30s a new branch of mathematical physics: the reconstruction of homogeneous bounded domain from its gravitational potential at infinity). My mother, Lyudmila Vsevolodovna Keldysh, was a prominent mathematician, full professor, an expert in set theory and geometric topology. The family had five children, and I was the third of them, the youngest of the three sons. All sons became physicists and mathematicians. The daughters chose other professions. My elder brother, Leonid Keldysh, is one of the world-known theoreticians in the

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\*since 1996 Profesor Novikov works as a Distinguished University Professor in the University of Maryland-College Park, USA. He keeps also strong ties with Russia occupying part-time positions in Moscow: he is a principal researcher in the Landau Institute for Theoretical Physics and Head of Geometry and Topology groups in the Steklov Mathematical Institute and in MSU. Novikov is a Fields Medallist (1970). Soviet Authorities did not allowed him to attend corresponding ceremony in Nice, 1970 as a punishment for the letters supporting people who were arrested and sent to mental hospital. Novikov is a member of several academies including the Russian Academy, US National Academy and some other european academies, honorary member of the London Mathematical Society, doctor honoris causa of the universities of Athens and Tel Aviv. His works were awarded by several prizes in the former USSR. During the period 1985–1996 he was a President of the Moscow Mathematical Society succeeding Kolmogorov

quantum solid-state and condensed matter physics. The other brother, Andrei Novikov, was a good expert in algebraic number theory. Unfortunately, he died prematurely.

Add to this that the brother of my mother, Mstislav Keldysh, was also a very talented mathematician in the theory of functions of a complex variable and in differential equations. An especially fundamental contribution was made by him to applied branches of aerodynamics. He was a Chief Theoretician-adviser of the government and an organizer of computational work related to jets and space in 1940-1960s. He was a widely known person in the Soviet society. All information on the work of such people was classified and not reflected in the world press. M. Keldysh was the President of the Academy of Sciences of USSR for a long time.

His and my mother's father, Vsevolod Keldysh, was one of the leading building engineers in USSR. He was mentioned in Nikita Khrushchev's memoirs.

By the way, the mother of Vsevolod Keldysh (my great-grandmother), Natalia Brusilova, was an aunt of the famous Russian general known by his victory over the Austrians during the World War I (Brusilov breakthrough, 1916). The dodge leaders of bolsheviks (Lenin and Trotsky) used his military talent in their General Staff during the civil war.

Among close friends of my parents there were also leading soviet physicists of that generation: I.Tamm, a Nobelist, a teacher of A.Sakharov; M.Leontovich and A.Andronov, applied physicists that played an important role in the country; they were known in the Soviet physical and mathematical community as the "carriers of honesty".

Aleksei Andreevich Lyapunov, a pupil of my father, a well-known mathematician and a distant relative of even a more famous mathematician, was organizing a DNO (this is a Russian abbreviation of Children's Scientific Society) where his children together with my brother Andrei and me, and also Vladimir (Dima) Arnold and some other children of our family's circle got acquainted with the elements of sciences. A.Lyapunov especially took a great interest in branches of biology that were prohibited at that time.

Traditionally, high school students having a **strain of mathematics** attended university and participated in the so-called olympiads. I was successful in olympiads at the age of 13 and 14 and decided that I can probably become a mathematician. However, I postponed the choice of profession till the university. I decided to wait whether or not some other profession will attract me. It seemed to me that our family had already a lot of mathemati-

cians.

Mathematics as my profession **was clenched** only when I was already seventeen and entered the Division of Mathematics of the Moscow State University (MSU), the Department of Mechanics and Mathematics.

**- Amplify? the story, please. There are many legends about the famous Department of Mechanics and Mathematics of the MSU in the fifties and sixties.**

I heard in my family that one can grow up as a scientist at the special learning seminars by solving nonstandard problems, and studying things missed in the standard programs.

I began to visit such a seminar already at the first year. It was supervised by V.Uspenskii, a pupil of A.Kolmogorov in mathematical logic, who was at that time a young employee at MSU. At this seminar we passed the complete cycle of elementary problems in the set theory, the theory of functions of a real variable, and the algebra of logic. Soon Sasha (Alexander) Olevskii, my fellow at seminar, became well-known for his papers on the theory of functions of a real variable.

At the second year, at the age of 18, I was to choose a research supervisor for the first time and work with him during a year. Only after that, on the third year, a student was to make a final choice of his specialization. This order was in use during Kolmogorov's **deanery** at the Department of Mechanics and Mathematics.

I chose algebraic topology, in contrast to many friends of mine. I was probably attracted by the announcement concerning the seminar by M.Postnikov, V.Boltyanskii, and A.Schwarz. This announcement excited the **petulance** of P. Aleksandrov, the chief of the Moscow topologists, because the point-set-theoretical topology was put there in an unfavorable light. Anyway, I wanted to join new areas of mathematics.

I began to work in this seminar by choosing M.Postnikov as my research supervisor and attended brilliant lectures of Schwarz (who was a postgraduate student at the time) in which modern achievements of topology were presented according to perfectly written works of J.-P.Serre.

At the second and third years in University I deeply **mastered** a large layer of modern topological ideas together with a group of friends including D.Anosov. When I began the fourth year, the following situation occurred: my research supervisor M.Postnikov left Moscow for China for almost the entire academic year, and Schwarz did not get a work in MSU as a punishment on him for the announcement mentioned above.

We organized a seminar without any senior supervisors. D. Anosov, D. Fuks, G. Tyurina, and A. Vinogradov were among its participants. During this year (1958/59) I made my first scientific works.

**- Tell us about the atmosphere at the Department of Mechanics and Mathematics at that time and on its influence on forming you as a scientist.**

The main component of the atmosphere on the Department of Mechanics and Mathematics was a creative spirit, an anxiety to start the scientific activity as early as possible.

Completing the third year at the age of 20, I saw the following picture on the Department of Mechanics and Mathematics: those who had chosen areas around the theory of functions of a real variable were already authors of well-known scientific works. Some of them, who became members of the Kolmogorov seminar, succeeded to make even famous works.

In algebraic topology the situation was quite different, and, at the time, Moscow was not the center of this area. Practically I had to seek the way to start scientific research without any help of research supervisors, especially after the departure of Schwarz. The most difficult problem is to make the first scientific works. How to find a worthy subject?

**- How your first works were made? Among them there is a widely known Milnor–Novikov theorem on the cobordism rings.**

How to begin? This is the most difficult problem for a new **entrant** into the science, especially if one has no supervisor similar to Kolmogorov who could readily and quickly introduce his pupils into the very center of mathematics and indicate problems both deep and accessible.

My approach to this problem was as follows: one must take some new outstanding work that was still not learned in detail by the community, and try to learn it deeply making great efforts. If your effort are successful, then you will be at home in methods with which the overwhelming majority of experts in your area is out of touch. Do not doubt that in this case you will soon succeed in making something new. So did I learning remarkable works of Frank Adams and Rene Thom in 1958.

Bizarre homological calculations with special Hopf algebras were a quite remarkable area which was absolutely unknown at the time outside a narrow circle of algebraic topologists.

Tens years later, when the importance of Hopf algebras became well known to everyone, people even forgot who discovered these objects first. I inform: this was done by Armand Borel (rather than Hopf) in 1954, but

for a rather narrow purpose, namely, for the homology of Lie groups and H-spaces.

Hopf algebras of a new "Steenrod" type were discovered by John Milnor in 1957. In Adams' works, they began to give deep results in the problem of Hopf invariant. I was the first (as well as J.Milnor) who applied this technique in cobordism theory. I would like to note that, in the topology of the time, refined algebraic manipulations got tightly mixed up with topology of function spaces, cooperating with the geometry of manifolds, on the basis of fundamental ideas of transversality, cobordisms, and calculus of variations.

My first works were published in 1959–60 and became well known. The technique of Hopf algebras and new homological constructions were applied to the calculation of homotopy groups of spheres and of cobordism groups. The most significant results are the theorems on the calculation of rings of cobordisms. The very ideas of cobordism were greatly extended.

Already in 1960, after these works, I began to feel like a mature and substantive scientist and decided to devote my efforts to a new area, namely, to differential topology.

**- I remember very well the great impression produced over the audience by your report on the International Congress of Mathematicians in Moscow (1966). Is it related to topics of your first works? Are the Adams–Novikov Spectral Sequence and the Landweber–Novikov Hopf algebras a development of your first subject, isn't it?**

Yes, some years after I returned to this topic, reconsidering the whole system of methods of algebraic topology from the viewpoint of the complex cobordism theory in 1966–68. The **interlacing** of homological algebra with the geometry of manifolds adds a special elegance to this area. In particular, in collaboration with my student A.Mishchenko we began to apply the so-called "formal groups" in topology in 1967. This idea was soon picked up by Quillen (1969) who made an important contribution. In collaboration with V.Buchstaber (also my pupil) we started the idea of multi-valued formal group (1971). Later on, both the theory of these objects and their topological applications were far developed by V.Buchstaber. In collaboration with the group of my students and pupils V.Buchstaber, S.Gusein-Zade, and I.Krichever, we started another direction in which formal groups were intensively used to study finite and compact smooth transformation groups of manifolds, which gave an approach alternative to Atiyah–Bott–Hirzebruch analytic methods. These ideas were ahead of their time for decades.

**- However, as is well-known, you also turned to this topic later**

on.

Yes, and in the last decade you and me had an occasion to return to this area in connection with the theory of quantum groups and other algebraic problems. The store of algebraic ideas originating from complex cobordism theory is still far from being exhausted.

**- Let us return to the early sixties, to the atmosphere of the Department of Mechanics and Mathematics of that time.**

You are right, only having completed the first works I became actively interested in the scientific life around me at the Department of Mechanics and Mathematics around me and outside topology. Dima Anosov brought my attention to geometry and topology of dynamical systems. I began to learn this area. At the end of the fifties, Sasha Dynin (pressed by I.Gelfand) came to me to consult in algebraic topology in connection with the index problem for differential elliptic operators, where topological information helped him to make a good work. He was awarded by the prize of the Moscow Math Society for this work. The index problem became fashionable already at the time.

Thus, I began to learn partial differential equations and functional analysis with the help of my friends, especially Boris Mityagin.

In 1962 the famous work of Atiyah and Singer appeared, which increased the public interest to topology. The discovery of Smale (1961) significantly increased the role of topology in understanding complicated dynamical systems. The works of Grothendieck, Milnor, Hirzebruch, Atiyah brought topology and algebraic geometry together.

Since then, I began to study different areas of mathematics, by actively visiting seminars by I.Gelfand, V.Arnold, I.Shafarevich, M.Vishik, . . . In turn, they needed me for acquaintance with modern topology.

This was precisely the atmosphere of the Department of Mechanics and Mathematics of that time, that is, to teach one another new ideas and methods of various areas in the simplest and optimum way and by transparently explaining ideas to one another without artificial complexities.

It should be noted that, at the time, the Department of Mechanics and Mathematics of MSU represented all branches of pure and applied mathematics. I do not know any similar scientific community assembled at a single place in the West after the World War II.

**- Tell us, please, about your results on the classification of manifolds. What is the Browder–Novikov theory?**

Milnor's remarkable discovery of smooth structures on the seven-dimensional

sphere together with the classification theory of the homotopy spheres greatly impressed me. During the academic year 1960/61 I studied works of Whitney, Pontryagin, Thom, and Milnor. I would like to stress that all these works were written absolutely clearly. The completeness of proofs in these papers was achieved without **detriment** to understanding and without artificial formalization.

In summer of 1961 I met stars of world topology: Milnor, Hirzebruch, and Smale, who came to USSR at various conferences. The iron curtain gradually began to raise.

These meetings were of great importance for me. In these contacts I could see boundary at which all results of differential topology known at that time ended. After my meeting with S.Smale in the Steklov Mathematical Institute, where I was a post-graduate student, the local superiors began to regard me as a serious scientist.

Very soon, in the Fall of 1961 I managed to make a decisive breakthrough in the classification problem for simply connected manifolds whose dimension is more than four. My new result won a confidence; this work was acknowledged as the best mathematical work over the Academy of Sciences of USSR made in 1961.

Specifying the homotopy invariants of a simply connected manifold and the integrals of the Pontryagin classes over the cycles, you determine the manifold uniquely up to finitely many possibilities. The description of this finite set is rather delicate, and we do not dwell on this topic here: this set can be computed in terms of the homotopy group of the special "Thom Space of Normal Bundle" describing peculiar cobordisms of normal maps of manifolds of degree one. These maps are analogous to the birational smooth maps in algebraic geometry. They have remarkable topological property which is a basis of the method. This technique was independently discovered by Bill Browder in 1962 when solving another (but related) problem.

The calculations in concrete examples led me to interesting conclusions. For example, it follows from one of these calculations that the group of diffeomorphisms of the eight-dimensional sphere (to be more exact, the connected component of this group) cannot be contracted to the orthogonal subgroup.

The PL-classification of manifolds also follows from these methods. As far as continuous homeomorphisms are concerned, the problem must be discussed separately.

**- Let us now dwell? on your famous theorem on the topological invariance of Pontryagin classes and on the Novikov conjecture on**

### higher signatures.

Continuous homeomorphisms substantially differ from smooth and piecewise-linear ones. For instance, the property of a map to be a diffeomorphism is stable in the  $C^1$  topology. Any map sufficiently  $C^1$ -close to a diffeomorphism is also a diffeomorphism. This is quite untrue for continuous homeomorphisms of manifolds. Perturbing a homeomorphism in the  $C^0$  topology, one can obtain something very complicated (but certainly homotopic to the unit map).

When establishing the topological invariance of various quantities (homology and cohomology, Stiefel–Whitney classes, etc.), the classical algebraic topology proved stronger theorems claiming that these quantities are in fact homotopy invariants.

For simply connected manifolds the integrals of Pontryagin classes over the cycles are certainly not homotopy invariant. For this reason, the problem of their topological invariance (that is, the invariance with respect to continuous homeomorphisms of manifolds) occupied a special place in topology. By the way, the complete Pontryagin class regarded as an integral cohomology class is not invariant under PL-homeomorphisms yet (Milnor–Kervaire, 1962). The topological invariance is valid only for the integrals of these classes over the cycles that can be expressed via the Riemannian metric.

I managed to prove this conjecture in 1965 by using a sophisticated technique based on the entire ensemble of achievements of algebraic and differential topology.

The key idea invented in my work consists in a construction which is in a sense similar to the so-called “étale topology” of Grothendieck. Working with simply connected manifolds, I artificially introduced toric neighbourhoods of submanifolds and constructed differential topology in their coverings.

It should be noted that in subsequent works (of Kirby and others), where other known problems in topology of continuous homeomorphisms were solved, the toric constructions were also presented.

Thus, all integrals of Pontryagin classes over cycles turned out to be topologically invariant.

As a rule, they are not homotopy invariant. For simply connected manifolds there is only one homotopy invariant expression in Pontryagin classes, namely, the Hirzebruch formula for the so-called signature of the manifold—the important homotopy invariant characteristics of the cobordisms discovered in the early 50s by R.Thom and V.Rohlin. Its importance in topology, algebraic geometry, and the index theory of elliptic operators is well known.

I observed that, if the fundamental group is non-trivial, then there exist non-trivial cohomology classes with the following property: if one multiplies this class by the Pontryagin–Hirzebruch polynomial and integrates over the entire manifold, then the result is homotopy invariant. For instance, the products of one-dimensional cohomology classes have this property.

In the course of my proof of topological invariance of the Pontryagin classes I needed to establish special cases of the above statement.

My conjecture concerning products of one-dimensional cohomology classes was completely proved about 1967 by a number of authors (V.Rokhlin, G.Kasparov, W.Hsiang, and T.Farrell).

In 1970 I formulated the general conjecture that this property holds for any Eilenberg–MacLane cocycle that sits on the fundamental group, that is, comes from homological algebra.

The corresponding integral (over the entire manifold) of the product of this class by the Pontryagin–Hirzebruch class is referred to as a higher signature. My conjecture also includes the hypothesis that the higher signatures give a whole list of all homotopy invariant expressions of the curvature tensor.

During the last 30 years, this conjecture was studied in many works in which homological algebra got mixed up with the theory of infinite-dimensional representations and functional analysis. The conjecture was proved for hyperbolic groups, for discrete subgroups of Lie groups, and for some other special cases.

At the end of the sixties and at the early seventies I developed ideas of a peculiar algebraic analogue of symplectic geometry (I called it a “Hamiltonian formalism over rings with involution”). I thought that the explanation of the meaning of higher signatures and of some other deep properties of multi-connected manifolds has symplectic origin. However, researches of topologists and analysts in the problem of higher signatures used another way.

In 1971 I.Gelfand went into my algebraic ideas. They impressed him greatly. In particular, he communicated to me his new observation that the so-called von Neumann theory of self-adjoint extensions of symmetric operators is simply the choice of a Lagrangian subspace in a Hilbert space with symplectic structure. Many years later, in 1997, I used this idea in my works on scattering theory on graphs.

**- Did you work in topology of low-dimensional manifolds? Is it true that your classical theorem on the theory of foliations is in fact purely topological?**

Yes: in 1963, under the influence of people working in dynamical systems

(especially of V. Arnold), I developed the qualitative theory of codimension-one foliations on manifolds. My best results here are dedicated to the two-foliations of three-dimensional manifolds. In particular, I proved the well-known conjecture that every non-singular foliation of the three-dimensional sphere has a compact leaf. In fact I proved that a foliation on the sphere  $S^3$  always contains a tube, that is, a two-dimensional torus filled in on the inside by a special Reeb foliation. The complete set of these Reeb components is necessarily knotted.

It remains unknown till now what knots (links) can serve as a complete set of Reeb components of a non-singular foliation on the three-dimensional sphere.

I gave a topological classification of analytic foliations in the solid torus in terms of the tree-like sequences of conjugacy classes in the braid groups. Using the braids, H. Zieschang and me showed also that a non-singular foliation exists on any three-dimensional manifold (1963).

I also managed to find first non-trivial topological restrictions on a three-dimensional manifold admitting an Anosov system with continuous or discrete time. For example, in the case of discrete time my result was complete: the manifold must be homeomorphic to a 3-torus. The Anosov foliations arising here are not smooth in general. Therefore, direct topological methods are necessary.

It should be noted that my proof had a gap (indicated by Anosov) which was filled by my student A. Brakhman (1968).

**- However, you recently returned to three-dimensional topological problems coming from physics of metals, isn't it? What is the "Novikov problem" concerning the motion of electrons along a Fermi surface? How it is connected with real physics?**

In 1982 I paid attention to the fundamental geometric picture arising in the theory of metals. Each single crystal "Normal" metal has a so-called "Fermi surface" that is a level surface of a Morse function on a three-dimensional torus corresponding to the dual ("reciprocal") lattice. This torus is called the "quasi-momentum space." The only electrons that are of importance for electrical conductivity in the normal metals at low temperature are free electrons close to the Fermi surface. In a magnetic field, electrons begin to move along the Fermi surface. The trajectories of their motion on the universal covering  $\mathbb{R}^3$  look as intersections of the Fermi surface by planes perpendicular to the magnetic field. This dynamical system on the Fermi surface can be extremely complicated if the image of the fundamental group

covers the entire lattice. To this end, the genus of the Fermi surface must be not less than three. It is of interest that some metals (gold, copper, lead and platinum) satisfy this condition.

Many years ago, the well-known Soviet physicist I.Lifshitz and his school (M.Azbel', M.Kaganov, V.Peschanskii, and others) formulated the principle of "geometric strong magnetic field limit". It states that all essential properties of electrical conductivity in "reasonably strong" magnetic field are determined by the above dynamic system on the Fermi surface. In normal metals, this description works for magnetic fields that do not exceed  $10^3$  T ( $10^7$  Gauss). What is the geometry and topology of this dynamical system? Is this system useful or not? This problem was investigated at my seminar for many years since I started it in 1982: the works of my students A. Zorich, I.Dynnikov, and S.Tsarev helped to overcome the most important topological difficulties in 1984–1993.

However, only during the latest years A. Mal'tsev (who was also my student) and myself found out that the topological results can be combined in an extremely successful way and lead to physical conclusions. It turned out that the conductivity in a strong magnetic field either vanishes asymptotically or has a very special limit characterized by a triple of "topological integers". These three numbers are topologically stable, that is, they are preserved under small perturbations of the direction of the magnetic field. This picture holds for a full-measure set on the two-dimensional sphere of directions of the magnetic field. For exceptional directions for which this picture fails, the situation can be much more complicated. According to my conjecture, these exceptional directions form a set whose fractal dimension is less than 1.

There are beautiful and deeply non-trivial theorems of three-dimensional topology standing behind this picture. The observable triple of integers is in fact a two-dimensional homology class in the three-dimensional torus; this class is called the "carrier of open trajectories." It turns out that for these physical systems the competition between the topological complete integrability and stochasticity is **gained** by integrable systems for the overwhelming majority of magnetic fields. Only a set of small fractal dimension falls into the share of complicated stochastic systems; the investigation of this set requires deeper analytical and numerical tools and exceeds the limits of regular differential topology.

**- You mentioned researches — in metal physics — actively involving topology and nonlinear dynamics. I would like to ask you to elucidate? your experience in interacting between a mathemati-**

**cian and the community of experts in theoretical physics in more detail.**

Working with physicists, I made my aim to find new points of contact between this science and modern mathematics, to apply effectively the modern areas of mathematics that were not applied earlier outside the pure mathematics.

I was not interested in problems like rigorous mathematical justification of results already obtained by physicists. I managed to find applications of topology and nonlinear dynamics, of the analysis on Riemann surfaces (algebraic geometry), and of some nonstandard aspects of Riemannian geometry in the areas of theoretical and mathematical physics in which nobody expected such applications. I helped some prominent physicists (A.Polyakov, I.Dzyaloshinskii, G.Volovik, and others) to apply topology in the theory of Yang–Mills fields and in condensed matter physics.

**- Tell us, please, about your initial steps in new areas. How you managed to learn new subjects?**

The lack of serious interaction of mathematicians with modern theoretical physics was a weak point of the Department of Mechanics and Mathematics in the fifties and sixties.

I had an opportunity to get acquainted with analytical mechanics and with the theory of incompressible fluid at the Arnold seminar, and this was all what I could get in this direction at the Department of Mechanics and Mathematics.

I heard in the circle of I.Gelfand that quantum mechanics conceals a beautiful mathematics. I also heard from different people including my brother, N.Bogolyubov and some others about the great new science—the quantum field theory.

In the middle of the sixties, under the influence of the progress in the theory of elementary particles, the community of physicists **thirsted** for studying modern mathematics. In turn, I also had the desire to study these branches of theoretical physics.

I started from statistical mechanics and quantum field theory and quickly understood that no success can be achieved in this way. It is necessary to learn the material step by step from simple parts to complicated ones. In due time, this scheme was thought through by Landau. Together with E.Lifshitz they wrote a cycle of textbooks forming a high road for studying theoretical physics (though it is useful to study some parts by using also textbooks of other authors): it is necessary to begin from “Mechanics,” then to proceed

with “Field theory” and “Quantum mechanics,” and only after that it is natural to study “Statistical mechanics” and “Quantum field theory.” It is also quite good to learn “Hydrodynamics,” “Elasticity theory,” “Physics of continuous media,” and “Kinetics.”

Following this path during several years, I decided to begin an active interaction with physicists of the Landau school at the end of the sixties. For this purpose we made **common cause** with Ya.Sinai. Experts in theoretical physics heard something about topology and wanted to get acquainted with it. After a period of study I faced the same problem as that at the end of the fifties: how to start?

**- What was the topic of your first works in new areas?**

I was interested in Einstein’s general relativity. I was deeply impressed by the fundamental discovery of this area that our universe is far from being eternal and lives only 10-20 billions of years, being continuously expanding.

I.Khalatnikov asked me to make a careful analysis of their works devoted to the behavior of Universe near the cosmological singularity. There were some points here which looked like paradoxes. He was deeply worried about that and asked my help. They certainly formulated their results more general than it was necessary, but many physicists did not believe them at all at that time.

Together with my student O.Bogoyavlenskii, I wrote a series of papers dedicated to homogeneous anisotropic perturbations of the standard A.Friedman model of the universe. We managed to apply the qualitative technique of working with multi-dimensional dynamical systems; I took the informal ideas of these tools from my participation in the seminars of my friends at the Department of Mechanics and Mathematics in the sixties. You have to understand that mathematicians normally know more than they write in the papers: they choose for the written “theorems” only those pieces that could be rigorously proved, while we had to exceed these limits. Otherwise you will not be able to solve any serious physical problem.

It became clear that it is meaningless to consider the period of compression that preceded the current expansion: after a small perturbation, a compressing universe will enter with probability one the very complicated V.Belinskii–E.Lifshitz–I.Khalatnikov regime with singularity that cannot be extended anywhere. Our dynamical results confirmed remarkable mathematical properties of that regime discovered by the above mentioned authors, but led to conclusion that it is typical for the compressing Universe only. The expanding universe generically passes at the early stage much more regular

power-like regimes with probability one. However, our conclusion was that only a weak isotropization follows from the theory. The strong isotropization does not follow inevitably from the laws of the Einstein classical general relativity in any natural statistical setting with initial data at an early stage if the matter is in some state accessible to understanding of modern physics. After clarifying this question I stopped to work in General Relativity.

Unfortunately modern astronomical observations made in the late 80s lead to the conclusion that the universe became isotropic already at a much earlier stage (the "inflation" period) in which the matter was in a mysterious state. This certainly lowered the value of works devoted to non-isotropic cosmological models.

I stopped my work in this area in mid 70s, though my friends and colleagues (Ya.Zel'dovich, I.Khalatnikov, I.Novikov, and others) invited me to turn to quantum gravity together with them, because I could not believe that quantization of the Einstein's gravity is really necessary: the scales at which this quantization must take place are too fantastic and unattainable.

**- How adverted? you to the periodic soliton theory, where so many important results are due to you?**

Working in a community of physicists, in 1973 I got acquainted with remarkable mathematical ideas of soliton theory, namely, with the method of inverse scattering transform, which was discovered as a result of the joint activity of theoretical physicists, experts in computations, and of mathematicians in the sixties. This method successfully worked for the so-called solitons, that is, for solutions of the well-known KdV equation with rapidly decreasing initial data.

Is it possible to develop the corresponding analogue for periodic boundary conditions? I wrote several works dedicated to this problem in 1974. My discovery consisted in the close unity of the following branches of mathematics: the spectral theory of operators with periodic coefficients on the line, the theory of completely integrable Hamiltonian systems, and the analysis on Riemann surfaces (that is, algebraic geometry in the concrete analytical form). The main role in the construction of exact solutions of the KdV equation is played by the Hill operators or periodic Schrödinger operators on the line that have the remarkable finite-gap property.

As I proved, this property of the spectrum follows from a purely algebraic assumption that a periodic differential operator is "algebraic." This means that there is another differential operator which commutes with the given one.

The theory of finite-gap (finite-zone) operators was soon completed in collaboration with B.Dubrovin (my student) and also by V.Matveev and A.Its who actively joined the development of these ideas after my first work on this topic.

Soon (1975) a part of these ideas was independently found in USA by P.Lax, H.McKean, and P.van Moerbeke.

Already in 1974 I.Shafarevich brought my attention to the fact that these constructions lead to new results even in algebraic geometry *per se*, giving an explicit (uni)-rational realization of the entire moduli spaces of hyperelliptic Jacobians.

These methods can be transferred without any modification from the Korteweg–de Vries equation to all one-dimensional systems integrable by the inverse scattering method.

For spatially two-dimensional systems (such as the Kadomtsev–Petviashvili equation), the situation turns out to be very interesting. The development of this method, which was realized in this case by I.Krichever (1976–77), required the total algebraization of the procedure and the complete cleaning of it from the self-adjoint operators. You obtain solutions of the Kadomtsev–Petviashvili equation by using nothing but data from algebraic geometry.

This was a decisive step in the final understanding of connection between solitons and complex algebraic geometry. It made much easier for the pure algebraic geometers to enter this theory. At the same time, the purely algebraic method meets difficulties in applications: we face the problem to single out real solutions having physical or geometrical sense. For example, for the famous Sine-Gordon equation, some problems of the theory of real periodic solutions remained open until very recently. Problems of this kind can be solved easily and effectively within the framework of the theory of self-adjoint operators; however, this is not the case for the Sine-Gordon equation. P.Grinevich and myself made now a serious progress here.

Later on many works dedicated to the development of these ideas were done in my seminar. I.Krichever, B.Dubrovin, A.Veselov, B.Bogoyavlenskii, I.Taimanov, and P.Grinevich made important contribution among others.

**- List, please, the most fundamental directions which you developed here.**

I would like to mention the following directions:

– The inverse problem for a two-dimensional Schrödinger operator for a fixed energy.

We started developing this direction together with S.Manakov, B.Dubrovin,

and I.Krichever in 1976, but the most interesting soliton systems were found by A.Veselov and myself later on, in 1984 (the Novikov–Veselov hierarchy).

– The problem how to classify the ordinary linear commuting operators whose rank is greater than 1 and the deformations of holomorphic bundles over algebraic curves (Krichever–Novikov equations and the solutions of the KP equation of higher rank).

This theory was created in 1978–80 in collaboration with I.Krichever; P.Grinevich and O.Mokhov also took part in it. Difference analogue of this theory was developed quite recently by I.Krichever and myself. By the way, I.Krichever beautifully applied these ideas now to the investigation and generalization of the so-called Hitchin systems.

– Universal approach to the Hamiltonian formalism of systems integrable by methods of algebraic geometry, that is, the so-called algebro-geometric Poisson brackets.

This direction was developed by A.Veselov and myself in 1982–84. As it was observed several years ago by Krichever and Phong, similar symplectic structures mysteriously arise in Seiberg and Witten works on the supersymmetric Yang–Mills theory (for  $N = 2$ ).

– Analogues of the Laurent–Fourier bases on Riemann surfaces, namely, Krichever–Novikov bases and algebras, and the operator quantization of a bosonic string.

This direction was developed by Krichever and myself in 1986–90.

This is the list of directions in soliton theory that were developed by me together with my pupils and former students and in which methods of algebraic geometry were intensively applied.

**- Tell us, please, about your conjecture giving a solution of the classical Riemann–Schottky problem by methods of soliton theory.**

During several years in the second half of the seventies I closely examined the formulas of Its–Matveev type for solutions of the KdV equations and of Krichever type for solutions of the KP equation. They are of the form

$$u(x, y, t) = 2 \frac{\partial^2}{\partial x^2} \log \theta(Ux + Vy + Wt + Z; B) + \text{const}.$$

I asked myself: “Is this expression an effective formula, as is the case in classical analysis?” I decided that it isn’t.

We do not know in which cases the matrix  $B$  is determined by a Riemann surface, and this is precisely the Riemann–Schottky problem.

Moreover, the vectors  $U$ ,  $V$ , and  $W$  must be connected with the matrix  $B$  by complicated transcendent relations known to nobody.

How one can make such formulas efficient?

My idea was that one should initially regard the matrix  $B$  and the vectors  $U$ ,  $V$ , and  $W$  as independent variables. All relationships among them must follow from the requirement that the entire expression satisfies the simple universal KP equation. If this is the case, then we obtain a significant effectivization of the Theta-functional calculus and solution of the Riemann–Schottky problem.

The first important result in this direction was obtained by Dubrovin in 1981. It was considerably strengthened by Arbarello and De Concini (1984).

My conjecture was completely proved by Shiota (1986)

This approach, starting with Dubrovin, was repeatedly used in soliton theory for practical effectivization of Theta-functional formulas.

An analogue of this conjecture was investigated by Taimanov for the so-called Prym Theta-functions. In this case the KP hierarchy must be replaced by the so-called Novikov–Veselov hierarchy related to a two-dimensional Schrödinger operator.

**- Tell us, please, about methods of Riemannian geometry in the theory of systems of hydrodynamic type. What are the Dubrovin–Novikov brackets?**

I was interested in the hamiltonian structure of hydrodynamics since the early eighties under the influence of the Landau school: I.Dzhaloshinskii and I.Khalatnikov paid my attention to the old work of L.Landau (1940) whose idea they started to use in the condensed matter physics about 1980. It turned out that Landau knew these brackets already in 1940 when trying to quantize a fluid. Two directions of my scientific works actually arose from this source of ideas: the multi-valued calculus of variations and the theory of systems of hydrodynamic type.

First order quasi-linear homogeneous systems of differential equations (systems of hydrodynamic type) were studied since Riemann in connection with hydrodynamics of compressible fluid. We investigated the following problem with Dubrovin: when such a system is Hamiltonian?

Physical systems of this kind usually arise when describing an viscosity-free fluid. It is natural to expect that they must be Hamiltonian. We need not mix up this problem with the possibility to represent a system as a subsystem or a quotient system of a Hamiltonian system because this is always possible.

We introduced a class of “brackets of hydrodynamic type” (1983) which

gives a natural answer to the question. These Poisson brackets are based on Riemannian geometry. For many reasons, our discovery had various consequences. Many new important systems of hydrodynamic type arose in the seventies and eighties from soliton theory (Whitham 1971, Flashka–Forest–McLaughlin 1980, etc.) to solve asymptotic problems by using the scheme similar to the famous semi-classical approximation in quantum mechanics and optics. This scheme was already used by physicists (A.Gurevich–L.Pitaevskii, 1973). This semiclassical or slow modulation of parameters leads to the various "Whitham" systems of hydrodynamic type. For the integrable KdV-like systems the Whitham systems are diagonal as it was found in the work by Whitham, Flashka, Forest and McLaughlin. How to integrate them?

I formulated the integrability conjecture that the diagonal systems of hydrodynamic type are very close to the completely integrable systems if they are hamiltonian. Dubrovin and I proved for the most important cases that the Whitham Systems are actually hamiltonian. The most general statement about that was finally proved by my student A.Maltsev many years later.

The Integrability Conjecture was proved by my student S.Tsarev (in his PhD) even in more general form than it was formulated. He constructed a scheme of exact integration of diagonal Hamiltonian and more general "semi-hamiltonian" systems of hydrodynamic type based on Riemannian geometry (1985). Later on, in the second half of the eighties, as a result of a series of numerical and analytical researches in which V.Avilov, I.Krichever, and G.Potemin worked together with me, this technique was combined with the analysis on Riemann surfaces and the exact solution for the problem of dispersive analog of a shock wave was found.

In our works in the eighties, a lot of beautiful mathematics appeared, both in algebra and geometry, which was used later on by Dubrovin (in the nineties) in the topological two-dimensional quantum field theory. These ideas were successfully applied during the last years in the classical problem of orthogonal coordinates on flat spaces (B.Dubrovin, V.Zakharov, and I.Krichever).

Very interesting non-local extensions of the Hydrodynamic Type Poisson brackets were found by O.Mokhov and E.Ferapontov. They explain that the semihamiltonian systems of Tsarev are in fact hamiltonian in the nonlocal Poisson brackets. This statement is not proved yet in the full generality. As a continuation of that, A.Maltsev and myself developed a new theory of the "weakly nonlocal Poisson brackets" very recently. In particular, we clarified the structure of the so-called "higher" Poisson brackets and symplectic struc-

tures for the most famous completely integrable systems like KdV and NLS known since the late 70s.

I note that, generally, the appearance of a lot of different important Poisson brackets, the effectiveness of their usage, and the understanding of their role in modern science is one of achievements of soliton theory. After this theory it became clear that precisely the local Poisson structure is the major primary object in theoretical physics, while the symplectic structure is more preferable in pure geometry.

**- What is the multi-valued calculus of variations (the Morse–Novikov theory and the Novikov ring)? How is it related to the Wess–Zumino–Novikov–Witten quantum field theoretical model?**

The multi-valued calculus of variations has also roots in my Hamiltonian researches. Examining textbooks in hydrodynamics, I observed (1981) that the so-called Kirchhoff equation for the free motion of a rigid body in an ideal incompressible fluid is a Hamiltonian system on the Lie algebra of the isometry group of three-dimensional Euclidean space.

Apparently, this fact was not mentioned earlier. Besides that, V.Kozlov pointed out to me that the equations of motion of a top, that is, a rigid body in a gravity field with a fixed point, can also be represented in a similar form. I proved (in collaboration with the student I.Schmelzer) that this system can be reduced to the motion of a charged particle along the surface of the two-dimensional sphere (with some metric) in an external magnetic field with non-zero flux through the sphere. This situation is like a Dirac monopole! Certainly, the physical magnetic field is absent here; however, mathematically, a magnetic field turns out to be equivalent to the correction of Poisson bracket (symplectic structure) that occurs in the reduction of the system. Intending to develop something similar to Morse theory for periodic orbits on the sphere (that is, 2-tori of an initial system), I understood that the action functional of the above system on the sphere is not defined as a single-valued functional: only its variation is correctly defined as a closed 1-form on the space of closed paths. I was shocked by this simple fact and immediately understood its fundamental role in mathematics and theoretical physics. Neither the calculus of variations nor field theory discussed this possibility. As a corollary, I came to the following conclusions formulated in 1981:

1. In quantum mechanics and quantum field theory, a “topological quantization of the coupling constant” arises based on the requirement that the 1-form, the so-called “variation of action,” defines an integer-valued 1-cohomology

class on the space of fields (under appropriate normalization). This is necessary and sufficient for the Feinman amplitude to be a well-defined circle-valued functional. The Dirac's idea was different: he worked in the Schrödinger formalism and (in modern language) required that the magnetic field must be the Chern class of a line bundle whose sections give the Hilbert space of quantum states. This requirement implies integrality of the magnetic flux along any 2-dimensional cycle. For simply connected spaces these requirements are equivalent to each other. The new approach is much more convenient to generalize the Dirac monopole to quantum field theory. I gave a classification of all local Lagrangians of field theory that lead to non-trivial 1-forms. Later on it turned out that, when calculating anomalies in the Yang–Mills theory several years earlier, Wess and Zumino obtained a Lagrangian for the  $SU_3$ -valued fields which was a special case of those above but saw no analogy with the Dirac monopole and made no topological analysis. Several physicists — Deser, Jackiw, Templeton (1982), and Witten (1983) — came to the similar ideas in some examples soon after my work.

2. Multi-valued analogue of the Morse theory. It turned out that there is a specific analogue of the Morse theory for closed 1-forms on finite-dimensional manifolds; however, instead of usual cell complexes, the gradient of a 1-form generates complexes over specific rings, which were later on called “Novikov rings” and used by the symplectic geometers for the homology theory of Floer type, where the analysis is more complicated, but the heart of the problem is the same, namely, 1-forms occur instead of functions.

3. In collaboration with Taimanov and Grinevich we obtained a series of results on periodic orbits in a magnetic field. Some deeper observations are still not justified; there are interesting mathematical problems here.

**- What scientific and pedagogical ideas underlay the known book series under the common title “Modern geometry?” Do you think that this program is completed or you plan a further development of this series?**

Already many years ago, in the sixties, I had a plan to present “modern” topology created since the early fifties in textbook literature. In my opinion, topology is a great achievement of the XXth century mathematics. Till the second half of the sixties, the best achievements were reasonably presented in the original works, including those by H. Whitney, L. Pontryagin, S. Chern, J-P. Serre, H. Cartan, R. Thom, J. Milnor, S. Smale, M. Atiyah, F. Hirzebruch, . . . I always tried to follow their example, that is, to write long works that are as clear as possible. The topological achievements of that time could be learnt

by these works. However, a formalized abstract style unnecessarily complicating the exposition of topology appeared in math community during this period. It is very difficult to understand the very core of the subject by such texts. However, this style gradually began to fill in topology, and not only topology. Besides, starting from the late sixties, a series of the best works remained uncompleted by their authors (and by anybody in community), but it was not understood by a wide community — at least, no publicity was given to this fact long period. It became clear that there can be no serious development in an area which cannot be learnt and in which one cannot separate known facts from unknown ones. For years of work, I brought up many good pupils. They grew up in seminars at which training problems were solved and the key ideas of topology were studied. I gave a series of courses whose material was carefully and multiply repeated. Besides, the joint work with physicists in the seventies and the study of these areas of a science deeply induced me to think about the creation of courses that could serve to both communities, to mathematicians and to theoretical physics. A.Fomenko helped me to introduce the elements of geometry and topology to the students in mechanics. I created a plan of a series of books whose beginnings we had already, and also invited B.Dubrovin. In collaboration we wrote a series of books “Modern Geometry,” parts I, II, III. According to the plan, part I (geometry prior to topology) should bring to mathematicians the elements of natural sciences, create bridges to theoretical physics, and make them easy with geometry together with elementary ideas of “physical” field theory. Part II — the differential approach to topology — should be a course of topology, both understandable and useful, both for physicists and mathematicians working in different areas of analysis. A unification of topology with a world of other areas of science that are less abstract was the main goal of that part. Part III should serve as an introduction to the modern algebraic topology. The series should be continued, and part IV should be written to fulfil the plan to reach a frontline of the modern algebraic topology, but we had neither strength nor time enough.

Later on it turned out that the community of topologists and geometers was not too much interested in unification with natural sciences: part I was spread among the mathematicians not widely enough. Now I.Taimanov and myself are writing a new textbook that greatly modernizes the ideas of “Modern Geometry - I.” I hope it’s a good time for this topic to be studied by a broad community of mathematicians. Part II was more lucky, and quite broad groups of mathematicians and mathematical physicists includ-

ing many students studied this book. As to part III, it should be noted that, generally, the education in the area of “high” algebraic topology had badly fallen during the last thirty years, and for now the potential readers of part III are a rather limited community of very narrow-minded scientists (who prefer very abstract formal sources). I also wrote an elementary textbook together with Fomenko (The Elements of Differential Geometry and Topology, Kluwer-North Holland Publishing Company) and an Encyclopedia Volume “Topology - I” (English translation in EMS, Vol. 12, Springer 1996), which, in my opinion, clearly presents the entire cycle of ideas of classical topology up to the early seventies. Other my educational books ( Theory of Solitons, Moscow, Nauka, 1980, English Translation made by Plenum Press in 1984) written together with a group of physicists, and an Encyclopedia Volume under the title “Dynamical Systems, Integrable Systems, and Symplectic Geometry, vol 4” English translation in EMS, Springer, Vol. 4, second edition in 2001, the editors V.Arnold and S.Novikov) are devoted to absolutely other areas; their major problem is a rather broad introduction into a new technique: the solution of periodic problems of soliton theory and the spectral theory of operators that comes from the analysis on Riemann surfaces and algebraic geometry is one of the main examples. The algebraic geometers **aspire** to convert these methods into something abstract, while the scientists who effectively use classical analysis cannot study this without difficulty, and thus the process of assimilation is very complicated.

**- Tell us, please, about your views on the contemporary physical and mathematical education. Many people heard about your published and unpublished articles in which you speak about a crisis of physical and mathematical community.**

My experience, the long-term connection with education, tells me that an active process of reducing the level of physical and mathematical education began in the last decade (and possibly earlier). This reduced education cannot produce scientists of broad profile anymore. The process **gears up**. It is going to reach soon even the elementary school education in Russia as it already happened in US and in several countries of the West Europe. I wrote some articles on the subject. It is possible to speak even about a deep crisis of our areas of science. Recently I wrote a new article (in Russian) about this crisis. It can be found in my Russian homepage in Steklov through the Internet, while it is not published yet. It is a very serious problem which cannot be **elucidated** in two words. Theoretical physics falls most rapidly. Pure mathematics turns out to be steadier for now: though it also falls, it

still has more chances to survive and to keep remains of the past high level. It is possible that some fundamental achievements of the second half of the XXth century that are mathematical in essence but made by theoretical physicists, for example, modern quantum field theory, will be saved for the future as very parts of mathematics. However, mathematicians must at first learn these achievements.

I would like to write textbooks based on this assumption to prepare a mathematician to the elements of this area. It should be noted that learning this subject requires a very serious preparation, and one shouldn't begin without it. Many people do not understand this rule: everybody knows, for instance, that it's impossible to build a house starting from the tenth floor, but some people still do not apply this analogy to science. It is also absurd to try to present this totality of knowledge in a super-formal language — after that the material would become so complicated that nobody could learn it.

**- During more than ten years (1985-1996) you were the President of the Moscow Mathematical Society whose role in the creation of the famous Moscow mathematical school of the XXth century was spoken and written about many times. What you can tell about it? What is the role of the Society now?**

Yes, really, Moscow Mathematical Society, which long period was the mathematical face of the Department of Mechanics and Mathematics of MSU, is the main scientific arena of the mathematicians of the Moscow school, wherever they work.

Our society was founded already in the XIXth century, but it obtained the actual value as a consequence of the development of the Moscow mathematical school in the 1910s and 1920s, especially when the Presidents were N.Zhukovskii and D.Egorov, the founders of outstanding schools in mechanics and mathematics. The Egorov–Luzin mathematical school became particularly famous all over the world and determined the face of the Moscow mathematics of the XXth century. In Moscow, all mathematical activity of the twenties and thirties was concentrated around the Society, being interrupted sometimes by periods of hunting of the Soviet system for intellectuals, for example, during 1929–33. So it was till the mid fifties. After passing a difficult period of changing the generations, since the mid sixties, the Society win back the position of central forum of the Moscow mathematicians and successfully keeps it till now.

Let me list the surnames of Presidents during the last period: P. Aleksandrov (1933–64), A.Kolmogorov (1964–66), I.Gelfand (1966–70), I.Shafarevich

(1970–73), A.Kolmogorov (1973–1985), S.Novikov (1985–96), V.Arnold (1996–).

In the period of the late Bolshevism marked by severe anti-semitism as a state policy and persecutions of liberal intellectuals during the seventies in USSR, our Society faced great difficulties. Besides, in the eighties the connection with MSU was weakened, where an anti-scientific approach had prevailed, especially at the Department of Mechanics and Mathematics, the more so since the leadership of MSU began to disprove Einstein's general relativity, etc. However, we managed to keep our scientific face. After disintegration of USSR at the early nineties we performed a reorganization and intensified the connection with the Steklov Institute, where a process of recovery took place; much was done by us in the nineties to create and stabilize the Independent University. This work proceeds quite successfully during the last years.