My life in science.

This is not a narrative of my life. I will not talk about events of my life that were not directly connected to my work. I will talk only briefly about the relation of my papers to the work of other people.

I started as a topologist, later switched to mathematical physics or, better to say, to physical mathematics. (The notion of mathematical physics is not very well defined, but physical mathematics has the following definition given in corresponding section of Nuclear Physics B : Mathematical ideas which are of current interest for their potential application to theoretical physics. Of course in this definition physical mathematics is a time dependent notion. Prof. F. Hirzebruch told me that his work on topological questions of algebraic geometry was considered as very abstract, very far from any applications. Calabi-Yau manifolds also were regarded as a purely mathematical toy.)

I worked in many directions, applying tools from various branches of mathematics, changing my field of interest every 4-5 years. Let me list some directions of my research.

1. Geometry of uniform continuity. Volume invariant (growth) of a group (1952-1955).
2. Topological questions of the calculus of variations. Space of closed curves. Genus of fiber space. (1956-1962).
3. Duality of functors in autonomous categories (1962-1968, 1971).
4. Scattering matrix in quantum field theory (adiabatic definition, axiomatic theory) (1971-1974).
5. Particle-like solutions to classical equations of motion and quantum particles. Topological charges. Magnetic monopoles. Symmetric gauge fields (1975-1978, 1980).
6. Instantons. Quantum fluctuations of instantons (1975-1979).
7. Topological quantum field theories (1978-1979, 1987, 1996, 2000).
8. Topologically non-trivial string-like fields. Alice strings (1981-1982).
9. Geometry of supergravity. Field-space democracy (1981-1986).
10. Superconformal manifolds. Supermoduli spaces. Multiloop contribution to superstring theory, (1985-1991).
11. Mathematical problems of 2D gravity. String equation (1990-1991).
12. Geometry of Batalin-Vilkovisky formalism (1992-1994).
13. Noncommutative algebraic equations (1995, 1997, 2000).
14. Noncommutative geometry and its applications to string/ M-theory (1997-).
15. Maximally supersymmetric gauge theories (2003-).
16. Applications of arithmetic geometry to physics (2005-)

This list does not cover all directions of my work (for example, my papers on combinatorial invariance of Pontryagin classes and on topology of infinite-dimensional spaces are not covered); it includes only the fields that attracted my attention for significant periods of time.

My first paper (joint with N. Ramm) was written in 1952 under the guidance of Professor V. A. Efremovich and published in 1953. I was fortunate to have Professor V. A. Efremovich as my advisor-he was an outstanding mathematician and remarkable human being. Most of his work was devoted to the geometry of uniform continuity. In 1935 he proved that a map of metric spaces is uniformly continuous iff it transforms two sets with zero distance between them into two sets having the same property. This observation led him to introduce proximity spaces (spaces with a relation of proximity between two sets; in metric spaces this relation corresponds to zero distance.) Another axiomatization of the notion of uniform continuity was given at the same by A. Weyl who introduced uniform spaces. My first paper was devoted to the analysis of the relation between proximity spaces and uniform spaces. Alas, Efremovich's results were not published on time: he was arrested during the purges of 1937 and was freed in 1944 because he was considered terminally ill. Against the odds he survived and returned to mathematics. However, in 1949 he was fired from his Moscow job in a new wave of purges; this was the reason why he landed as Professor in Ivanovo. (300 km from Moscow where his family lived.)

My story was a little bit similar. My parents were imprisoned in 1937. My father was sentenced to 10 years without the right to write letters. It was discovered later that this sentence was a euphemism for death penalty. My mother was arrested as a spouse of "an enemy of the people" and after several years in prison camp was exiled to Kazakhstan. Even my grandmother was arrested; after a couple of months in jail she found me in an orphanage under a changed name. In 1941 my grandmother and I joined my mother and in 1948 we were able to leave Kazakhstan[1].

We settled in Ivanovo. There I met Professor V. A. Efremovich while still a high school student-he organized "mathematical circles" and olympiads. I entered Ivanovo Pedagogical Institute in 1951. (My application to Moscow University was predictably unsuccessful-I was a son of "an enemy of the people".)

By the time I graduated from high school I knew already a little bit of mathematics: calculus, elements of Lobachevsky geometry. Therefore in my freshman year I was able to study general topology and work in this direction. This was an ideal starting point from which Professor V. A. Efremovich led me to more geometrical questions. My first serious work was inspired by Efremovich's remark that the "volume invariant" of universal covering of a compact manifold is a topological invariant of the manifold. (If two compact manifolds are homeomorphic, then the natural homeomorphism between universal coverings is uniformly continuous. Efremovich proved that under certain conditions the growth of the volume of a ball with radius tending to infinity is an invariant of uniformly continuous homeomorphisms.) I proved that the volume invariant of universal covering can be expressed in terms of the fundamental group of the original manifold; in modern language it is determined by the growth of the fundamental group. I also gave estimates for volume invariants of manifolds with non-positive and with negative curvature. Thirteen years later J. Milnor published a paper containing the same results with the only difference that Milnor was able to use in his proofs some theorems derived after the appearance of my paper. At the moment of writing his first paper in this direction Milnor did not know about my work, but his second paper contained corresponding references. The notion of growth of a group (volume invariant of a group in my terminology) was studied later in numerous papers (one should mention, in particular, the results by Gromov and Grigorchuk). A new interesting field -geometric group theory- was born from these papers.

I am very grateful to P. S. Alexandrov and V. A. Efremovich for the possibility to spend several months at Moscow University, in the best Mathematics Department of Russia, remaining a student at Ivanovo. (The status of visiting student, not uncommon in the US, was unheard of in Russia.) In 1955 I graduated from Ivanovo Pedagogical Institute and was nominated for graduate school. (Without such a nomination I would be obliged to teach mathematics and physics in high school.) In the same year I was admitted to graduate school at Moscow University. I was very lucky-in 1954, after Stalin's death, my father was exonerated. Moreover, 1955 was the best year for a Jew to enter the University: the discrimination of Stalin's time was abolished, the new period of discrimination started later. My advisor at Moscow was P. S. Alexandrov, but at this moment I was already able to work independently (I wrote 7 papers in my undergraduate years) and he gave me complete freedom.

My years in graduate school were quite productive. I would like to mention my paper with V. Rokhlin containing a proof of combinatorial invariance of rational Pontryagin characteristic classes. (At the same time this result was obtained by R. Thom.) Most of my papers of this time were devoted to topological problems that appear in the calculus of variations. In one of my papers I studied topology of the space of closed curves on a manifold. This space is a infinite-dimensional orbifold (in modern terminology). The fact that it is not a manifold was not understood in preceding papers by M. Morse and R. Bott; this led to some errors. I corrected them and used new methods to study the homology of this space. (By the way, this space appeared later in papers by Sullivan and his collaborators in relation to string theory; he uses the name string homology for its homology. It was considered also in my paper about A-model and generalized Chern-Simons theory.) In another paper I introduced and studied a notion of genus of a fiber space; it generalized the notions of the Lusternik-Shnirelman category and of Krasnoselsky genus of a covering. The same notion was rediscovered (under another name) 25 years later by S. Smale and used to estimate topological complexity of algorithms. S. Smale found the simplest estimates of genus and applied them to estimate topological complexity of algorithm of solution of algebraic equation having degree n. His estimate from below was $(log_2 n)^{2/3}$. Later V. Vasiliev, applying my estimates of genus, strengthened this result and proved that the complexity is close to n. (If $n = 2^k + a$ where $0 \leq a < 2^k$ then the complexity is between $2^k - 1$ and $n - 1$).

[1] Millions of people were deported or exiled to Central Asia and Siberia. The situation of exiled people was worse than that of deported ones: they were convicted and their rights were restricted. However, after the end of their term they were free to leave; the deported people had no sentence, no term, hence no end of the term.

My results were based on methods of topology that were new at that time, especially on the use of spectral sequences. When I entered the graduate school Moscow mathematicians did not have working knowledge of these methods, I was one of the first Russian mathematicians successfully applying them. I gave numerous talks explaining these methods, later V. G. Boltyansky, M. M. Postnikov and I organized a seminar on Geometric Topology devoted mostly to applications of spectral sequences. The seminar was very successful; it attracted many brilliant young mathematicians. (First of all I should mention S. Novikov, who after receiving Fields medal for his work in topology, made very important contributions to integrable systems and mathematical physics.) The seminar announcement contained the following description of geometric topology: General topology studies, simple properties of complicated spaces, geometric topology studies complicated properties of simple spaces (manifolds, polyhedra). Experts in general topology, including P. S. Alexandrov, one of creators of this field, were insulted by this description. It seems that P. Alexandrov thought that this was my fault (although among the sponsors of the seminar were two Full Professors and I was a graduate student). I don't remember who invented the "criminal" sentence (the announcement was written by Boltyansky and me), but in any case we did not have any intention to insult anybody.

In 1958 I defended my Candidate of Sciences (=PhD) dissertation. At this time I was already an author of 15 papers; many of them were well known. (In a review book "Mathematics in USSR:40 years" published in 1957 my results were mentioned many times.) However, it was not simple for me to find a job after graduation. The main obstacles came from the KGB. It did not give me clearance in two places that wanted to hire me (the Laboratory for Theoretical Physics in JINR, Dubna, and the Division of Applied Mathematics of Steklov Institute.) I accepted an offer from Voronezh University. At this time the Department of Mathematics at Voronezh University was a lively place. It was informally headed by Professor M. Krasnoselsky, who created a very active group working in non-linear functional analysis and using topological methods in their work. In Voronezh I continued my work on the genus of fiber space that was close to the interests of Voronezh group. In 1960 I defended my Doctor dissertation in Moscow University; it was based on the work on genus. (A Doctor degree in Russia corresponds to German Habilitation; a Full Professor as a rule should have this degree.) I am very grateful to Krasnoselsky who convinced me that my results are sufficient for the Doctor dissertation and, which was more important, explained to me that I should become a Doctor if I would like to get an apartment from the university. The defense of the dissertation is not a final step in the Russian system; the dissertation must be certified by a special organization, the so-called VAK. In my case everything went smoothly, but several years later the VAK became a major tool in the fight against Soviet mathematicians of Jewish origin; I was very lucky that I did not postpone submission of my dissertation.

In Voronezh I worked also on topological questions arising in functional analysis. I analyzed the topology of several classes of operators. In particular, I calculated homotopy groups of the space of Fredholm operators; my results agreed with results of Atiyah and Janich, who studied this space from the viewpoint of K-theory at the same time. I proved that the embedding of finite-dimensional unitary group $U(n)$ into the infinite-dimensional unitary group is homotopic to zero and conjectured that the infinite-dimensional unitary group is aspheric; this conjecture was proven by Kuiper. I constructed homotopy invariants of non-linear Fredholm operators in Banach spaces and gave a talk about these results in 1961 at the Congress of Mathematicians of the USSR (Leningrad) but never published these results. I was convinced that they were incomplete: for operators of index 0 I obtained a residue mod 2 instead of an integer as an invariant. Moreover, Professor Yu. Borisovich informed me that Caccioppoli studied the degree of Fredholm operators of index zero in the thirties. Later S. Smale independently found constructions that were very similar to my work. He also did not have a complete picture (the construction of invariants in terms of oriented cobordism classes was invented later by Elworthy and Tromba), but he did not know about the paper of Caccioppoli and he published his results.

One more direction of my work in Voronezh was related to category theory. Being a graduate student in Moscow I advised several very gifted undergraduate students. A couple of them were willing to continue to work under my guidance after my departure from Moscow. I was reluctant to agree -at that time email did not exist, long distance phone calls were expensive. However, I agreed to supervise informally D. Fuchs' work on his PhD dissertation. As I understand now I was following the advice of one tutor that prepared high school students to university entrance examination: "To be a very successful tutor you should work with students that are able to pass the examination without your help". D. Fuchs found the topic of his dissertation by himself. Moreover, for some time I worked in the direction he started. Fuchs's goal was to understand Spanier duality in topology in terms of duality of functors. Together with D. Fuchs, B. Mityagin and my Voronezh students (V. Kuznetsov and R. Pokazeeva), I studied duality of functors in autonomous categories (in categories where the set of morphisms is equipped with the structure of an object of the category-we used the word D-category for this notion).

In 1960, during the week I spent in Moscow after the defense of my Doctor of Sciences dissertation, I met my future wife, L. Kissina. We married several months later. My wife was born in Moscow and did not want to leave her city. I convinced her to give it a try and to live in Voronezh for a year. She found that Voronezh was great, but it was a great village and Moscow was a great city. As I promised I started to look for a job in Moscow. One of our friends learned from physicists working in M E Ph I (Moscow Engineering Physics Institute) that they were not satisfied with the level of mathematicians there. This friend mentioned me as a young Doctor of Sciences looking for a job in Moscow. (Probably, I was at this moment the youngest Doctor of Sciences in the USSR.) In 1964 I was offered a position as Professor of Theoretical Physics at M E Ph I. The original idea of the physicists was that at some moment I'll be transferred to the Department of Mathematics to strengthen it. This never happened. Mathematicians were not eager to have me in their department, I enjoyed teaching various courses in Theoretical Physics and was eager to stay in the Physics Department and physicists were not disappointed with my work.

I was interested in physics for a long time. The possibility to work with physicists was very exciting for me. My first goal was to understand thoroughly the main physical theories. I thought a lot about the foundations of quantum mechanics, especially about measurement theory. However, conversations with physicists convinced me that these questions were not interesting for them and I turned my attention to other things. After years of work on the more concrete problems of theoretical physics I have also lost interest in foundations. There was some evolution in physicists' attitude to measurement theory; the modern view on these problems does not contradict my old ideas. (I have published some of my ideas together with Yu. Tyupkin in 1987.)

The last thirty years were marked by very intensive and fruitful interaction between mathematics and theoretical physics. However, in the sixties this process had only just started- in Russia slightly earlier than in the West. A handful of Russian mathematicians started to study seriously theoretical physics and to work on related mathematical problems; I belonged to this small group, that included F. Berezin, R. Dobrushin, L. Faddeev, R. Minlos, S. Novikov, Ya. Sinai.

My attention was concentrated mostly on attempts to understand quantum field theory. Together with my students V. Fateev, V Likhachev, Yu. Tyupkin I have written several papers on the scattering matrix in quantum field theory. We studied the scattering matrix in the axiomatic approach and the relation of an adiabatic S-matrix to the scattering of physical particles. I was not satisfied with textbooks in quantum field theory. Of course, I understood that there is no reason to require mathematical rigor from such a book. My main concern was that the main concepts, such as the notion of scattering matrix were ill-defined, that the rules of the game were changed in the middle of the game. Physics students were convinced that renormalization is necessary only to remove divergences and did not understand that it appears already in statistical physics where all Feynman diagrams are convergent. I have written two books devoted to foundations of quantum field theory. The exposition in these books was not rigorous, but it was based on clear definitions. These books published thirty years ago in Russian were not translated. At the end of eighties I have published a book "Quantum field theory and topology" containing a short introduction to topology and to its applications in quantum field theory. An English translation of this book was published by Springer Verlag as two volumes ("Gauge theory and topology" and "Topology for physicists".)

In the sixties topology did not play any significant role in quantum field theory. I continued to work on some topological and algebraic problems, but this work was not related to physics in any way. The relations between physics and modern mathematics changed drastically in the seventies. The change was prompted by discovery that soliton solutions of classical equations of motion are related to quantum particles and that topological considerations can be used to guarantee stability of solitons and corresponding quantum particles. I will not discuss in detail the history of this discovery that started with almost unnoticed papers by Skyrme. My interest in this subject was inspired by influential papers by G. 't Hooft and A. Polyakov who independently suggested in 1974 that classical solutions in one of the models describing interactions of gauge fields and scalar fields (the Georgi-Glashow model) can be interpreted as magnetic monopoles. In a paper joint with V. Fateev and Yu. Tyupkin we proved that the space of finite energy fields in gauge theories is disconnected in general and the components of this space are labeled by numbers that may be called topological charges or topological invariants of motion. Topological charges can be identified with elements of some homotopy groups; one can calculate these groups by means of homotopy theory. If the gauge group is simply connected and the group of unbroken gauge symmetries is one-dimensional then there exists only one topological charge and it can be identified with magnetic charge. This statement of our paper leads to a prediction that magnetic monopoles exist in any grand unification theory, i. e. in a gauge theory describing strong, weak and electromagnetic interaction at the same time.

Magnetic monopoles and other topologically non-trivial fields were considered in many of my papers with the help of homotopy theory, homology theory, characteristic classes and index theory. The joint paper with A. Belavin, A. Polyakov, Yu. Tyupkin (1975) was especially influential. In this paper we introduced and analyzed topologically non-trivial extrema of euclidean action in gauge theory (pseudoparticles in the terminology of our paper = gauge instantons in modern terminology). Instantons were studied later in numerous papers, including papers written by me (often in collaboration with my students V. Fateev, I. Frolov, V. Romanov, Yu. Tyupkin). In particular, I calculated the dimension of the moduli space of instantons by means of index theory and analyzed quantum fluctuations of instantons. A slightly different calculation of the dimension of the instanton moduli space was given by Atiyah, Hitchin and Singer. It seems that it was inspired by my preprint, that did not contain complete results, but their paper appeared after publication of mine (and refers to it). Both of these papers are devoted to consideration of instantons on the four-dimensional sphere, however, it was clear from the very beginning that moduli space of instantons on more general four-dimensional manifolds can be studied in similar way. Atiyah, Hitchin, Singer worked in this direction and their results formed a basis of famous papers by Donaldson on classification of four-dimensional manifolds.

I studied also one-dimensional topologically stable configurations in quantum field theory (topologically stable string-like objects). I have shown that such configurations appear in gauge theories if the gauge group is connected but the group of unbroken symmetries is disconnected and that a particle that goes around topologically stable string-like objects can change its type. The last statement found an experimental confirmation in condensed matter physics. It led also to the statement that in grand unification theories describing weakly interacting "real" and "mirror" worlds there exist quite unusual string-like objects (Alice strings) having the property that a particle going around such a string becomes a mirror particle[2].

Electric charge is not localized in the presence of Alice strings-one can say that it becomes Cheshire charge, remembering the Cheshire cat from L. Carrol's book. In principle Alice strings can be detected by means of astronomical observations; there exist some observations that can be explained by means of Alice strings very naturally, however, all of them have more complicated, but less exotic explanations.

I enjoyed my work on topological integrals of motions and topologically non-trivial solutions. My research in topology gave me an excellent background for this work and I was happy to work in a new and fast moving field. I gave several talks with titles like "Applications of topology to physics" and these talks were interesting both to physicists who realized there exists a new powerful tool and to mathematicians who discovered a new rich source of exciting mathematical problems.

I was even happier when in 1978 I found a way to use ideas from physics in topology. I realized that action functionals that do not depend explicitly on the metric should give rise to topological invariants. In lectures about these new ideas I talked about "applications of physics to topology". Of course, this was not a precise statement; however, the very fact that one could use ideas from physics to obtain topological results was fascinating. I have found that Reidemeister torsion (or, more precisely, its differential counterpart defined by Ray and Singer) can be obtained as a partition function of some metric-independent action functional. The gauge transformations for this functional were not independent (in today terminology the theory was reducible), hence it was impossible to perform quantization using Faddeev-Popov approach; I developed techniques allowing quantization of theories of this kind.

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[2] It is well known that reflection symmetry and even CP-symmetry is violated in the realm of elementary particles. It was conjectured that in a complete theory reflection symmetry is not violated; apparent violation of it is due to existence of mirror particles. We don't see mirror particles because their interaction with ordinary particles is very weak.

I understood very well that the idea to construct topological invariants. from metric-independent action functionals (such as the Chern-Simons functional) was quite general and it should lead to new interesting invariants; moreover, it was clear that this is also a way to find invariants of other structures (say, invariants of complex manifolds). However, at that time I was able to analyze non-quadratic action functionals only in semiclassical approximation or in the framework of perturbation theory. It was easy to write down perturbation series for invariants, but it was very difficult to analyze these series.

Much later, in 1987 V. Turaev told me that V. Jones constructed an invariant of knots that can be considered as a noncommutative generalization of the Alexander polynomial. One can express the Alexander polynomial in terms of Reidemeister torsion. V. Turaev asked me whether it is possible to modify the action functional I used to obtain Reidemeister torsion in a way that leads to the Jones polynomial. Pretty soon I came to a conjecture that the role of such a modification should be played by the Chern-Simons action functional; I talked about this conjecture at the topological conference in Baku. I was not able to prove this conjecture; the perturbative expressions I tried to analyze together with Yu. Tyupkin were too complicated (the difficulties arising in this way were overcome only in 1992 by Axelrod-Singer and Kontsevich). However, in 1988 E. Witten independently conjectured that Jones polynomial was related to Chern-Simons action functional and gave a (heuristic) proof of this conjecture.

My 1978 paper was a precursor to a series of remarkable papers where the methods of theoretical physics were applied to topology, algebraic geometry, symplectic geometry and other branches of mathematics. Many of those papers were related to topological quantum field theories (my paper gave the first examples of such theories). The axioms of topological quantum field theories were suggested by M. Atiyah who conjectured that Donaldson invariants and Floer homology can be obtained from topological quantum field theories. This conjecture was proven by E. Witten, who found a new way to construct topological quantum field theories. (I considered metric-independent action functionals; E. Witten has shown that many very interesting theories can be constructed if we allow BRST-trivial dependence on the metric.)

Today topological quantum field theories play an important role both in mathematics and physics. I returned to them several times (for example, in 2000, when I was invited to give a plenary lecture on TQFT at International Congress for Mathematical Physics). In my recent paper with S. Gukov and C. Vafa I gave an interpretation of Khovanov and Khovanov-Rozansky invariants of knots in terms of topological strings.

In the eighties I studied the geometry of supergravity (together with my students M. Baranov, A. Gayduk, O. Khudaverdian, V. Romanov, A. Rosly). This work led me to an idea that fields should by considered on an equal footing with space-time coordinates. Following the suggestion of E. Brezin I called this idea field-space democracy . Although explicit references to my papers on field-space democracy are rare the idea is alive. (In particular, field-space democracy was used by Townsend to construct action functionals of D-branes.) An unexpected consequence of my paper on field-space democracy was an invitation to attend a conference on parapsychology in Italy. (The organizers of the conference had a goal to present parapsychology as a science, therefore they included a section devoted to new ideas in the theory of space and time. A. Salam was listed as one of the speakers; perhaps, the organizers knew my name from him.) Of course, I did not attend the conference.

My work in supergravity was based on the theory of G-structures, in particular on the theory of CR-structures (Cauchy-Riemann structures). CR-structures were used later by A. Rosly to formulate theories in terms of isotopic analogs of twistors (isotwistors). I also worked in this direction together with A. Rosly . Rosly's idea of isotwistors (under the name of harmonic space) was used by V. Ogievetsky and his group (Sokatchev, Ivanov, Kalitzin, Galperin) to obtain very interesting explicitly supersymmetric formulations of some supersymmetric theories. Recently, I came back to this circle of ideas studying maximally supersymmetric gauge theories together with M. Movshev.

In the second half of eighties I worked on geometric questions arising in superstring theory (together with M. Baranov, I. Frolov, A. Rosly, A. Voronov). I introduced a notion of a superconformal manifold (this notion was introduced independently by D. Friedan and S. Shenker under the name "super Riemann surface"). We studied the moduli space of superconformal manifolds (supermoduli space), that appears in the calculation of multiloop contributions for superstrings. I would like to mention my paper about universal moduli space ; it seems that this space deserves further study. I talked about my results in this direction in an invited lecture at the International Congress of Mathematicians (Kyoto, 1990).

At this moment I have to interrupt the discussion of my work to talk about political changes in the Soviet

Union and about changes in my life. This was the time of Gorbachev's perestroika. All of my colleagues were fascinated by new possibilities and afraid of imminent dangers. For me, the first sign of new times was an almost successful attempt to go to Swansea. I was invited to give a lecture at the International Congress for Mathematical Physics and the KGB had no objections. I was excluded from the Soviet delegation by bureaucrats in the Academy.

I received many invitations to spend some time in the West; I could not accept any of them until 1989. In early eighties L. Michel told me that I had a standing invitation from IHES, but a new formal letter would be sent only if I have a chance to come. Before "perestroika" I was allowed to go abroad only twice-I visited Bulgaria in 1980 and Chechoslovakia in 1981. In 1988 I was able to attend a conference in Poland. There I met K. Gawedzki and asked him to inform the administration of IHES that the situation had changed. I never received the promised invitation letter. Later, in Italy, K. Gawedzki explained to me that usually IHES invited about 20 mathematicians from Russia every year and only a couple of invitations were accepted. That year all twenty invited scholars came; IHES did not have any possibility to send an additional invitation. I came to IHES for the first time in 1995. Later I spent many summers in this wonderful institute. IHES has extraordinarily strong permanent members; I was fortunate to collaborate with several of them (with A. Connes, R. Douglas, M. Kontsevich, N. Nekrasov).

In 1989 I was able to accept an invitation from the ICTP (International Center for Theoretical Physics) to spend 1 1/2 months in Triest. I was immediately invited to come back in July to participate in the Supermembrane Conference. I explained to Prof. Sezgin that there was no chance for me to be allowed to make another formal trip to Italy on such short notice, but that I could try to come as a private citizen. By my request Prof. Sezgin formally invited me and my wife to visit him in Italy. My family considered the attempt to come back to Italy in two months as completely hopeless, but things were moving very fast in the Soviet Union. On July 17, 1989 I left the Soviet Union-forever.

Very positive changes in the Soviet Union went hand in hand with dangerous developments. Gorbachev's economic policy was disastrous. Instead of radical changes on a small scale he accepted large scale half-baked reforms. As a result the economy, that was already in bad shape before Gorbachev, rapidly deteriorated. Nationalistic sentiments both in Soviet republics and Russia, suppressed under communist rulers, flourished in more democratic times; the government antisemitism declined, but the appearance of openly antisemitic organizations and newspapers was very disturbing. My children did not see any future for them in the Soviet Union. I decided to leave Russia fearing for my family.

I would like to express my sincere gratitude to all people that helped me to come to the US and settle here, first of all to D. Gross, A. Jaffe, J. Schwarz, I. Singer, E. Witten and B. Zumino.

I spent the academic year 1989-1990 in Princeton (in the Institute for Advanced Study) and in Cambridge (at Harvard and MIT). Since 1990 I have worked in the Department of Mathematics at UC Davis. This is now a very strong department; it was completely transformed due to the efforts of its chairmen, first of all A. Krener, then C. Tracy, M. Mulase , J. Hunter and B. Nachtergaele.

Russian science was concentrated mostly in Moscow and Leningrad; American mathematicians and physicists are dispersed over the whole country. Happily now, using email and telephone, scholars are able to collaborate even across the ocean; American scholars used this possibility earlier than scholars of other countries. I started such a "long distance collaboration" immediately after coming to the West. My first joint paper after leaving Russia was written together with A. Sen from India (our collaboration started when we met in Triest for a couple of days); it was devoted to boundary conformal field theory. I collaborated with V. Kac, H.S. La and P. Nelson, K. Anagnostopoulos and. Bovick, A. Tseytlin, M. Kontsevich, A. Connes, M. Douglas, M. Rieffel, N. Nekrasov, B. Pioline, S. Gukov, V. Vafa, V. Vologodsky and with many people from Davis: D. Fuchs, A. Penkava, O. Zaboronsky, A. Konechny, . M. Alexandrov, A. Astashkevich, M. Movshev, Yujun Chen, X. Tang, I. Shapiro .

I would like to thank all of my coauthors.

As usual I worked in various directions. One of my first papers in America was written in collaboration with V. Kac; we studied the partition function of 2D Gravity from the viewpoint of geometry of infinite-dimensional Grassmannian. More complete results in this direction were obtained later in my papers and in a joint paper with K. Anagnostopoulos and M. Bovick. In particular, I have studied in detail the so called string equation: $[A, B] = const$, when A and B are differential operators on a line [41].

Several of my papers were devoted to the geometry of Batalin-Vilkovisky quantization [32],[35],[37],[39].

I am convinced that the BV-formalism should be used not only as a tool, that allows us to quantize degenerate Lagrangians, but also a starting point in classical and quantum field theory. This idea together with the geometric approach to BV-formalism was applied in a paper [32] written jointly with M. Kontsevich and my students M. Alexandrov and O. Zaboronsky to give a geometric construction of a BV sigma-model containing many important theories as particular cases. Later M. Kontsevich used this construction in his famous paper on quantization of Poisson manifolds.

The geometric approach to the BV-formalism is based on the notion of a Q-manifold (=supermanifold equipped with an odd vector field with square equal to zero). This notion is closely related to the notion of an L_∞-algebra (see [32]); A_∞-algebras can be characterized as formal noncommutative Q-manifolds. Hochschild cohomology corresponds to A_∞-deformations and cyclic cohomology corresponds to deformations of A_∞-algebras with invariant inner product [33]. My recent work with M. Movshev on supersymmetric gauge theories is based on these relations.

My work on noncommutative algebraic equations started with a proof of "matrix Vieta theorem" in joint paper with D. Fuchs. We proved that for generic roots $x_1, ..., x_n$ of a matrix algebraic equation $x^n + a_1 x^{n-1} + ... + a_n = 0$ one has $\mathrm{Tr} x_1 + ... + \mathrm{Tr} x_n = -\mathrm{Tr} a_1, \quad \det x_1 \cdot .... \cdot \det x_n = \det a_n$. The coefficients $a_1, ..., a_n$ can be expressed in terms of generic roots $x_1, ..., x_n$; corresponding expressions can be considered as noncommutative analogs of elementary symmetric functions and our results can be formulated as a statement about these functions. We did not think that our work was related to physics in any way, but several years later physicists P. Aschieri, D. Brace, B. Morariu and B. Zumino came to the consider noncommutative algebraic equations when studying Born-Infeld type Lagrangians. My paper [12] was inspired by their work; it was based on another proof of the matrix Vieta theorem given by Connes and me . The papers on noncommutative algebraic equations are related to papers by I. Gelfand, V. Retakh and their collaborators []; all these papers belong to an emerging field that still does not have a good name.

The main direction of my work in recent years can be described as application of Connes' noncommutative geometry to string /M-theory.

The "second string revolution" of the nineties made clear that superstring theory should be embedded into a more general theory, that was christened M-theory. It was conjectured that M-theory lives in eleven-dimensional space; it should give superstrings after compactification on a circle and eleven-dimensional supergravity in its low energy limit. All existing superstring theories can be obtained from M-theory in some limits; all of them are related by dualities that can transform objects, that allow a perturbative description, into more exotic objects- branes.

A mathematical definition of M-theory is not known, however in 1996 Banks, Fischler, Shenker and Susskind suggested that M-theory can be formulated in terms of a matrix model. This model (the BFSS model or M(atrix) theory) is defined as a reduction of ten-dimensional supersymmetric Yang-Mills theory to (1+0)-dimensional space (all spatial dimensions are reduced to a point). I was very impressed by the BFSS conjecture;it gave me a mathematical framework to work with M-theory.

It is clear today that in some sense the BFSS conjecture is correct; however, the role of the BFSS model is not quite clear. I believe that the complete M-theory can be extracted from the BFSS model; it seems that my opinion is much more optimistic than the viewpoint of the creators of this model. It is possible, however, that the way from M(atrix) model to M-theory is very complicated and that there exist other, better approaches. (E. Witten and J. Schwarz independently expressed in conversations with me the same opinion: that it is clear now that M(atrix) model is correct, but it is not clear whether it is useful.)

When I started thinking about the BFSS model I realized that there exists another attractive matrix model that can be used to understand string/M-theory: the reduction of ten-dimensional supersymmetric Yang-Mills theory to a point. I found very soon that I was not the first scholar that came to this idea: this model was suggested by the Japanese physicists Ishibashi, Kawai, Kitazawa and Tsuchiya ; it is now known under the name IKKT model.

The BFSS model can be obtained from IKKT by means of compactification on a circle and Wick rotation. This remark led me to consideration of the general problem of compactification of the matrix model on a circle and, more generally, on a torus. This problem was considered already in the original BFSS paper. However, I found that the logic used in this paper and other papers leads to more general compactifications; they can be described in terms of Connes' noncommutative geometry. More precisely, these compactifications lead to noncommutative tori, that were thoroughly analyzed by A. Connes, M. Rieffel and other mathematicians.

In a joint paper with A. Connes and M. Douglas [25] we have shown how noncommutative tori arise very naturally in matrix models and what is the physical meaning of compactifications on noncommutative tori. This paper paved the way to application of beautiful theorems of noncommutative geometry proven by A. Connes and his followers to string/M-theory. Among numerous papers with applications of noncommutative geometry to physics I would like to single out the very influential Seiberg-Witten paper, that gave, in particular, a very clear explanation of the appearance of noncommutative geometry in the framework of string theory, as well as an analysis of the relation between gauge fields on noncommutative spaces to ordinary gauge fields and the proof of background independence of noncommutative Born-Infeld theory. (A little bit earlier background independence was discovered in my paper with B. Pioline [15] at the level of the energy spectrum.)

I also have written many papers developing the ideas of [25]. In [22],[23] it was shown that Morita equivalence of noncommutative tori is related to physical duality of gauge theories on these tori and to T-duality in string theory. I am convinced that the mathematical notion of Morita equivalence of associative algebras and its generalization for differential associative algebras should be regarded as the mathematical foundation of dualities in string/M-theory. Among other papers I would like to mention the paper with N. Nekrasov [24] where we introduced and studied noncommutative instantons and papers with A. Konechny, devoted mostly to the study of BPS-states on noncommutative tori and their orbifolds [11], [14], [16],[19]. Together with A. Konechny I have written a review paper "Introduction to Matrix Theory and Noncommutative Geometry", published in Physics Reports [5], [8].

My recent projects (together with M. Movshev) are devoted to maximally supersymmetric gauge theories [1]. These theories include ten-dimensional super Yang-Mills theory and its dimensional reductions, in particular, BFSS and IKKT matrix models. Our methods are closely related to noncommutative supergeometry, more precisely, to the theory of A_∞-algebras, that can be regarded as formal noncommutative Q-manifolds.

Recently I have studied two-dimensional topological quantum field theories coupled to gravity(=topological string theories), in particular, theories of this kind defined on bordered surfaces. I have applied methods of arithmetic geometry in this study. Together with M. Kontsevich and V.Vologodsky I have analyzed integrality of instanton numbers using p-adic methods. This paper and subsequent papers were based on the application of Frobenius map on p-adic cohomology; in a paper written in collaboration with I. Shapiro a construction of this map based on the ideas of supergeometry was given.

I am convinced that very soon the methods of number theory will play an important role in quantum field theory and string theory. In the paper entitled "Twisted de Rham cohomology, homological definition of integral and Physics over a ring" (with I. Shapiro) we argue that together with conventional physical theories based on real or complex numbers one can consider theories where the role of numbers is played by elements of an arbitrary ring. The study of such unconventional theories can be used to obtain interesting results in conventional framework, but it is possible that these theories have direct physical meaning.

I am planning to continue my work in this direction.

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