Mathematical Events of the Twentieth Century

Mathematical Events of the Twentieth Century

Edited by A. A. Bolibruch, Yu. S. Osipov, and Ya. G. Sinai

Springer

PHASIS

† A. A. Bolibruch

Yu. S. Osipov
Academy of Sciences
Leninsky Pr. 14
117901 Moscow, Russia
e-mail: osipov@ras.ru

Ya. G. Sinai
University of Princeton
Department of Mathematics
Washington Road
08544-1000 Princeton, USA
e-mail: sinai@math.princeton.edu

Originally published in Russian as "Matematicheskie sobytiya XX veka"
by PHASIS, Moscow, Russia 2003 (ISBN 5-7036-0074-X).

Library of Congress Control Number: 2005931922

Mathematics Subject Classification (2000): 01A60

ISBN-10 3-540-23235-4 Springer Berlin Heidelberg New York
ISBN-13 978-3-540-23235-3 Springer Berlin Heidelberg New York

Springer is a part of Springer Science+Business Media

springeronline.com

Printed in Germany

Set for publishing by PHASIS using a special LaTeX macro package
Cover design: Erich Kirchner, Heidelberg

Printed on acid-free paper 46/3142/YL - 5 4 3 2 1 0

Preface

Russian mathematics (later Soviet mathematics, and Russian mathematics once again) occupies a special place in twentieth-century mathematics. In addition to its well-known achievements, Russian mathematics established a unique style of research based on the existence of prominent mathematical schools. These schools were headed by recognized leaders, who became famous due to their talents and outstanding contributions to science.

The present collection is intended primarily to gather in one book the testimonies of the participants in the development of mathematics over the past century. In their articles the authors have expressed their own points of view on the events that took place. The editors have not felt that they had a right to make any changes, other than stylistic ones, or to add any of their own commentary to the text. Naturally, the points of view of the authors should not be construed as those of the editors.

The list of mathematicians invited to participate in the present edition was quite long. Unfortunately, some of the authors for various reasons did not accept our invitation, and regretfully a number of areas of research are not fully represented here. Nevertheless, the material that has been assembled is of great value not only in the scientific sense, but also in its historical context. We wish to express our gratitude to all the authors who contributed.

We hope that this collection will induce other authors to write their memoirs of mathematical events of the twentieth century which they witnessed and participated in. This will open a possibility for a continuation of the present edition.

We are very glad to thank Owen de Lange for his help in the preparation of the English edition of this book.

The Editors

P. S. A. A. Bolibruch played a very important role in preparing this book. His untimely death was a great shock and unrepairable loss for all people who were fortunate to work with him.

Heidelberg, 2005 Moscow, 2005

Contents

D. V. Anosov

Dynamical Systems in the 1960s: The Hyperbolic Revolution

Translated by R. Cooke

Probably everyone would agree that the theory of dynamical systems underwent a profound change during the 1960s, that its general features as then formed have been retained to the present time despite significant changes in content, and that this theory turned into a separate discipline at that time, having been up to that point a branch of the theory of ordinary differential equations. Here I intend to discuss certain events of this period, after which I shall say a few words about what came after.

The theory of dynamical systems consists of three branches, corresponding to the general character of the objects and questions it considers: differential dynamics – the theory of smooth dynamical systems, the ergodic theory – the theory of metric dynamical systems (in the sense of measure theory), and topological dynamics – the theory of topological (continuous but not differentiable) dynamical systems. [1]

[1] These explanations of the subject matter of each of these three parts, being rather general and brief, are not entirely accurate. Thus, in reality the first part also encompasses one-dimensional dynamics, including real dynamics, while the mappings considered in the last part are nonsmooth and even have discontinuities. Such failure of the subject matter of real one-dimensional dynamics to comply with the norms of the theory of smooth dynamical systems is compensated by their conceptual proximity. The picture is different for symplectic systems. They have not lost their

The rise of dynamical systems as a separate discipline was due mostly to two events in differential dynamics: the development of KAM theory (its seeds were planted earlier, but did not immediately receive wide development) and the "hyperbolic revolution." The chronology of all this is framed by the penetration of ideas of probabilistic origin into ergodic theory: the entropic theory arose (and immediately began to develop vigorously) in the mid-1950s. The Ornstein [2] theory (which within the limits of its applicability sometimes establishes an even closer connection of dynamical systems with the most ordinary random processes such as coin tossing than does the entropic theory, while at other times it points up subtle differences between them) arose in the 1970s.

It should be mentioned that two topics vary with particular vigor in the theory of dynamical systems. [3] In their "purest" form they occur in differential dynamics as quasi-periodicity, for which a certain regularity is characteristic, and as hyperbolicity, which is connected with those phenomena that are descriptively named "quasi-randomness," "stochasticity," or "chaos" (see the article of Yoccoz [1]). The use of spectral concepts and methods in ergodic theory, which began as early as the 1930s, can be regarded as a sort of implementation (in a suitable context) of the first topic. [4] The second topic found an adequate expression in ergodic theory just in the 1950s and 1970s, when the entropic theory and the Ornstein theory arose. Kolmogorov, whose name provided the first letter in the acronym KAM, was the only one at that time who made an equally large contribution to the study of both regular and chaotic motions (but in different parts of the theory of dynamical systems – the differential and the ergodic). His colleagues in the acronym exhibited a lively interest in chaos and of course interest on the part of scholars of such stature was bound to have consequences; but these consequences pale noticeably in comparison with their contributions to the study of regular motions. The same people who succeeded in distinguishing

former smoothness at all, but specifically "symplectic" considerations, questions, and so on, have increased to such an extent and have acquired such great importance both within the theory of dynamical systems and (mainly) outside it that symplectic dynamics, which previously belonged to the differential theory, now appears to have attained the status of a separate, fourth branch of the theory of dynamical systems.

[2] More precisely, the first papers by Ornstein on this topic appeared at the end of the 1960s.

[3] And not only in that theory. Kolmogorov was guided by similar considerations in a broader context when he allocated a significant portion of his papers into the first two volumes of his collected works.

[4] This by no means goes all the way back to the original source. A continuous spectrum, and even more, a Lebesgue spectrum is more characteristic of a dynamical system of "chaotic" type. But, because of its very origin in the analysis of sufficiently regular motions, the spectral approach does not suppress certain essential specific features of "chaos" (which, of course, does not make it superfluous or even unimportant).

themselves in "chaos" left no marks at all at that time in matters connected with regular motions. Only later was this "tradition" violated by Ornstein [2], [3] and Sinai [4], [5], but again, despite the importance of these papers, the authors do not owe their fame to them. All this goes to show that the differences between regularity and chaos do not simply lie on the surface, but are buried deep within, so that it is difficult for even the best specialists to switch from one to the other. Only in recent times have the contributions of a few brilliant representatives of French mathematics (Yoccoz, and Perez-Marco) in the two areas become more equally balanced.

The question arises: Is there some other "sufficiently substantial" class of motions that could occupy an intermediate position between the quasi-periodic and the hyperbolic motions (or, perhaps, lie somewhere to the side of both)? Could horocyclic flows and (or) nilflows (or, perhaps something of the sort that we do not yet know about) play this role? In any case, one would like to investigate what takes place in perturbations of these flows. Passing to discrete time, one could pose the same question in relation to pursuit mappings for nilflows. In the simplest case this involves perturbations of a mapping of the two-dimensional torus

$$(x,y) \mapsto (x+\alpha, x+y) \quad (x,y \bmod 1; \;\; \alpha \text{ irrational}).$$

For a start it would be good to develop at least a formal theory of perturbations (like the formal series in celestial mechanics, which may be divergent, but nevertheless contain significant information, being, in particular, asymptotic expansions of real solutions). As far as I remember, such questions were posed in the early 1960s by Arnold.

I was a witness to all these events from the late 1950s on and a direct participant in the "hyperbolic revolution." The memories of a participant, of course, are more complete than those of a witness; and I hope that their value is higher.

But first I must relate something of my life before the "hyperbolic revolution" began. I was a student, and later a graduate student, of Pontryagin, who had switched entirely from topology to ordinary differential equations and associated questions (variational problems of automatic control theory, and later to the theory of differential games, which was created by him, to a significant degree). It seems to me that Pontryagin's greatest achievements are nevertheless in topology, and that he was past his prime when I met him. But the beginning of the decline was very gradual, and the level was still very high. The only critical remark about this period of his activity, in particular, is not connected with his activity itself, but with a certain frame of mind that came over him:

he used to say that he was studying real problems, which he sometimes naively called "physical" problems. (What kind of physics is it in which no knowledge of any physical law is needed?) He made quite clear, although it wasn't said explicitly, that the others ... Those like Petrovskii, Bogolyubov, or Gel'fand, who really had come into contact with applied problems of great importance could not, or almost could not, talk about it at the time, but what did they think? Still, outwardly all seemed to be well. I remember only one critical remark about Pontryagin's attitude. It was made by Kolmogorov, who had also worked with physics and certain applied problems (not the same ones that the three named above had dealt with). He said that for ordinary differential equations there is an excellent intuitive picture – the geometric picture of the behavior of the trajectories in phase space (Andronov, as is known, had given it the expressive name of the "phase portrait"), which holds so much information that nothing else is needed; but for partial differential equations and the like, one really must understand the physics that they are connected with.

I was far away from these exalted spheres, but I formed the impression that Petrovskii was not overjoyed by Pontryagin's new research. I cannot judge to what extent he resented the appearance of a new competitor in this area, where he had reigned alone previously (besides, he had no more time for research, having become rector of Moscow University; the best he could do was to keep up with what was happening in mathematics), and to what extent he sensed Pontryagin's shortcomings, which had so far only slipped out innocently in conversations, but were later to manifest themselves more seriously. But I will nevertheless venture to suggest that the former was the main factor at that time. Pontryagin himself, after giving a revised mandatory course of ordinary differential equations and writing the textbook for it,[5] ceased to apply great efforts to overcome Petrovskii's cautious attitude. But he got most of what he wanted: any presentation of that course anywhere in the world on a serious level is now done in the spirit of Pontyragin.[6]

[5] As it happened, this course was given for the first time when I was a student, and we received a mimeographed edition of the textbook (slightly edited later, of course) at the end of the academic year – it was a sort of gift for us. Things were not then what they are now, when you can write everything you want on a computer and publish it on a printer. The process then was incomparably more drawn-out and required more work, along with the necessary administrative approval. I too made notes for my lectures, but by the time the course was over all I had left (even in the present computer-printer age!) was a few fragments. A more systematic presentation was worked out (if at all) only later, sometimes much later. For that reason I can only express my amazement at Pontryagin's capacity for work and his sense of responsibility.

[6] In the USA the textbooks of Lefschetz and Hurewicz, which have a closely similar approach, had appeared earlier, but for some reason had not become so universally used. Lefschetz later rewrote his textbook and made it into a voluminous graduate-level text.

At Pontryagin's direction, I became familiar with the mathematical sections of the first edition of the *Theory of Oscillations* by Andronov and Khaikin while still in my second year. (I learned only later that the book had a third author, Vitt, and the reason why his name was not mentioned in the first edition.) They made an extraordinarily strong impression on me — I would say that no book that I have read since has made such an impression on me. I think there were two reasons for that. First, it was the first time I (being a student!) was reading not a textbook, but a monograph written by scholars, one of whom was a leader in his field and the other of whom possessed unquestionable pedagogical talent. (The talents of the third author did not get a chance to develop, but judging from what he managed to do, they also were considerable.) Second, the two-dimensional qualitative theory produces a strong impression all by itself, even at first acquaintance.[7] The mathematical sections of the *Theory of Oscillations* could no longer make such an impression on the new generation of students, because the elements of the two-dimensional qualitative theory soon found their way into many textbooks and lecture courses, after which the *Theory of Oscillations* became for the students merely a more complete exposition of a subject whose main parts were already familiar to them.

At that time a group of (relatively) young mathematicians formed in Moscow — Boltyanskii, Gamkrelidze, Onishchik, Postnikov, Shafarevich, and Shvarts (at one time Dynkin also belonged to the group) — who were familiar with the new French algebraic topology. (The application of auxiliary fiber spaces to solve homotopy problems, which became possible thanks to the spectral sequence, was much admired at the time.) When I was a third-year student, this group organized special-topics courses and seminars on the new topology.[8]

[7] Before Pontryagin this theory was barely mentioned in the mandatory course of ordinary differential equations. One could have learned about it from the collection of Poincaré's papers "On curves defined by differential equations" (published on the initiative of Andronov), which contained detailed commentaries, and from *The Qualitative Theory of Differential Equations* by Nemytskii and Stepanov. I naturally became acquainted with the first only some years later, due to its overly specialized nature. The second, despite its unquestionable wealth of content (for that time), has always bored me quite as much as the *Theory of Oscillations* has interested me.

[8] In the advertisement for students (written by Shvarts?) it was said, in particular, that one could study either the simple properties of spaces of complicated structure or the complicated properties of spaces of simple structure. This phrase annoyed Aleksandrov, who by that time had switched over completely to general topology. However, he did not resist the new trends so much as he simply did not cooperate with them, striving to preserve a place for general topology. Several years later he said at an all-Union conference on topology in Tashkent that in the opinion of Kolmogorov, a scholar with great breadth of insight, algebraic and differential topology despite their remarkable achievements, had not yet attained a general-mathematical significance as wide as that of general topology. In a certain sense Kolmogorov was right — you encounter continuity much more frequently than, say, any bordisms. But of course in real life you encounter addition of positive integers even more often.

I was one of those who responded enthusiastically to the invitation of B-G-O-P-Sh-Sh. It must be admitted that our teachers had thought out and executed the organization of our education splendidly.[9] Not all of us remained in science, and of those who did remain, not all went on to work in the area of algebraic and differential topology or in the associated algebraic geometry.[10] But I think that the culture acquired did no harm to any of those who remained.

Smooth manifolds played no particular role at first in our education. But in 1955 (that is, simultaneous with the beginning of our topological education) Pontryagin's book *Smooth Manifolds and Their Application in Homotopy Theory*, the first chapter of which was the first textbook treatment of smooth manifolds[11] anywhere in the world, was published; and when I was in the fourth year, Gamkrelidze organized a seminar with a "smooth" orientation. Familiarity with the material to which Serge Lang later gave the happy name of "the no-man's-land between the three great differential theories" (differential topology, differential geometry and differential equations) was to be of special importance for me. I learned rather quickly how to think in invariant terms (that stand for the corresponding concepts) and "bring into general position using small displacements" (based on Sard's theorem[12]) I would add that later on (when I was a graduate student) Novikov and I read the monograph of M. Morse *Calculus of Variations in the Large*. Each of us, in his own way, found the knowledge thereby acquired to be of great use later on. At the time, the most important thing to me was not calculus of variations in the large, but the fact that I saw how a far-reaching mixed analytic-geometric theory on manifolds was being developed.

I now return to dynamical systems. Having learned about structurally stable flows in a planar domain from the *Theory of Oscillations*, and having some idea of smooth closed manifolds, I naturally began to reflect on structurally stable systems on n-dimensional smooth closed manifolds. These reflections were naive. I thought my way through to the same two conjectures which many others have undoubtedly reached, but only Smale dared to publish. They amounted to

---

[9] A large role was also played by the collection of translations *Fiber Spaces*, which was published somewhat later, in 1958.

[10] This last is also true of our teachers of the time.

[11] The book *Differentiable Manifolds* by G. de Rham, which was written in 1953 and translated into Russian in 1956, could not play that role. It led the reader to its main object – homology theory on the basis of differential forms and currents – too quickly, so that the reader did not have time to become familiar with manifolds in particular. De Rham seems not to have had any particular "educational" goal, but Pontryagin did, to a certain degree.

[12] Sard's work was published during World War II and was not known in Moscow for a long time. His theorem was soon after rediscovered by Dubovnitskii, and we called it by his name.

the conjecture that structurally stable systems are everywhere dense in the space of all dynamical systems of class C^n ($n \geqslant 1$) and that structurally stable systems are what were later called Morse–Smale systems. These conjectures were a direct generalization of the Andronov–Pontryagin theorem on structurally stable systems in a bounded planar domain and the theorem of Peixoto on structurally stable flows on closed surfaces,[13] which appeared around 1960 and has a similar statement. I note that Smale proved something very significant for Morse–Smale systems, which was obtained by highly nonobvious reasoning — the Morse–Smale inequalities. I myself (like many others, I believe) had proved nothing about these systems, and that was what saved me (us) from publicly stating false conjectures.

In attempting to prove that Morse–Smale systems are generic, I proved what is now called the Kupka–Smale theorem. But maybe it only seemed to me that I proved it. The proof was based on the "technique of bringing into general position by small displacements," which I had mastered by that time, and I found it simple. Later, in a 1964 conversation with Peixoto, I expressed myself in this way, but was met with incomprehension on his part, which is not surprising: Peixoto had just published a simplified proof of this theorem and seems to have known that there were dangerous rocks just below the surface of those waters. Thus he was partly correct, and I am no longer sure that there was a complete proof in my sketchy arguments. But I also was partly right: if I had been ordered under pain of death (or even expulsion from graduate study) to write a complete proof of the Kupka–Smale theorem, I would still have been able to cope with the assignment by finding and going around all those rocks. My preliminary work later came in handy in the proof of Abraham's theorem on bumpy metrics, which hung in the air for 15 years [6]. There was again no great deed of mine, but somehow over those 15 years an incorrect proof was published, and not by some inexperienced novices, but by well-known authors (Klingenberg and Takens[14]).

I must confess one of my own lapses of thought. In 1959 I thought up the following example of a noninvertible and nonsmooth mapping f of the closed interval $[0,1]$ into itself:

$$f(x) = \begin{cases} 3x, & 0 \leqslant x \leqslant \frac{1}{3}; \\ 2-3x, & \frac{1}{3} \leqslant x \leqslant \frac{2}{3}; \\ 3x-2, & \frac{2}{3} \leqslant x \leqslant 1. \end{cases}$$

[13] This theorem was published in 1962, but it seems to me that the result had somehow become known earlier.

[14] To be fair, it should be noted that the theorem about bumpy metrics is only a part of their paper. If that theorem is proved, all the rest is correct.

I reasoned that the cascade $\{f^n\}$ is topologically transitive, has a countable set of periodic trajectories, and that it was doubtful it could be approximated by something which resembles a Morse–Smale cascade. But I ascribed all this to the fact that the mapping f is nonsmooth and noninvertible. What would it have cost me (“thinking in manifolds”) to reason my way to the dilation of the circle $\mathbb{R}/\mathbb{Z}$ into itself, mapping x to $3x$ mod $\mathbb{Z}$? The nonsmoothness would have disappeared, and probably the role of instability would have become obvious. It would then have been possible to think about the role of noninvertibility. In one dimension you can’t construct such an example without it, but what about the multi-dimensional case? In general, what would it have meant to me to become Smale for a few hours? Well, this is too much to ask, but I might have thought of an expanding mapping of the circle and its connection with the one-sided Bernoulli shift, and even to the favorite number-theoretic mapping $x \mapsto \{3x\}$, where $\{\cdot\}$ denotes the fractional part of a number. However, I didn’t.

To console myself, I can say that I am not the only one who didn’t think things through (although in my case the lapse was greater — after all, I was “right next door to it”). Indeed, the mapping $x \mapsto \{ax\}$, where $a > 1$ is an integer, was being studied in number theory as early as the beginning of the twentieth century; its connection with n-ary expansions is obvious, and that might have become a source of a symbolic dynamics more widely accessible than geodesic flows, where a certain “technical minimum” is still necessary. But Émile Borel understood its connection with a sequence of independent random trials. The mapping undoubtedly played a role in the construction of the now generally accepted Kolmogorov interpretation of the theory of probability on the basis of measure theory; and when ergodic theory formed into an independent area, Bernoulli shifts became an important example in it. But in the theory of smooth dynamical systems, expanding mappings attracted attention only after the role of hyperbolicity had been recognized and these mappings could no longer play the role they might have done earlier. [15]

All this is about thoughts that remained “reflections for the soul.” But in parallel with them I was studying the theory of differential equations — without

[15] The book [7] contains a section in which it is shown that there are small perturbations of a hyperbolic automorphism of the two-dimensional torus under which area remains an invariant measure but the metric entropy varies. (This leads to the “scandalous” conclusion that the homeomorphism linking the perturbed system to the unperturbed one maps some set of full measure to a set of measure zero.) Before I discovered that fact, I had realized that a similar phenomenon occurs for an expanding mapping of the circle. Thus, I got a small “hint” from these mappings even so. As usually happens with hints, I didn’t say anything about it at the time.

I recall that Shub, who was then a young student of Smale, spoke about expanding mappings in the theory of dynamical systems (starting from the general case, and not only for the circle).

any brilliant results, but not entirely without success. I owe these successes — to what extent I am not sure (somewhere between 50% and 99%) — to the fact that Pontryagin and Mishchenko had fortunately assigned some independent-study problems to me, gradually increasing their difficulty and leaving an ever greater scope for my independent initiative. [16] And I myself picked up something from the creative atmosphere surrounding Pontryagin at the time.

By 1961 I had become thoroughly familiar with a number of questions of the classical theory of differential equations. I used some of it in my own papers, but normally studied not only what "worked" for me but a great deal of surrounding material as well. In particular, I knew the work of Lyapunov and Perron on stability and conditional stability, not only the part of it that involves equilibrium positions and periodic trajectories (which would have been sufficient for my needs) but in full generality. I also new Bogolyubov's work on averaging and integral manifolds, which had no direct relation to what I was doing, but which influenced me, as I have already had occasion to write [9].

Outside the sphere of the new topology and differential equations, I was somehow attracted to Riemannian geometry and I read (at least in first approximation) the book of É. Cartan [10] (normally other books were read).

[16] Postnikov has written that Pontryagin assigned problems to graduate students to which he already knew the answer, and this guaranteed that the student would finish his dissertation before the deadline [8]. Postnikov himself rebelled when he discovered that Pontryagin knew everything in advance, and wrote his dissertation on a different topic, close to the interests of Pontryagin, of course, but containing results that Pontryagin had not known. I also turned out to be something of an exception to the usual practice. When I was a graduate student, it was proposed that I think about multi-frequency averaging in the presence of a reactive effect of slow motions on rapid ones. The problem was general and indefinite — my teachers had no explicit picture of convergence with respect to the measure of the initial values. But I succeeded in justifying the confidence shown. As for the defense deadlines, mine took place some months before my graduate study period had expired, and to my dismay I had to leave graduate school. The graduate program at the Mathematical Institute of the Academy of Sciences (in the second and third years, when philosophy and foreign languages are finished) is so pleasant! (There is no fixed schedule.) Now I had to go to work regularly for a while, and in the morning at that! I am a night owl and work best in the evening. For a while I arrived alone in the morning and napped on the sofa. However, the administration at the Mathematical Institute was never zealous about labor discipline and did its best to hinder orders arriving from above. In that way, during the winter of 1961–1962 I was again allowed to work according to a schedule that was better suited for my body — at home at night (where, by Soviet standards, conditions were good — I had my own room). It was at home at night that my first paper on hyperbolicity and structural stability was conceived. Before and since, a few good ideas have come to me at other times of the day and when I was not at home; but while I was on the job at the Mathematics Institute, nothing at all came. On the other hand, at the Institute one can do other things that are also useful and even necessary. The main one is to discuss various technical questions with colleagues. One can even edit texts, write reviews, and make a preliminary examination of the literature in the library, so as to get a general idea about it and decide whether it should be taken home for detailed examination.

That is approximately the state I lived in up to September 1961, when there was an international symposium on nonlinear oscillations in Kiev. I hesitated at first, wondering whether it was worth going there. I had spoken on my most recent work (on averaging in multi-frequency systems) in spring of the same year at the fourth (and last) all-Union mathematical congress in Leningrad, and had defended my *kandidat* dissertation on the same topic. I was reluctant to repeat myself and speak about the same thing again. But Smale, who was already famous in topology by then, but not yet in dynamical systems (his works in this area did not yet rival those in topology), had written to Novikov that he would be in Kiev and then wanted to go to Moscow and meet with Novikov (and perhaps with other young Moscow mathematicians, but not with me — he knew nothing about me, and indeed there was nothing to know, since I had written a few papers, but not on topics of interest to Smale and not on a level that would attract his attention, despite their distance from his own interests). I went to Kiev mainly to assure Smale that Novikov and other advanced young people would be in Moscow.

Before the conference started, the Kievans had published abstracts of a number of the talks as separate brochures (which required considerable effort at the time), and the abstracts of the foreigners were translated into Russian. So, while standing in line to register as a participant and looking over the shoulders of the people in front of me, I began to examine the stacks of these brochures. I read the title of one of them: S. Smale. "A structurally stable differentiable homeomorphism with an infinite number of periodic points." At that moment the world turned upside down for me, and a new life began.

In addition to his scheduled talk at the conference, Smale kindly spoke about his discovery (I mean, of course, the "Smale horseshoe") in more detail to a group of interested people. Several of the Gor'kii mathematicians were there, but from Moscow I recall only myself and Postnikov. Afterward Smale came to Moscow and met with us (Novikov, Arnold, Sinai and I; I don't recall if anyone else was there) at the Steklov Institute, where he spoke in even more detail.[17] Moreover, he noted that a hyperbolic automorphism of the two-dimensional torus also refuted his naive conjecture that Morse–Smale systems were dense, and stated two conjectures: (1) that this automorphism was structurally stable, and (2) that a geodesic flow on a closed surface (or on an n-dimensional manifold) of (constant or variable?) negative curvature was structurally stable. We now know that the Smale horseshoe, a hyperbolic automorphism of the (n-dimensional)

[17] Of course, he must have discussed topological questions as well with Novikov, and he may have spoken to all of us on some topological topic on a popular level, but I don't remember if he did.

torus, and the geodesic flow just mentioned are all hyperbolic sets, but of course Smale did not know this at the time. (Even in 1966, after he had introduced that general concept, he was somewhat doubtful, or rather, cautious, as to the claim that invariant stable and unstable manifolds exist in all trajectories of a hyperbolic set, and not just in the periodic ones.) But I am sure that he intuitively sensed something common in these three examples.

I cannot help telling about the route that led Smale to the horseshoe. As he explained later, when he stated his unsuccessful conjectures, a few people wrote to him that these conjectures were wrong. He mentioned two of these people by name, who gave different arguments: Thom and Levinson.

Thom called Smale's attention to a hyperbolic automorphism f of the torus. It is trivial to prove that the Lefschetz number $|L(f^n)|$ tends to infinity as $n \to \infty$, and consequently that must also be the case for all mappings g close to f. This gives grounds for expecting that the number of periodic points of a mapping g with minimal period $\leqslant n$ also tends to infinity as $n \to \infty$. Here, however, one must exclude the possibility that there is only a finite number of periodic points whereas the local indices of f^n at these points (the Kronecker–Poincaré indices) increase without bound. Such a possibility disappears for Morse–Smale diffeomorphisms: it is easy to show that it disappears for all g that are close to f in the C^1 metric; this reasoning was obvious to Smale. (I remark that a more refined argument, developed later by Shub and Sullivan, excludes this possibility for all smooth g homotopic to f, and the long-known Nielsen theory, which was seemingly forgotten for a while, excludes it for all continuous g homotopic to f.[18]) Anyway, f cannot be approximated by Morse–Smale diffeomorphisms.

The letter from Levinson forced Smale to think seriously. Levinson mentioned the papers of Cartwright and Littlewood, and also his own papers, in which for certain second-order differential equations with a periodic perturbing force (which leads to a flow in three-dimensional space) he established the existence of an infinite number of periodic solutions. Moreover, such solutions (and in general all the basic properties of the "phase portrait"), as Levinson emphasized, are preserved under small perturbations. In his words, this is more or less clear from the reasoning of Cartwright and Littlewood, and in Levinson's work it is even directly proved (for a slightly different equation).

[18] These words sound somewhat ironic when applied to Shub, and especially to the professional topologist Sullivan. But, on the other hand, having forgotten about Nielsen, they traveled a different path. They investigated what could be said about the local indices of f^n at a fixed point that is isolated for all n and obtained nontrivial results (see [11], Part II, Chapter 2, § 2). Other people also studied these questions and related ones. The definitive results on the behavior of the local index as n varies were obtained for continuous mappings in [12] (Chapter 3, § 3) (where the infinite-dimensional case is discussed, but the results are new even in the finite-dimensional case), and for smooth mappings in [13] and more fully in [14].

How would any other mathematician than Smale have proceeded if he became interested in what Levinson had written? Probably he would have looked up the papers in question and started to read them. This is not easy to do, especially in regard to the work of Cartwright and Littlewood. If one is interested only in the existence of a certain example, one could limit the search to the less cumbersome paper of Levinson, but it too is far from easy reading.

Smale proceeded differently. He felt like a god who is to create a universe in which certain phenomena would occur. How was this to be done? From the articles of the three authors (or perhaps from Levinson's letter alone) he deduced only that in relevant examples a strong friction and a large perturbing force combine, while the trajectories remain in some bounded domain of the phase space. The first of these could be modeled geometrically by imagining that in one direction the trajectories converge rapidly, while in the other they diverge rapidly. If one is talking about iterations of a mapping instead of a continuous motion, one can imagine that a long narrow rectangle is obtained from a square. But since the trajectories remain in some bounded domain of phase space, this rectangle must be bent to keep it from leaving the region. Experimentation with drawings quickly led to the horseshoe. Reflecting on the preservation of the fundamental properties of the phase portrait (as pointed out by Levinson) led to the conclusion that the horseshoe was structurally stable.

Smale, as far as I know, has not described the details of his thought process. But it seems to me that in this case they can be reconstructed as related above. But how he guessed that the other two dynamical systems are structurally stable — that, I repeat, is a mystery to me.

Subsequently, events developed as follows. Arnold and Sinai discovered a proof, unfortunately incorrect, that a hyperbolic automorphism of the two-dimensional torus is structurally stable. Without noticing their mistake and accepting their theorem as valid (after all, the theorem is true), I could not help thinking about a certain resemblance to the Grobman–Hartman theorem, which had appeared shortly before and which asserts that a multidimensional saddle point is structurally stable. In the end my thoughts turned to the following: Let the diffeomorphism g of the torus be C^1-near to a hyperbolic automorphism f, and assume that the latter is structurally stable; then for any point x, somewhere near the trajectory $\{f^n x\}$ there must be a trajectory $\{g^n y\}$. Can we be sure that such a trajectory exists if we do not assume in advance that f is structurally stable? And how many such trajectories $\{g^n y\}$ are there? As soon as I thought about this, everything became obvious.

Obvious to me, but it may be that I was the only one to whom it was obvious at the time. I have heard it said that British Conservatives claim that good laws and institutions are not after all so important — that what is important is to have a suitable person in the requisite place (the British version of the formula "everything depends on the personnel"). Of course, in the case of the Conservatives, it involves the political and economic sphere. In my case this formula worked in the sphere of science. My entire preparation had made me the most suitable person for this particular question — for I "handled myself well in manifolds" (inside "no-man's-land") and I knew the classical things about differential equations. Finally, the hyperbolicity of geodesics in spaces of constant negative curvature (if only in the two-dimensional case) was well known to any educated mathematician by then. That this hyperbolicity continues to hold with variable negative curvature, was known to me from Appendix III of [10].

So began my studies of dynamical systems having a closed phase manifold and all of whose trajectories behave hyperbolically (in fact as hyperbolically as it is possible to do). With time, ergodicity and stronger statistical properties were added to structural stability. All this comprised my doctoral dissertation [7].

I was not allowed to attend the International Congress of Mathematicians in Stockholm in 1962, although in general young people at that time aroused less suspicion in the security institutions than was to be the case later, and some of my contemporaries were there. I think the question of my trip arose too late; in any case I had no invitation, and it was too late to get one. But Arnold and Sinai tried to bring my work to general notice. Still, I was able to travel to the USA in the autumn of 1964, in the company of Pontryagin and his wife. But they spent part of the time with Gamkrelidze and Mishchenko, who had been sent there earlier for a longer business trip. Thus, Pontryagin and his wife did not need my help, and I was able to fly to California for a while. In Berkeley I of course met with Smale and made the acquaintance of Pugh, who had just proved his "closing lemma." Smale very kindly named dynamical systems whose phase space is a hyperbolic set (as we would now say) after me, and thereby advanced me to a leading position among citations in *Mathematical Reviews*.[19] At the time Pugh was finishing the proof of the closing lemma, but he later undertook a serious study of hyperbolicity and was one of the pioneers of "stable ergodicity."[20]

[19] Subsequently, William Thurston reinforced this trend with his "pseudo-Anosov maps." Smale and Thurston thereby set up a collision with the "scientifically based" Citation or Impact Index, which in my case, as I understand it, is not terribly high. Well then, am I famous in the mathematical world or not?

[20] Ergodic smooth dynamical systems with a "good" invariant measure (one that has a positive density in terms of local coordinates) and such that all dynamical systems sufficiently close to them (in the sense of C^r for a suitable r) with the same invariant metric are also ergodic, are called *stably ergodic*. Anosov dynamical systems are of this type. But it turns out that there exist other stably ergodic dynamical systems. For references, see [15].

I repeat that, from all my impressions, Smale was unable for a time to express in well-articulated concepts what he could feel intuitively. For that reason, it became my best known achievement in mathematics that I explained to Smale what his achievement amounted to.

The following year, Smale stated the general concept of a hyperbolic set, which immediately became one of the basic concepts of the theory of dynamical systems. Together with the long-known hyperbolic equilibrium positions and closed trajectories,[21] locally maximal hyperbolic sets began to play the role of the basic structural elements, to which the most attention must be paid when describing the phase portrait. Smale at first hoped that, with such an enlargement of the stock of structural elements, suitable modifications of his earlier conjectures would turn out to be true. But as soon as he and his colleagues began to analyze more attentively how these elements combine among themselves by means of trajectories going from one element to another, the conjectures were refuted once again, as were new, hasty attempts to modify them. At present hardly anyone except Palis would venture to advance conjectures on the structure of "generic" dynamical systems.[22] Thus the grandiose hopes of the 1960s were not confirmed, just as the earlier naive conjectures were not confirmed.[23] But the scope has been enlarged beyond belief, even though by no means all dynamical systems fall into the enlarged "field of vision." A shining memorial to that era is the article of Smale [16], devoted largely to the hyperbolic theory, which for a time became almost the basic source of inspiration for many people working in the theory of dynamical systems.

That is essentially all I am going to say about the rise of the general concept of hyperbolicity, as it took place in pure mathematics. I have said nothing here about the long prehistory that begins with the discovery of homocyclic points in Poincaré's memoir "On the three-body problem and the equations of

[21] This means equilibrium positions for which the linear position spectrum lies outside the imaginary axis and also closed trajectories whose multipliers, except for the unavoidable unit, do not lie on the unit circle. (I am speaking of flows; the changes needed to talk about cascades are obvious.) From Smale's new point of view these equilibrium positions and closed trajectories arose as merely the most trivial examples of hyperbolic sets. For that reason, he began to call such equilibrium positions and closed trajectories *hyperbolic*, although in the earlier qualitative theory a saddle point was called hyperbolic, but a node and a focus were not.

[22] For Palis' concepts see [1]. (However, these have changed over time.) Here hyperbolicity still plays a major role, but weakened in comparison with the version from the 1960s. (The latter is now spoken of as "complete and uniform hyperbolicity" when it must be distinguished from the later versions.)

[23] The only conjecture that was confirmed was the one on the structure of structurally stable systems stated by Smale and Palis. For citations, see the literature below.

dynamics,"[24] and Hadamard's paper on surfaces of negative curvature. Enough is said about that in the literature. But in the literature connected (at least verbally) with applications the history is told differently. The discovery of homocyclic points by Poincaré (and sometimes the "Papal memoir" of G. D. Birkhoff) is still mentioned, but there is usually not one word about geodesic flows. The basic action begins with the paper of E. Lorenz "Deterministic nonperiodic flow" published in 1963 in the *Journal of Atmospheric Sciences* — not the most popular outlet among pure mathematicians. This article, and a number of others connected with it, can be found in the collection [17]. As a result of numerical experiment with a particular system of third order, interest in which was motivated by hydrodynamic considerations, Lorenz discovered a number of interesting phenomena. But his discovery attracted attention only a decade or so later (by which time hyperbolicity and the phenomena connected with it were already well known to theoreticians). Moreover, it interested applied mathematicians and theoretical mathematicians for different reasons. It convinced the applied mathematicians that there exist strange (or chaotic) attractors. (The hyperbolic strange attractors discovered by mathematicians were unknown to the majority of them, since no examples of such objects had been encountered in problems having their origin in natural science.[25]) But the pure mathematicians were interested in the Lorenz attractor not as one more demonstration of the possibilities for behavior of trajectories, but conversely, as an object that, although close to hyperbolic attractors in a number of its properties (and, as it happens, precisely those that made a particular impression on the applied mathematicians), was at the same time different from them in other properties. (For the mathematicians it was these subtleties that were of interest; see [15] and the literature it contains.)[26]

For information on hyperbolic sets, see [19]. The survey [20] is devoted specifically to structurally stable systems, but it is now slightly out of date. More recent information can be found in [15]. In the same place there is a discussion of stable ergodicity and much else that has appeared since the 1960s. Peixoto [21] and Smale [22] have written their reminiscences, which partly intersect with my own.

---

[24] And if we were to tell the whole story, we should begin with their "nondiscovery" in the first version of that memoir (which was withdrawn).

[25] Neither then nor now. One gets the impression that the Lord God would prefer to weaken hyperbolicity a bit rather than deal with the restrictions on the topology of an attractor that arise when it really is (completely and uniformly) "1960s-model" hyperbolic.

[26] I take the opportunity to correct a mistake there. "A strange strange attractor" is not the name of the article of Ruelle and Takens, who were the first of the pure mathematicians to notice the work of Lorenz (and whose article is in [17]), but the name of the followup article of Guckenheimer, found in [18].

In conclusion, I should remark that since the time period discussed here different weakened variants of hyperbolicity have arisen (the stable ergodicity mentioned above is connected with some of them), but the role of hyperbolicity has nevertheless decreased. Hyperbolicity (when it occurs) is now an important component of the description of the phase portrait of a dynamical system, but hyperbolicity itself is now the subject of much less research. The position that hyperbolicity occupied in the theory of dynamical systems has now passed by legal inheritance to the theory of nonlocal bifurcations. But there still remain unsolved problems in the hyperbolic theory, some of which are better known than others. Here is one question that is comparatively little known, although I posed it about 30 years ago: *Is it true that in an arbitrarily small neighborhood of every hyperbolic set there is a locally maximal hyperbolic set* (a "basis" set in the terminology of R. Bowen)? Since a great deal is known about locally maximal hyperbolic sets, a positive answer would give information about all hyperbolic sets. A negative answer would reveal hyperbolic sets of some fundamentally new type.

Bibliography

[1] J.-C. Yoccoz. Recent developments in dynamics. In: *Proceedings of the International Congress of Mathematicians*, Vol. 1, 2 (Zürich, 1994). Basel: Birkhäuser, 1995, 246–265.

[2] Y. Katznelson, D. Ornstein. The differentiability of conjugation of certain diffeomorphisms of the circle. *Ergod. Theory Dynam. Systems*, 1989, **9**, 643–680.

[3] Y. Katznelson, D. Ornstein. The absolute continuity of conjugation of certain diffeomorphisms of the circle. *Ergod. Theory Dynam. Systems*, 1989, **9**, 681–690.

[4] K. M. Khanin, Ya. G. Sinai, A new proof of M. Herman's theorem. *Comm. Math. Phys.*, 1987, **112**, 89–101.

[5] Ya. G. Sinai, K. M. Khanin. Smoothness of conjugacies of diffeomorphisms of the circle with rotations. *Russ. Math. Surveys*, 1989, **44**(1), 69–99.

[6] D. V. Anosov. On generic properties of closed geodesics. *Math. USSR, Izv.*, 1983, **21**, 1–29.

[7] D. V. Anosov. *Geodesic Flows on Closed Riemann Manifolds of Negative Curvature*. *Proc. Steklov Inst. Math.*, 1967, **90**. Transl. from Russian by S. Feder. Providence, RI: Amer. Math. Soc., 1969.

[8] M. M. Postnikov. Pages of a mathematical autobiography (1942–1953). *Istoriko-Matematicheskie Issledovaniya* (2), 1997, No. 2 (37), 78–104 (Russian).

[9] D. V. Anosov. On N. N. Bogolyubov's contribution to the theory of dynamical systems. *Russ. Math. Surveys*, 1994, **49**(5), 1–18.

[10] É. Cartan, *Geometry of Riemannian Spaces*. Transl. from French by J. Glazebrook; notes and appendices by R. Hermann. Brookline, MA: Math. Sci. Press, 1983.

[11] D. V. Anosov, V. I. Arnold, eds. *Ordinary Differential Equations and Smooth Dynamical Systems*. Berlin: Springer, 1988 (Dynamical Systems I; Encyclopædia Math. Sci., **1**).

[12] M. A. Krasnosel'skii, P. P. Zabreiko. *Geometrical Methods of Nonlinear Analysis*. Transl. from Russian by Christian C. Fenske. Berlin: Springer, 1984.

[13] S. N. Chow, J. Mallet-Paret, J. A. Yorke. A periodic orbit index which is a bifurcation invariant. In: *Geometric Dynamics* (Rio de Janeiro, 1981). Berlin: Springer, 1983, 109–131 (Lecture Notes in Math., **1007**).

[14] I. K. Babenko, S. A. Bogatyi. Behavior of the index of periodic points under iterations of a mapping. *Math. USSR, Izv.*, 1992, **38**(1), 1–26.

[15] D. V. Anosov. On the development of the theory of dynamical systems over the past quarter-century. In: *Student Readings of the Independent University of Moscow*, No. 1. Moscow Center for Continuous Mathematical Education, 2000, 74–179 (Russian).

[16] S. Smale. Differentiable dynamical systems. *Bull. Amer. Math. Soc.*, 1967, **73**, 747–817.

[17] Ya. G. Sinai, L. P. Shil'nikov, eds. *Strange Attractors*. Selected articles transl. from English. Moscow: Mir, 1981 (Math.: Recent Publ. in Foreign Sci., **22**) (Russian).

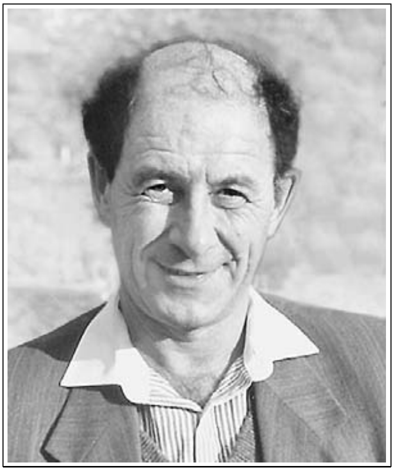
[18] J. Marsden, M. McCracken, *The Hopf Bifurcation and its Applications*. New York: Springer, 1976.

[19] D. V. Anosov, ed. *Dynamical Systems with Hyperbolic Behavior*. Berlin: Springer, 1995 (Dynamical Systems IX; Encyclopædia Math. Sci., **66**).

[20] D. V. Anosov. Structurally stable systems. In: *Topology, Ordinary Differential Equations, Dynamical Systems*. *Trudy Matem. Inst. im. Steklova*, 1985, **169**, 59–93 (Russian).

[21] M. M. Peixoto. Acceptance speech for the TWAS 1986 award in mathematics. In: *The Future of Science in China and the Third World*. Proceedings of the Second General Conference Organized by the Third World Academy of Sciences (eds. A. M. Faruqui, M. H. A. Hassan). Singapore: World Scientific, 1989, 600–614.

[22] S. Smale. *The Mathematics of Time. Essays on Dynamical Systems, Economic Processes, and Related Topics*. New York: Springer, 1980.



V. I. Arnold

From Hilbert's Superposition Problem to Dynamical Systems

Presented by the author

Some people, even though they study,
but without enough zeal, and therefore live long.
Archibishop Gennady of Novgorod
in a letter to Metropolitan Simon, ca 1500

Here I shall try to explain the diversity of subjects I have worked on. In fact, I followed one line from the very beginning and there was essentially one problem I was working on all my life. This fact seems strange even to me, but I shall try to explain it.

When you are collecting mushrooms, you only see the mushroom itself. But if you are a mycologist, you know that the real mushroom is in the earth. There's an enormous thing down there, and you just see the fruit, the body that you eat. In mathematics, the upper part of the mushroom corresponds to theorems that you see. But you don't see the things which are below, namely *problems, conjectures, mistakes, ideas, and so on.*

You might have several apparently unrelated mushrooms and are unable to see what their connection is unless you know what is behind. And that's what I am now trying to describe. This is difficult, because to study the visible part of the mathematical mushroom you use the left half of the brain (which deals

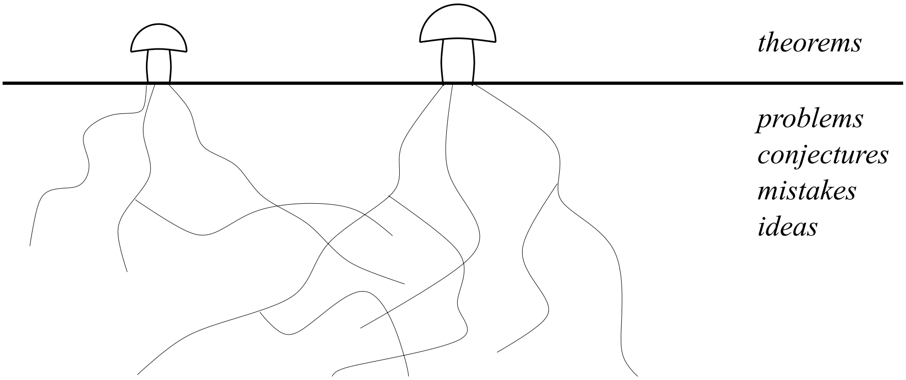


Fig. 1. The mathematical mushroom

with logic), while for the other part the left brain has no role at all, because that part is extremely illogical. It is therefore difficult to communicate it to others. But here I shall try to do it.

First, I shall mostly discuss some history, and then I'll turn to the hidden part of the mushroom, namely to ideas, providing the main motivation for the research. And then some theorems will appear in the end.

The first serious mathematical problem which I considered was formulated by Hilbert. It is a problem on superpositions emerging from one of the main mathematical problems: the solution of algebraic equations.

The roots of a quadratic equation

$$z^2 + pz + q = 0$$

can be expressed by a simple formula in terms of p and q. Similar formulas are also available in degrees 3 and 4. If the degree is 5, then you know from Abel's theorem (or in other terms from the monodromy of the corresponding algebraic function and the fact that the alternating group in five variables, that is, the group of even permutations of 5 elements, is not solvable) that there is no such formula.[1] However, there is a classical result that if you know how to

[1] I have lectured on this topological version of Abel's theorem to Moscow high-school children. This course, together with exercises, was later published by one of the students, V. B. Alekseev, in the form of a nice book *Abel's theorem in problems and solutions* (Moscow: Nauka, 1976). In these lectures, I started from the geometry of complex numbers, then passed to Riemannian surfaces, coverings, fundamental group, monodromy, homomorphisms, normal factors, and solvable groups. Abel's theorem is proved topologically as a demonstration how the monodromy group of a superposition of functions is expressed via monodromy groups of the functions under

solve one very special equation, namely

$$z^5 + az + 1 = 0,$$

i. e., if you know one particular algebraic function $z(a)$, then you can solve all the equations of degree 5. For quadratic equations you need square roots; for cubic equations — both square and cube roots (which can be considered as simple special functions); for quartic equations — also the fourth root; but for degree 5 you need a more complicated special function, and this function $z(a)$ suffices. This was classically known.

And then people (for example, Hermite) tried to solve the equation of degree 6 using a function of one variable. But no one succeeded. How was this supposed to be done? You kill the terms of the equation one-by-one using some substitutions, and to find these substitutions you solve auxiliary equations. In degrees 2, 3 and 4 all the auxiliary equations can be solved and thus all the terms can be killed.

But in degree 5 there remains one coefficient you cannot kill (the coefficient a above). And in degree 6 two coefficients remain and you get the following normal equation which is sufficient to solve all the equations of degree 6:

$$z^6 + az^2 + bz + 1 = 0.$$

Thus, there is a special function of two variables $z(a,b)$ which solves all the equations of degree 6. By the way, no one has ever proved that you really need two variables here — the conjecture is that there is no such function of one variable which would suffice, but no one has ever proved this.

For degree 7 the same procedure leaves you with 3 coefficients

$$z^7 + az^3 + bz^2 + cz + 1 = 0,$$

---

superposition (these groups are commutative if the functions are radicals). This book prepares the reader for proving Abel's theorem by solving topological problems. Unfortunately, it was not translated into English till 2004. My student A. G. Khovanskii extended these topological ideas to differential algebra in his thesis. He proved by topological arguments the nonsolvability of some differential equations in terms of combinations of elementary functions and of arbitrary single-valued (holomorphic) functions in any number of variables. The idea was that the monodromy of the complex solution is too complicated to be the monodromy of such a combination.

Here it is worth mentioning the topological theorem of Abel that Abelian integrals along nonspherical algebraic curves (tori for elliptic integrals, etc.) are nonelementary. G. Hardy wrote (in *A Mathematician's Apology*) that "no harm to mathematics would happen if it were without Abel, Riemann, and Poincaré." He was apparently unaware of this theorem.

which defines an algebraic function of 3 variables $z(a,b,c)$.[2] Hilbert asked whether such functions of three variables really do exist.

If you have a function in two variables $z(a,b)$ and you substitute, say instead of a, a function in two variables $a(u,v)$ and continue in this way, then you can get a function in any number of variables. You cannot do a similar thing with one variable, but using only functions in two variables, you can construct functions with an arbitrary number of variables. Hilbert asked whether you really need functions in three variables to solve the universal equation of degree 7 written above and, more generally, whether you can represent *any* function in three variables as a superposition of functions in two variables — i. e., whether functions in three variables really do exist.

It is easy to see that, using *discontinuous* functions, it is always possible to find an expression in functions of just two variables representing any given function of three variables. Hilbert asked whether you can get any continuous function in three variables by combinations of *continuous* functions in two variables. It is strange, by the way, that Hilbert formulated this problem of algebraic geometry in terms of functions of real variables — but he did.

In 1956 I was an undergraduate student and A. N. Kolmogorov, my supervisor, was working on this problem. He proved that "functions in 4 variables do not exist": any continuous function in 4 variables or more can be reduced to continuous functions in 3 variables. But he was not able to reduce the number of variables from 3 to 2 and gave this problem to me.

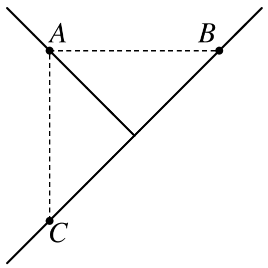


Fig. 2. The simplest tree

Kolmogorov had proved that it is sufficient to represent any function on a tree in Euclidean space — actually, to find a universal tree such that any continuous function on this tree can be represented as a sum of three continuous functions, each depending on one coordinate. If you can do this, then "there are no functions of three variables" and you can reduce any continuous function to the continuous functions of two variables — and the function $z(a,b,c)$ is reducible too. This was a problem I managed to solve. It was essentially simple — I shall show you the idea because I will need it for dynamical systems. For the simplest example shown in Fig. 2, the claim is that on this tree any function can be represented as $f(x)+g(y)$. How to do this? You choose any point A on the tree, you take the value of the function at this point and

[2] Starting from degree 9, one can kill one more coefficient. The known possibilities to kill more coefficients occur along a rather strange infinite sequence of degrees.

decompose it arbitrarily. Then, at point B lying on the same horizontal level as A, the second function is known, and the sum is known, and you get the value of the first function. And at point C lying on the same vertical line as A, the first function is known, and the sum is known, and you get the value of the second function. That's all. If the tree is more complicated, you will have more branches, or even an infinite number of branches. Then you will have to work more, and in fact to make the infinite process converge you need 3 variables, not 2. But the above is the principal idea – that's how it worked.

Now I shall discuss this problem returning to polynomials, and I shall reformulate the Hilbert problem in the way I would like it to be formulated. The function $z(a,b,c)$ that satisfies $z^7+az^3+bz^2+cz+1=0$ is an algebraic function in three variables. You can construct algebraic functions in three variables from algebraic functions in two variables by superpositions. The problem is whether this particular algebraic function $z(a,b,c)$ can be represented as a combination of algebraic functions in two variables.[3] I would say that this was the *genuine* Hilbert problem. Unfortunately, he did not formulate it in this way and probably because of that this problem is still open: no one knows whether there is such a representation. I think this is a very nice problem, and many times I have attempted to do something in this direction.

Of course, you also have other types of functions: for instance, you have continuous functions, but you also have smooth functions. For smooth functions this problem has been attacked by A. G. Vitushkin in the beginning of the 1950s. He has proved that you have to lose some number of derivatives. For example, if you have a C^3 function in three variables, you cannot, generally speaking, represent it by C^3 functions in two variables. The best you may hope to do is to express it by C^2 functions in two variables. The proof was based on a technology which he called the *theory of multidimensional variations* and which is, in fact, a version of integral geometry of the Chern classes describing the integrals over cycles in Grassmann varieties.

Vitushkin's technology was based on some evaluations of topological complexity in real algebraic geometry. This is also one of the main problems in mathematics. In the simplest case, for the curves you have a polynomial equation, say, of degree n, in 2 variables, and you want to know the topology of the variety defined by this equation (in higher dimensions, by a system of such equations in the affine or projective space). This question was also formulated by Hilbert as a part of his 16th problem. For many years people have been

[3] If the given function is an entire algebraic function (without poles), then it is natural to consider the representations using only entire algebraic functions. Thus one should distinguish two representation problems: that admitting only entire, and that admitting arbitrary algebraic functions.

working on this problem: Hilbert himself obtained some results, while Harnack found the number of ovals for the curves. For higher-dimensional varieties, the problem was studied by I. G. Petrovskii and his student O. A. Oleinik. They found the bounds for the Betti numbers of algebraic varieties defined by (systems of) polynomial equations in terms of their degrees and dimensions. This was the crucial part of Vitushkin's proof of his statement about smooth functions. Of course, the fact that for generic functions in 3 variables you need some not very smooth functions in 2 variables does not imply anything for algebraic functions. Algebraic functions are such a small portion of all functions that you still can have such a representation for them.

By the way, this theory by Oleinik and Petrovskii (dating from the 1940s and 1950s) was later rediscovered in the West by J. Milnor and by R. Thom. Although they did quote Petrovskii and Oleinik, the results are mostly attributed to Milnor and Thom, who introduced the modern terminology related to the Smith theory[4] and to the interaction between the homology of real and complex manifolds. But stronger results were already present in the papers by Petrovskii and Oleinik, and these were heavily used by Vitushkin. I studied this as an undergraduate student because of its relation to the Hilbert problem.

I tried to do something on this problem later, and this was the motivation for me to study the algebraic function $z(a_1,\dots,a_n)$ defined by the equation $z^n + a_1 z^{n-l} + \cdots + a_n = 0$. This function has a complicated discriminant hypersurface in the base space of coefficients $\mathbb{C}^n = \{(a_1,\dots,a_n)\}$. The discriminant hypersurface is the set of all points where the function z is not "nice," in particular, not smooth. In about 1967, I started to think about how to use the topology of this object to deduce from it an obstacle to the representation of algebraic functions in terms of algebraic functions of a smaller number of variables. I thought that the topology of our algebraic function for higher n is complicated and, if there were an expression in terms of functions of fewer variables, then it should be simpler.

So I studied the topology of this space — the complement to the discriminant — which is in fact the configuration space of sets of n points in $\mathbb{C}$ and the Eilenberg–MacLane space $K(\pi,1)$ of the braid group. In one of the first

[4] It follows from the Smith theory that the sum of Betti numbers of a real algebraic variety is no greater than the sum of Betti numbers of the complex variety specified by the same equations (all Betti numbers being $\mathbb{Z}_2$-homologies ranks). For example, for a real algebraic curve with b components, the sum of Betti numbers equals $2b$, and for a Riemannian surface of genus g (a sphere with g handles) it equals $2g+2$. Therefore, the Smith theory implies the Harnack inequality $b \leqslant g+1$. The Riemannian surface of a plane curve of degree 3 (an "elliptic curve") has genus $g=1$ and hence at most $g+1=2$ real connected components on the projective plane $\mathbb{R}\mathrm{P}^2$. Examples are given by equations of phase curves for cubic potential $y^2 = x^3 - x + c$ with various values of the total energy constant c.

papers on this subject — "On cohomology classes of algebraic functions which are preserved by the Tchirnhausen transformation" (1970) — I mentioned an interesting analogy between the theory of fiber bundles and that of algebraic functions. The complement to the discriminant is the counterpart of the Grassmannian. The analogy (existing both in the complex and the real case) goes very far, for instance, to the Pontryagin–Thom cobordism theory. These ideas were later used by many people, and recently have even been formalized (by A. Szűcs and R. Rimányi, 1996).

This was the beginning of my work in singularity theory. And in fact all those works on A, D, E singularities, Coxeter groups and so on are a byproduct of the study of this special function $z(a_1,\ldots,a_n)$, and the question of how complicated is the topology of the discriminant.

Thinking about this, I decided to find the cohomology ring of the braid group. I have computed the first dozen of these groups (mostly torsion) and obtained a lot of information. Then D. B. Fuchs computed all those groups modulo 2. Later came the theorem of May–Segal on the relation of all these groups to the second space of loops of the 3-sphere $\Omega^2(S^3)$ which has the same homology as the braid group. In fact, the space $\Omega^2(S^3)$ is the Quillenization of the complement to the discriminant. All this was done in an attempt to find some higher dimensional properties of the braids which could prevent algebraic functions being representable as combinations of the algebraic functions of fewer variables.

It is interesting that perhaps the most useful mushroom coming from this root is the application of my results by S. Smale in his theory of complexity of computations. In his topological complexity theory, Smale discovered (using a theory which was essentially developed by Albert Schwarz years before) that my structure of the cohomology ring of the complementary space of the discriminant is an obstacle to numerical computation of the roots of a complex polynomial with few branchings (operators IF, THEN, ELSE) in the algorithm. For polynomials of degree n Smale proved (using essentially my computations of the cohomology) that the complexity is at least $(\log n)^{2/3}$ (you really need this number of branchings). One can obtain stronger results using the information about braids found in the marvellous paper by D. B. Fuchs "Cohomology of the braid group modulo 2" published in *Funktsional'nyi Analiz i ego Prilozheniya*, 1970, **4**(2), 63–72. By the way, the English translation of this paper was titled "Cohomologies of the group COS mod 2," and the term 'the braid group' was translated as 'the group COS' in the whole paper.[5] So, many people were

[5] In Russian, 'the braid group' is *gruppa kos*, and the cosine function is referred to either by its international notation or by the word *kosinus*. — *Eds.*

interested in what were the cohomologies of the group SIN. Perhaps due to this misunderstanding, Fuchs' paper was not properly appreciated at that time. But later it was understood, and now V. A. Vassiliev (using the results of Fuchs) has increased the number of topologically necessary branchings to $n - \log_2(n+1)$, which means that the topological complexity is almost n. (Smale had developed algorithms with n branchings.)

But the origin of all this is in Hilbert's thirteenth problem! Later, inspired by the analogy between algebraic functions and fiber bundles, I constructed a theory of characteristic classes of entire algebraic functions and found a class which is invariant under the substitutions; so I was able to prove some theorems on the impossibility of representations, using this cohomology. Later these works have been continued by V. Lin who proved the strongest result in this direction, also providing a correct basis for N. G. Chebotarev's ideas (dating back to the 1940s) on the topology of ramification of algebraic functions of several variables. Unfortunately, what we were able to do was just to prove that there is no formula representing the function we need, $z(a_1, \dots, a_n)$.

Let me explain the difficulty that arises for cubic equations. The Cardano–Tartaglia formula gives you the roots you wish, but it also gives you some other, parasitic roots, because you have some signs in the formula, some multivaluedness. The difficulty is how to understand such functions; for example, $\sqrt{z} - \sqrt{2z}$. What is the number of values of this function? There are several theories. For me, this function has four different values. If we understand the algebraic functions and their combinations in this way, then we can prove, using this cohomology theory, that there are functions which cannot be represented in the desired way. But then the cubic equation is not solvable too, which is not nice.

I think, however, that more work on this problem might bring some invariants of algebraic varieties and mappings of these varieties into each other which correspond to the superposition in such a way that one gets in this topological structure (in these algebraic invariants) some memory of the number of variables one had in the functions participating in the superposition. Perhaps there is some kind of a mixed Hodge structure whose weight filtration provides the information on the dimension of the smooth algebraic variety from which a given cycle was born. Unfortunately, I cannot formulate this as an exact theorem, but I hope that such a theorem might exist.[6] My conjecture is that our special function

[6] First steps in this direction were made recently by my Paris student F. Napolitano ("Pseudo-homology of complex hypersurfaces," *C. R. Acad. Sci. Paris, Sér. I Math.*, 1999, **328**, 1025–1030) who constructed generalizations of the braid group, which are based on the sequence of discriminants obtained from an algebraic hypersurface in the complex affine space by consecutive projections onto hyperplanes of decreasing dimension (and providing generators and relations of the fundamental groups of the complements to these discriminants).

$z(a,b,c)$ cannot be represented as a combination of algebraic functions in two variables for some essentially topological reasons. I also think that the representation remains impossible even if we replace all the algebraic functions in the superposition by nonholomorphic complex functions which are *topologically* equivalent to algebraic ones.

Now I come to mechanics. As I have mentioned, my supervisor was Kolmogorov. He formulated the problem at a seminar and went to Paris for a semester. When he returned, I explained what I had invented. He told me that I had solved the Hilbert problem and added, "Well, now it would be very dangerous for you to ask me for the next problem, I think this will be harmful for you. I would be glad to discuss with you any kind of mathematics, but do not ask me for the next problem. Choose it yourself, this will be much better for you."

Perhaps I should explain one more thing here. Kolmogorov took my first article (for the *Doklady* – the Soviet analog of *Comptes Rendus*) and he told me that the supervisor must write the first article of a student, the student being never able to write correctly, because it's a very different art from the art of solving problems and proving theorems. "I shall show you once," he said. "A good student never needs a second experience of this kind." And indeed my first article was completely, word by word, rewritten by Kolmogorov. I wonder whether Kolmogorov had been involved in the writing of the first paper of I. M. Gel'fand, who was also his student. This is one of the few papers signed by Gel'fand alone, with no collaborators. Gel'fand, whose brilliant papers and highly influential seminar I always admired very much, mastered a special and enviable art of day-to-day collaboration with extremely gifted mathematicians (mostly his former students), resulting in important and beautiful joint publications. I dare to guess that these papers were actually written in most cases by the collaborators.

I have never collaborated with Gel'fand. Recently, at the Zürich Congress, he asked me what was the reason for this. My answer was that I preferred to preserve good personal relations with him. However, in the third volume of collected works of Gel'fand there is a paper which is not by Gel'fand and where he is not even a coauthor. It's my paper signed *Arnold*. I shall now explain what it is about and how it is related to my story.

When Kolmogorov advised me to choose a problem myself, I wanted to choose something completely orthogonal to all the works of Kolmogorov. This was difficult, because he was working on so many subjects, but still I tried to invent my own problem. I had the list of Hilbert problems, written down one by one in my notebook. (Gel'fand once saw it and laughed a lot.) I was completely ignorant of the existence of anything else in mathematics at that time and so it was difficult for me to imagine a problem. You can see this from the problem I did choose.

For a tree in Euclidean space, I was able to represent any continuous function as the sum of continuous functions depending on one coordinate each. I decided to study other curves: what would happen if the curve was not a tree? [7] So I started to study curves with cycles. We choose a point (Fig. 3, point 1), and at this point we can decompose the function into the sum of functions of x and of y in an arbitrary way. We go upwards and in the new point of the curve (Fig. 3, point 2) we know the function of x. Since we know the sum, we can find the function of y at that point. We draw a horizontal line and find the decomposition at its other end (Fig. 3, point 3), and so we continue. We thus get a dynamical system on this curve. We have two involutions of the curve: a vertical involution A and a horizontal involution B, and hence we have a mapping $T = AB$, a diffeomorphism of the circle preserving the orientation.

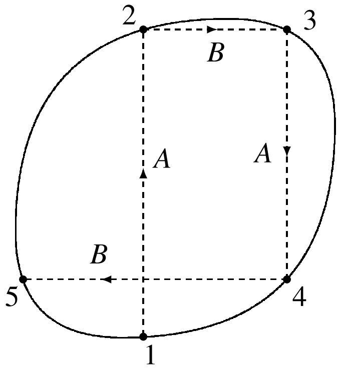


Fig. 3. Dynamics on the oval

After some experiments I was able to find that for these diffeomorphisms the rotation number exists, and there might be resonances, periodic orbits and so on. Then I found that Poincaré had already studied diffeomorphisms of the circle onto itself which preserve the orientation, and had created a theory for this. Then I read Poincaré and observed that for the ellipse, for instance, this transformation is equivalent to a rotation through an angle which depends on the ellipse. This angle is, in general, incommensurable with 2π and hence the mapping T represents an ergodic dynamical system.

In the resonance case, when there is a periodic orbit, [8] the periodic points are obstacles to the solution of the initial problem, because the alternating sum of

[7] This problem has recently been re-examined by A. B. Skopenkov (1996) who listed all the obstacles to the representation of any continuous function on a plane curve in the form of the sum of two continuous functions of the coordinates. The same problem reappeared in singularity theory of the late 1970s in the works of J.-P. Dufour and S. M. Voronin. They studied the representations of functions on the germs of curves with cusps as the sums of smooth and holomorphic functions of coordinates.

[8] The question how many such orbits can be in the algebraic case is far from being solved and is probably related to the question on limit cycles from Hilbert's 16th problem (being its version for dynamical systems with discrete time). For example, consider an algebraic correspondence of bidegree (a,b) and genus g on an algebraic curve K of genus h (an algebraic curve of genus g in

values of the function over a period must be 0, otherwise you cannot decompose it. If there are no periodic orbits, like in this "irrational elliptic case," then you can formally continue decomposition, but you will have a convergence problem. For one orbit you can calculate everything, but then it is a question whether you get a continuous function. If you write the Fourier series for the mapping of a circle equivalent to a rotation, you immediately get the problem of resonances and the small denominator problem for the rotation number.

Just at this time, Kolmogorov was giving at Moscow University a course on his work, on small denominators, on Hamiltonian systems, and on what is now called KAM theory. And so my attempt to invent something independent was completely unsuccessful!

Here I would like to talk about the history of Kolmogorov's discovery of his famous theorem on invariant tori. In 1954 he proved the theorem on conservation of invariant tori under a small analytic perturbation of a completely integrable Hamiltonian system. Later Kolmogorov said that he had been thinking about this problem for decades, starting from his childhood, when he read the book *Astronomy* by Flammarion; but success came only in 1953 after Stalin's death when a new epoch began in Russian life. Hopes which were aroused by this death made a strong influence on Kolmogorov, and the period 1953–1963 was one of the most productive in his life.

The initial point of the 1954 work on invariant tori was the mathematical practicum for second year students of the Department of Mechanics and Mathematics of Moscow University introduced by Kolmogorov as compulsory at a time when computers were practically unavailable in the USSR.[9] One of the offered problems was an investigation of integrable dynamical systems (geodesics of surfaces of rotations, motion of a heavy particle on a horizontal torus, and so on). To his surprise, conditionally periodic motion along invariant tori in phase space was observed in all these integrable systems.

---

the product $K \times K$ which intersects the factors a and b times respectively); let the real part of this correspondence be a diffeomorphism of the circle. How many periodic points of period n can it have (of course, provided that their number is finite, which need not be the case)?

[9] As a problem for this practicum, Kolmogorov gave each of several hundred students a system of differential equations from Hilbert's 16th problem: $\mathrm{d}x/\mathrm{d}t = P(x,y)$, $\mathrm{d}y/\mathrm{d}t = Q(x,y)$, where P and Q were quadratic polynomials with random integer coefficients. The task was to draw a phase portrait (and, in particular, to find the number of limit cycles). By this random sampling, Kolmogorov intended to find an example of a system with many cycles. But surprisingly it turned out that there was no one limit cycle in any of these randomly chosen systems. At the time, examples with three cycles were known, later Chinese mathematicians found examples with four cycles, but, up to now, even the boundedness of the number of cycles (uniform with respect to the coefficients of the polynomials P and Q) conjectured by Hilbert has not been proved.

In trying to understand this phenomenon, Kolmogorov investigated its abstract variant: a dynamical system on a torus given by a divergence-free field with respect to a volume element. In 1953, in a short paper published in *Doklady*, Kolmogorov proved that such a generic vector field (which satisfies some Diophantine conditions on mean frequencies that are valid almost always) is equivalent to a standard (translationally invariant) field on the torus.[10] Such a field determines quasi-periodic motion ("conditionally periodic," as Kolmogorov said using old-fashioned terminology). The system is ergodic (does not have nontrivial measurable invariant sets), but it does not mix up the particles of phase space (torus) which are carried over by the field flow with preserving their shape.

However, Kolmogorov discovered intermixing motions, not quasi-periodic ones, even in the case of analytic fields at some "exceptional" mean frequencies of revolution along the torus. This intermixing is explained by nonuniformity of motion along orbits filling the torus in a quasi-periodic way as parallel lines. Immediately a question arises: do such exceptional intermixing systems on tori have real importance for investigation of Hamiltonian dynamical systems?

Motion along tori is quasi-periodic (or periodic) in integrable systems. So, in order to find real applications of the abstract theory of vector fields on a torus, it is necessary to find invariant tori in nonintegrable systems. The simplest way to find them is to employ a variant of the perturbation theory of integrable systems. This was the method which Kolmogorov used in his research, and it is in this way that in 1954 he arrived at the theorem on conservation of invariant tori.

But the initial goal was not achieved. Motion along the invariant tori obtained by Kolmogorov is quasi-periodic. Are there invariant tori which carry over them intermixing flows in the phase space of a typical Hamiltonian system close to an integrable one? Up to now the answer has remained unknown. Kolmogorov supposed their existence, so that the effect (discovered in the short paper of 1953) is observed in generic Hamiltonian systems, close to integrable ones. It is interesting to note that the "partial" success of the paper of 1954 is of much greater importance than the technical matter on intermixing to which Kolmogorov tried to get an answer. Kolmogorov's achievement is similar to that of Colombus, whose attempt to find a Western route to India failed.

I came to Kolmogorov with my theorems. "Well," he said, "here is my paper of 1954 in *Doklady*. I think it will be good if you continue with this problem, try to think of applications to celestial mechanics and rigid body rotation. I am

---

[10] This theorem of Kolmogorov has a natural multidimensional generalization in the theory of polyintegrable systems (*St. Petersburg Math. J.*, 1992, **4**(6), 1103–1110).

very glad that you have chosen a good problem." But I was completely upset by this outcome, because it was just the opposite to my plan of complete independence. However, it was an interesting problem. A few days later I learned from N. N. Vakhania's thesis (which was defended at the Department of Mechanics and Mathematics of Moscow University) that this problem had been considered before me by S. L. Sobolev in a classified work of 1942 on oscillations of fluid in rotating missiles. Resonances are dangerous there, since they can destroy the tank. This theory is also related to Sobolev's equation (which was, in fact, first obtained by Poincaré in 1910).[11]

In this way I started to learn some mathematics. I read some other people's works and finally I discovered some papers by C. L. Siegel, who met Kolmogorov at Göttingen in the 1930s. Kolmogorov was not aware that Siegel had later worked on the small denominators problem. Siegel's paper was published in 1941, but was unknown to Kolmogorov. He knew about the works of Poincaré, of Denjoy and of Birkhoff, but not about Siegel. So he told me that we were in a very good company: "Siegel is really serious," he said.

I discovered a theorem of Siegel (which is related to the normal forms for circle rotations) due to the system of education in Moscow University, which was different from that in America. I think the Moscow system followed the German tradition that when you have a result and you wish to publish it, you have first to check the literature to see whether someone else had ever studied it. We were told this at our first introductory course in library work where they taught us how to find, starting from zero information, everything you need. Of course, there was no Internet at that time, but still we were able to find the references, and this is how I discovered Siegel's work.

The circle diffeomorphisms problem is related to many other problems, and I shall give you some examples. One of them is a problem which was also studied in classified works on the stability of missile and rocket shells. This stability problem is very important, because a shell must be very thin if it is to travel far. But you cannot, by the architecture of the system, avoid nonconvexity. In the convex part, you have good theorems by Cauchy that the metric determines the shape and thus the shell is inflexible. But, in the parts of hyperbolic curvature no one knows the answer. Even for the idealized problem of isometry – if you have, say, a torus in 3-space (this is one of the problems I like in mathematics) – no one knows whether it is flexible; that is, whether you can deform it without deforming the metrics. Only in some particular cases has the inflexibility recently been proved. (I have been told of the example of a rotationally symmetric

[11] Applications of the modern KAM theory to hydrodynamical problems have been discovered recently by A. Babin, A. Makhalov and V. Nikolaenko.

torus lying between two parallel planes.) But the general case is still open. By the way, some polyhedra are flexible, and there is a theorem that the flexion of a polyhedron homeomorphic to the sphere does not change its volume.[12]

Problems of the interior geometry of surfaces in Euclidean space are, in fact, closely related to the theory we are now discussing. Many years ago I conjectured that any germ of a function vanishing at the origin and having there a critical point of finite multiplicity is diffeomorphic to the Gaussian curvature of the graph of a smooth function $z = f(x,y)$. This conjecture was proved by the present author in January 1998 (see *Topol. Methods Nonlinear Anal.*, 1998, **11**(2), 199–206).

Let us return to the question of stability of missile shells. People who constructed these shells have observed that the geometry of the characteristics, which are the asymptotic lines of the shell surface, can present obstacles to inflexibility. The asymptotic lines define a dynamical system similar to that which leads us to the diffeomorphisms of the circle. Consider a hyperbolic cylinder between two horizontal circles (like a section of the Moscow TV tower designed by Shukhov). Moving upward from the lower circle along the asymptotic line ("element" of a hyperboloid of one sheet), we will arrive at a point on the upper circle. From this point, we can move downward along the other asymptotic line emanating from it. This line will come to a point on the lower circle, which we consider as the image of the initial point of the lower circle under the action of the dynamical system just constructed (which is a diffeomorphism of the lower circle onto itself).

This dynamical system is, in fact, related to the characteristics of wave equation for oscillations of a string, when it is wtitten in the form $\partial^2 f/\partial x \partial y = 0$. To represent a function as a sum of a function of x and a function of y means to solve this string equation. Our representability problem is thus the Dirichlet problem for the string equation. In the case of shells having the shape of a piece of a hyperboloid of one-sheet, you have the Dirichlet problem for a hyperbolic equation.

Professor Goldenweiser discovered that the resonances of the dynamical system on the boundary circle which depend on the shape of the hyperboloid, are responsible for the flexibility. As far as I know, this is not a theorem — there are some formal obstacles to inflexibility, but no mathematical proof of flexibility. People who studied these problems were doing real work, they were actually constructing shells. I have seen those shells: they are really flexible, but no one can prove that they are. It depends on the resonances. If you have

[12] Recently, the volume invariance at flexion was proved for all polyhedra (R. Connelly, I. Sabitov, A. Walz, 1997).

resonances, then they are flexible in your hands — but I have not seen any mathematical proof of the existence of resonances.

I have written a paper on this subject, applying the technology of small denominators that Kolmogorov had invented in 1954 and adding some new results. Working on the circle analytic diffeomorphisms, I came to some conjectures in what is now called holomorphic dynamics, which I was unable to prove. One of them (claiming that an analytic circle diffeomorphism with a good rotation number is analytically conjugate to a rotation, the bad numbers forming a set of measure zero) was proved by M. Herman some twenty years later. One of the others still remains a challenge and I shall formulate it here once more. It is a part of a general project on "resonance materialization," providing the topological reasons for series divergence in perturbation theory.

Consider an analytic diffeomorphism of a circle onto itself (defined by a holomorphic mapping of the neighboring annulus onto another neighboring annulus). Suppose that the mapping is analytically conjugate to an irrational rotation and that the closure of the maximal annulus where the conjugating holomorphic diffeomorphism is defined lies strictly inside the annulus where the initial holomorphic mapping is defined. The conjecture is that *there exist periodic orbits of the initial holomorphic mapping in arbitrarily thin neighborhoods of the boundary of the maximal annulus.* One is even tempted to conjecture that *the points of such orbits exist in any neighborhood of any point of the boundary of the maximal annulus.*

As far as I know, these conjectures are neither proved nor disproved, even for the standard circle mappings $x \mapsto x + a + b\sin x \pmod{2\pi}$ (for which the conjectures were first formulated in 1958) or for the generic mappings. In the special case of rational mappings, similar conjectures were proved recently by J.-C. Yoccoz and R. Perez-Marco. Counterexamples to the initial conjecture were also constructed, but only for exceptional rotation numbers (forming a set of measure zero). In 1958 I also formulated similar conjectures for the boundary of the Siegel disk (centered at a fixed point).

In the case where there exists no analytic conjugation to a rotation (and where the maximal annulus is reduced to a circle and the Siegel disk to the fixed point) I have conjectured at least the generic presence of close periodic orbits. To be more precise, fix a bad rotation number. Then, for generic analytic mappings with this rotation number, one should expect the presence of periodic orbits in any neighborhood of the invariant circle (of the fixed point). This conjecture has not been proved so far.

Dealing with these problems, I observed that to define what should and what should not be called generic in dynamical systems theory is highly nontrivial.

Indeed, both the topological and the probabilistic approaches provide pathological answers (studied by P. Halmos, V. A. Rokhlin and others). So the notion of "physical genericity" should be different from what mathematicians have suggested.

The topological definitions (using the "Baire second category sets," etc.) have the following defect. A phenomenon happening with positive probability (in the sense of the measure of the set of corresponding parameter values) might be negligible from the point of view of topological genericity (for instance, if this set of parameter values has an everywhere dense open complement). This happens, for example, in the very natural families of circle diffeomorphisms (like $x \mapsto x + a + b \sin x$). Such a diffeomorphism is close to a rotation if b is small. From the topological point of view they are "generically" structurally stable. The structurally stable circle diffeomorphisms have attracting and repelling periodic orbits. They correspond to the resonances and have rational rotation numbers. The complementary set of the nonresonant ergodic diffeomorphisms is "topologically negligible." But the ergodicity of the diffeomorphism happens with probability 99% if b is small, while the "generic" behavior is highly improbable!

The alternative probabilistic approach has a different defect — the corresponding measure is always concentrated on sets of functions with some specified smoothness. All the sets of functions which are smoother than this are then negligible (have zero probability).

To overcome these difficulties, I have proposed to call generic those events which happen when the parameter of the topologically generic finite-dimensional family of systems belongs to a set of positive measure in the finite-dimensional parameter space. For years I have been thinking that this definition of "physical genericity" was introduced by Kolmogorov in his Amsterdam talk (1954). However, recently Yu. S. Il'yashenko has explained to me that Kolmogorov used rather a dual definition and that I was perhaps the first one to introduce (in 1959) the notion of physical genericity described above (and now called "prevalence").

I tried to apply this philosophy to many problems; for instance, to the study of chaotic dynamics of the area-preserving mapping in the neighborhood of a hyperbolic fixed point whose separatrices have a homoclinical transversal intersection. My guess was that the positiveness of the measure of a Cantor set of the "Smale's horseshoe"[13] type (on which the dynamics is chaotic) should be a physically generic event. As far as I know, this conjecture is still neither

[13] Some decades before Smale, this horseshoe had been discovered and investigated by J. Littlewood and his student M. Cartwright in their (for some reason forgotten and rarely cited) works on nonautonomous second-order differential equations with periodic coefficients.

proved nor disproved. Its topological version (not referring to measure) was proved by V. M. Alekseev in a very general situation.

I was still an undergraduate student while I was doing this work. Once Gel'fand invited me to talk on the circle rotations, and when I explained my theorems to him, he said that they could be applied to what he was working on. He was working with M. L. Tsetlin on a mathematical model of the heart beat. In the heart, you have resonance between the ventricles and the atria. There is an atria-ventricular node and then there is an electric system synchronizing the ventricles and the atria. In the model of Gel'fand and Tsetlin this system was described by a mapping of the circle into itself.

My theorems were applicable, and I added several pages to my paper of 1959 on applications to the heart beat problem of the theory of resonances and structural stability of the mappings of a circle into itself and of the theory of small denominators. The paper was sent for publication to the journal *Izvestiya Akademii Nauk SSSR* (mathematical series), where I. M. Vinogradov was editor-in-chief, but it was rejected. Kolmogorov told me: "You should delete the part related to the Gel'fand theory." I was puzzled because I liked it, but Kolmogorov's reaction was that the heart beat theory, although very interesting, is not of the kind mathematicians should work on. "You should concentrate on the three body problem," he told me. This was the only mathematical advice I ever got from Kolmogorov.[14] When I deleted the part on the Gel'fand theory from the paper, it was accepted by Vinogradov and the shortened text appeared in the *Izvestiya* in 1961. Together with the heart beat theory, I also deleted a paragraph about the influence of small noise on the circle diffeomorphism invariant measure. Today these problems are included in the general Morse–Witten theory (but the discrete time case, which I studied, seems to remain still unsettled in the modern theory). Kolmogorov did not approve my naïve approach to the theory of asymptotics of solutions to the (discrete time) Fokker–Planck equation in the small diffusion limit — which was, of course, his kingdom.

My observation is that the influence of a small noise on the dynamics of a circle diffeomorphism results in that some attractors capture nearby points rapidly, generating Gaussian maxima of the phase mass density. Later, larger

[14] Later, when I was his graduate student (in 1961), Kolmogorov learned about the existence of differential topology from Milnor's talk in Leningrad. He immediately suggested that I should include it in my graduate curriculum (having in mind connection with the superposition problem). As a result, I started studying differential topology from S. P. Novikov, D. B. Fuchs and V. A. Rokhlin — and even served as an opponent for the *kandidat* thesis of Novikov (an ingenious topologist and the glory of Russian mathematics) on the differential structures on the products of spheres.

maxima are formed by attractors with larger attraction basins. However, eventually it is not the apparent candidate that wins, but "general attractor" — the winner of the tunneling race, determined by the greatest eigenvalue of the matrix corresponding to tunnel transitions from one attractor to another (I called this attractor "general" because it was just as difficult to predict it at the beginning as it was to guess who will be the next General Secretary of the USSR Communist Party).

Later I had to discuss C. Zeeman's results on the influence of small noise on a dynamical system. He rediscovered some results of A. A. Andronov, L. S. Pontryagin, and A. A. Vitt on this topic published in the 1930s. Although their paper was in English, Zeeman did not refer to it according to the Western tradition of discrimination against Russian contributions.

The deleted heart beat part of the paper lay on my shelf for 25 years. Then two events happened. The Canadian physiologist Leon Glass discovered that the mathematical theorems on resonances proved in my published paper have applications to heart beat. He published them in a paper and later in a book titled *From Clocks to Chaos*. About the same time Gel'fand told me that he was preparing his collected works. "My congratulations," I said, "I am very glad." "Yes," he answered, "but I want your paper to be published in it." I was puzzled, but since this was not the dangerous genuine collaboration, I gave him the old paper. And the paper was published almost simultaneously with the paper by Glass. The results were practically identical! Glass only added that his clinical tests of the patients at a hospital had substantiated my theory.

This was the story of how my work in what is now called KAM theory was started. Later I worked on the many body problem, following Kolmogorov's suggestion. Reading the *Méthodes nouvelles de la mécanique celeste* of Poincaré and having discussions with V. M. Alekseev during our weekly common "windows" (breaks between two classes) at Moscow University, I realized that the problem of celestial mechanics has several difficulties which one might tackle separately. The first difficulty ("the limit degeneration") is already present in the simplest problem on the plane area-preserving diffeomorphisms near a fixed point, the so-called Birkhoff problem. Suppose that the mapping linearized at a fixed point is a plane rotation. A rotation is *resonant* if the rotation angle is commensurable with 2π. If the linearized mapping is a nonresonant rotation, Birkhoff was able to reduce the mapping to a rotation (through a variable angle) using some symplectic (area-preserving) formal coordinate change. The celebrated problem, formulated by Birkhoff, was to decide whether the fixed point was stable in this case. The difficulty is that Birkhoff's series (reducing the mapping to the Birkhoff normal form, which is a rotation through an angle depending on the distance to the fixed point) is generically divergent, due to the isolated periodic orbits born at the places where the rotation angle is commensurable with 2π — these periodic orbits form the "materialization of resonances" in this problem. I had solved this Birkhoff's problem and the paper was presented to *Doklady* by Kolmogorov in 1960.

At the Stockholm Congress of 1962, Moser, speaking about his recent results on the Birkhoff problem, explained how to replace the analyticity assumption by continuity of the 333rd derivative. His method was not too far from Kolmogorov's 1954 paper, but the details were different. His result was even better than the solution of Birkhoff's problem: he had proved the stability provided that the rotation angles of the linearized mappings were not of the form $k\pi/2$ or $k\pi/3$. Rational numbers with denominators higher than 4 behave in this problem like irrational numbers! The resonances of order smaller than 5 are now called *strong* resonances, those of higher order are called *weak* resonances. Moser discovered that the stability holds even in the presence of resonances, provided that they are weak.

Listening to Moser, I immediately understood that my 1960 stability proof was applicable (for analytic mappings) to the case of weak resonances, while I had formulated the result only in the nonresonant case. Instead of studying the phenomenon, I was trying to solve a celebrated problem and was hypnotized by Birkhoff's formulation, which forbade all resonances. This was a good lesson: one should never be hypnotized by the authority of predecessors.

My first trip abroad was to the Stockholm Congress in 1962. My report was devoted to the stability problem of planetary systems, but the Panel (the Committee which chooses speakers) and the Program Committee did not consider planetary systems worthy of an invited lecture at a Mathematical Congress. Fortunately, at that time there were special sessions where uninvited speakers made 15-minutes talks. So I presented a short talk.

Organizing Committees of International Mathematical Congresses made a lot of effort to exclude uninvited reports. The question whether it is good or not causes arguments. The best lecture (and the most important for me) I have ever heard at an International Congress is the lecture by F. Hirzebruch at the Moscow Congress in 1966. He made a survey of E. Brieskorn's works about connection of the singularity theory with Milnor's spheres,[15] and he was not an

[15] Brieskorn's works suggest, for example, equations in $\mathbb{C}^5$ such as

$$x^a + y^3 + z^2 + u^2 + v^2 = 0, \qquad |x|^2 + |y|^2 + |z|^2 + |u|^2 + |v|^2 = 1$$

($a = 6k - 1$, $k = 1, \dots, 28$) for smooth 7-dimensional Milnor spheres which are all homeomorphic to the standard sphere S^7 but not diffeomorphic to each other.

invited speaker. I think that the harm from uninteresting talks made by uninvited speakers is less than the loss made by rejection of important and interesting lectures. It is unlikely that Galois would have been invited to a Congress. In 1992 V. A. Vassiliev was not invited to the First European Mathematical Congress in Paris, though four invited speakers talked about his works. [16]

The second main difficulty of the planetary motion problem was the so-called "proper degeneration." The terminology was introduced, I guess, by M. Born. I found it in his remarkable *Atomic Mechanics* (published in Khar'kov in 1934) where three-dimensional Lagrangian manifolds were present. The point is that some of the frequencies of the quasi-periodic motion of the perturbed system might be small together with the perturbation parameter. The simplest cast is the theory of adiabatic invariants. Consider, for instance, the motion of a charged particle along a surface under the influence of a strong magnetic field which is orthogonal to the surface. Mathematically, this involves the problem of the description of the curves of prescribed large geodesic curvature on the surface. In the first approximation, such a curve is a circle of small radius, the so-called Larmor circle. But in the next approximation (provided by the theory of adiabatic invariants), the center of the Larmor circle starts to move along the surface. The drift of the Larmor circle is described by the averaged system. In the adiabatic approximation, the center moves along the level line of the prescribed geodesic curvature (that is, the line where the intensity of the given magnetic field is constant). In the case of a constant magnetic field intensity the drift occurs in a higher order approximation. In this case the center of the Larmor circle follows the level line of the Gaussian curvature of the surface.

On a compact surface, a typical approximate trajectory of the Larmor center is a closed curve, and one may ask whether the genuine orbits of the charged particle remain close to these closed trajectories. The theory of the proper degeneration that I had constructed (the paper was presented to *Doklady* by Kolmogorov in 1961) gave a positive answer to this question, also providing many other physically important results on the infinite time behavior of adiabatic invariants.

Just at that time these physical problems were formulated at Kolmogorov's seminar on dynamical systems by two well-known physicists, M. A. Leontovich and L. A. Artsimovich, who related them to the plasma confinement problem important for the controlled thermonuclear reaction project. Kolmogorov suggested

[16] Knot invariants discovered by Vassiliev are in the same position among all invariants as are polynomials among all functions. Vassiliev's theory of invariants (later linked with quantum field theory by Kontsevich) is one of the major accomplishments in the twentieth century mathematics that connected singularity theory and topology with combinatorics and Feynman integrals.

that I send the resulting paper to *JETP — Zhurnal Eksperimental'noi i Teoreticheskoi Fiziki*, the main physical journal in the USSR. A few weeks later, Leontovich (who was, as far as I remember, the vice-chairman of the editorial board) invited me to his home, near the Atomic Energy (now Kurchatov) Institute to discuss the paper. Leontovich, who headed the theoretical physics division of the thermonuclear controlled reaction project, was a friend of Kolmogorov and also of my father (he helped our family to survive when my father died and I was 11 years old). Treating me, as usual, with buckwheat porridge and calling me, as usual, "Dimka" (he used this nickname until his death some 20 years later), Leontovich explained to me the reasons why the paper cannot be published in *JETP*:

i) the paper uses the forbidden words "theorem" and "proof";

ii) the paper claims "*A* implies *B*" while every physicist knows examples showing that *B* does not imply *A*;

iii) the paper uses nonphysical notions like "Lebesgue measure," "invariant tori," "Diophantine conditions."

He proposed that I should rewrite the article. Now I understand how right he was trying to defend a physical journal from the Bourbakist mathematical style.[17]

An author, claiming that *A* implies *B*, must say whether the inverse holds, otherwise the reader who is not spoiled by the mathematical slang would understand the claim as "*A* is equivalent to *B*." If mathematicians do not follow this rule, they are wrong. Nowadays, every physicist studying Hamiltonian chaos or using KAM theory in plasma confinement or accelerator control problems, freely uses the Lebesgue measure, the invariant tori and the Diophantine conditions. But in 1961 one of the first papers on what is now called KAM theory was, as we see, rejected by a leading physical journal for the use of these words (and also words "theorem" and "proof").

I took the paper back from *JETP* and it appeared a year later in *Doklady*. By that time I had already combined the study of degenerations of both kinds and applied them to the planetary motion problem. The results were first presented at a conference on theoretical astronomy held in Moscow on 20–25 November 1961. The main topic of the conference was the motion of artificial satellites. I was delighted to meet there and make friends with M. L. Lidov, whose students

[17] Rumors later reached me that the paper had been reviewed by L. D. Landau, but I do not know if this was indeed the fact.

A. I. Neishtadt and S. L. Ziglin later made profound contributions to perturbation theory, averaging, adiabatic invariants, Hamiltonian chaos and the materialization of resonances. The resulting theories are well known.

Of Lidov's results, I would point out the study of evolution of an Earth's satellite orbit which is initially a circle of the same radius as the Moon's orbit but inclined significantly to the ecliptic (say, forms an angle of 80° with the Earth's orbit). It turns out that, in case of so large inclination, the Moon would fall onto the Earth within only four years! The "Laplace theorem" on the stability of planetary orbits that I have proved assumes that the mutual inclinations of unperturbed orbits are small.

Now you have almost the whole picture of all my mathematical subjects. They all start from this problem of superpositions, and you now see how they are connected. There is one more topic, namely hydrodynamics and hydrodynamical stability, but this is also related to the same origin. Such mysterious correlations between various fields of mathematics, which at first sight are not connected at all, have remained a puzzle to me. Discoveries of such correlations is the greatest pleasure provided by mathematics; and I am very lucky to have experienced it several times in various fields.

When I finished the works on celestial mechanics and on other applications of what is called KAM, I tried to find some applications of the theory of dynamical systems to continuous systems, in particular, to hydrodynamics. Kolmogorov, of course, was also a classic hydrodynamicist and he had a seminar at that time (1958–1959) called "Seminar on dynamical systems and hydrodynamics," where the celebrated work by L. D. Meshalkin and Ya. G. Sinai was done on Kolmogorov flows instability and continuous fractions, where the Kolmogorov–Sinai entropy was invented, and so on.

In 1961 S. Smale came to Moscow. He was the first foreigner I met in my life. We discussed a lot of interesting projects on the roof of Moscow University (he speaks of "the steps of Moscow University" in his reminiscences). Among other things we discussed structural stability and he formulated the conjectures that torus diffeomorphisms and geodesic flows on negatively curved surfaces should be structurally stable. I have even written a paper with my friend Sinai, proving the first conjecture. While describing this proof at my seminar on dynamical systems, I suggested that one might prove the second Smale's conjecture, identifying the perturbed geodesic with the nonperturbed one, connecting the same two points at the absolute. The following week, D. V. Anosov reported his proof of this conjecture, but my proofs were wrong, since I was using too many derivatives of the invariant foliation whereas Anosov had shown that they might not exist.

And this is why I have never tried again to prove collective theorems. This happened in 1961–63, and since that time I have been trying to find applications of this philosophy of structural stability. My first idea was to think of the model of hard spheres in statistical mechanics. I speculated that such systems might be considered as the limiting case of geodesic flows on negatively curved manifolds (the curvature being concentrated on the collisions hypersurface).

The simplest model of this kind is a system of two elastically colliding disks on the surface of a two-dimensional torus. Alternatively, one can consider the motion of a particle along a torus with a hole that elastically repels this particle. The latter system can be regarded as a motion along a two-sided torus, each repulse entailing transit to the other side, and also as a geodesic flow on a pretzel whose surface is homeomorphic to a two-sided torus with a hole and whose curvature is concentrated along the hole's edge taking large negative values there.

I had never proved anything in this direction,[18] but I explained this idea to the greatest expert on dynamical systems and ergodic theory that I knew (Sinai) and he started a long series of works continued by many people (let me mention only the recent works by D. Szász and N. Simányi — the project is still alive and not exhausted). My second project was to apply the new theories of dynamical systems to hydrodynamics. I started to discuss this work already in 1961–1962 with V. Yudovich and O. Ladyzhenskaya.

During that period, most of the mathematicians working in this area (including Leray and Ladyzhenskaya) thought that the turbulence may be explained only by the fact that the equations of three-dimensional hydrodynamics do not have unique solutions (their attempts to prove the uniqueness had lasted many years without success). Both Leray and Ladyzhenskaya strongly opposed my idea that the observed phenomena are due not to nonuniqueness but to instability, that is, to a fast increase of originally small perturbations of the initial conditions. Mathematically, such increase means that the Lyapunov exponents of the dynamical system are positive.

The idea was that because of the high sensitivity of the flows on a surface of negative curvature, the positive Lyapunov exponents are stable. The Euler and Navier–Stokes equations contain many "parameters": the domains and the exterior forces. I conjectured that one might find somewhere, at least numerically, an attractor of the Navier–Stokes equation on which the geodesic flow of a negatively curved surface is realized. It was of course very naïve, but in

[18] "To approach formulas of chaos and Brownian motion of capricious particles with the lucid light of logic and intellect is the same as eating a jelly with a needle." — Tatiana Tolstaya, "The Russian World"; page 402 in: *The Day*. Moscow: Podkova [The Horseshoe], 2001 (Russian).

1964 I made some numerical experiments (with the help of N. Vvedenskaya) on a model with six Fourier modes. Unfortunately, I was unable to find the positive Lyapunov exponent numerically. At that time, computers produced very, very long tapes with numbers, kilometers of numbers. We were trying to imagine the orbit in 6-dimensional phase space looking at those numbers. I think that probably the Reynolds number was not sufficiently high, so what we observed was a 3-dimensional torus in 6-dimensional phase space – a scenario predicted by Landau. But I was sure that with more work one might find the positive Lyapunov exponents, and perhaps even the geodesic flow on a surface of negative curvature. This was the reason for my 1966 paper on the differential geometry of infinite dimensional Lie groups, on the diffeomorphism groups which are the configuration space in hydrodynamics. I have calculated the curvature of this group[19] and I even used it to show that weather prediction is impossible for periods longer than 2 weeks. In a month you lose 3 digits in the prediction, just because of the negative curvature calculated by me. This instability is not the Euler instability, it's not describing a chaotic attractor of the Euler equations – but it comes from the same line of ideas. Thus all my hydrodynamical works were the byproduct of the works on dynamical systems, which in turn were the byproduct of the works on the Hilbert problem.

In trying to study the slow mixing in Hamiltonian dynamics, I have introduced the "interval exchange" model (as the simplest discrete time description of the event in the pseudo-periodic system whose Hamilton equations are defined by a closed, but not exact, 1-form on a surface of genus 2), being interesting already in the case of three intervals of integer lengths, $(a,b,c) \to (c,b,a)$.[20] This model is so natural that I am always amazed not to find it in works on ergodic theory prior to my 1963 paper, where it was introduced.[21] I have returned to pseudo-periodic topology many times since 1963. Pseudo-periodic functions are

[19] A few years earlier I had translated Milnor's wonderful *Morse theory* into Russian. My calculations in the 1966 paper were based on his short description of the Riemannian geometry. Milnor later (in 1972) proved the formulas for the curvature of a left invariant metric on a Lie group, which are essentially equivalent to my coordinate-free formulas in the hydrodynamics paper.

[20] The exchange of intervals of integer lengths (a,b,c) can be regarded as a permutation of $n = a+b+c$ elements. This permutation splits into permutations consisting of several cycles of equal length (like a rotation of a regular n-gon). It is interesting to study the statistics of lengths of the cycles of the resulting permutation and its limit for large n, assuming that all decompositions of n into three summands are equiprobable. For example, how often do all the cycles have the same length?

[21] My colleagues told me that the first research on this problem about interval exchange, posed by me as early as 1950s, had been published by the participants of Sinai's seminar before my publication of the problem and with a reference to me (which, however, disappeared in further publications on this subject).

sums of linear functions and periodic functions, like $f = ax + by + \sin(x+y)$. Pseudo-periodic manifolds are those defined by pseudo-periodic equations, like the plane curve $f = 0$ (think of the Pacific coast of California and try to understand whether such a curve may have many unbounded components — a typical problem of the young pseudo-periodic topology, to which the interval exchange model also belongs).

The present state of research in pseudo-periodic topology is presented in the book *Pseudo-periodic Topology* (edited by V. I. Arnold, M. L. Kontsevich, and A. V. Zorich; Providence, RI: Amer. Math. Soc., 1999; AMS Transl., Ser. 2, **197**; Adv. Math. Sci., **46**). While studying the interval exchange model, Zorich discovered (by computer experimentation) astonishing new laws of correlation decay in such systems. In a recent work, Zorich and Kontsevich were able to explain most of these observations, relating them to the ergodic theory of geodesic flows on the Teichmüller spaces. The study of the interval exchange model has thus returned to the nonexact Hamiltonian pseudo-periodic topology on higher genus surfaces, which was the initial motivation for the publication of this model in 1963.

My recent paper (*Funct. Anal. Appl.*, 2002, **36**(3), 165–171) also applies to pseudo-periodic (or quasi-crystallic) topology. The paper is devoted to Harnack's quasi-crystallic theorem providing ergodic mean values of various topological and geometric invariants of pseudo-periodic functions and manifolds (like the numbers of critical points or maxima, Betti numbers, or the numbers of manifolds' components, etc.) via powers or Newton polyhedra of Fourier series giving periodic parts of pseudo-periodic functions. The existence of mean values (almost everywhere) has been proved by S. M. Gusein-Zade.

In the late 1960s I also explored some other areas related to dynamical systems with my undergraduate students:

— G. Margulis (in his first and unpublished paper he started the theory of Diophantine approximations on submanifolds of Euclidean space, later continued by A. Pyartli, A. Neishtadt, V. Bakhtin and used by M. Sevryuk);

— D. Kazhdan (who studied the ergodic properties of the Euclidean actions of free groups, continued later by R. Grigorchuk);[22]

— N. Nekhoroshev (whom I have persuaded to apply the Diophantine net geometry to the problem of the action's drift);

[22] Now I would like to carry over these studies to the orbits of the modular group $SL(2,\mathbb{Z})$ on the de Sitter world (which is an analytic continuation of the Klein model of Lobachevskian geometry from the interior of the unit circle to its complement in $\mathbb{R}P^2$, that is the Möbius band, or to the hyperboloid of one sheet doubly covering this Möbius band).

— A. Kushnirenko (slow mixing, structural stability of analytic actions of semi-simple groups — later, Newton polyhedra and fewnomials conjecture);

— A. Khovanskii (nonsolvability of differential equations, later — Newton polyhedra and fewnomials theory).

I turn now to the KAM (Kolmogorov–Arnold–Moser) theory. People say that there is even a KAM theorem, but I have never understood what theorem it is. In 1954 Kolmogorov proved his marvellous theorem on the preservation of the tori in Hamiltonian systems, when the Hamiltonian is almost integrable and all functions are analytic. What I have contributed was the study of some degenerate cases — when one of the frequencies vanishes in the nonperturbed system, or when one considers vicinities of the fixed points, or periodic points, or tori of a smaller dimension — and then applications to celestial mechanics. All these facts are separate theorems. My main contribution was the discovery (in 1964) of the universal mechanism of instability in systems which have many degrees of freedom, and are almost integrable, — later called "Arnold diffusion" by the physicists. This "diffusion" contradicted Kolmogorov's intuition. He thought that stability can be preserved in generic multidimensional systems in spite of the fact that in these cases stability is not provided by the existence of the invariant tori.

In systems with the phase space of small dimension, invariant tori close areas between them and in this way ensure stability (for example, in Birkhoff's problem). In my 1964 paper, I constructed an example of instability in a situation where Kolmogorov tori are preserved. I supposed then (and I suppose now) that the diffusion mechanism described there works in generic systems. So, there exist trajectories connecting a vicinity of any invariant n-dimensional torus, close to an integrable Hamiltonian system, with a vicinity of another torus on the same hypersurface of an energy level (if the dimension of this hypersurface $2n-1 \geqslant 5$, that is, if $n > 2$. However, this has not been proved.

In 1962 Moser extended Kolmogorov's theorem to the case of smooth functions.[23] In the first papers of Moser the number of derivatives was enormous.

[23] It is interesting to note that when Moser's papers appeared, some American mathematicians began to publish papers that "extended the Moser theorem to the case of analytic functions." Moser himself has never supported these attempts to attribute Kolmogorov's results to him.

I permanently failed to comprehend some details of Moser's proof, but eventually managed to write down my own version after he had explained his basic ideas to me. Each proof is based on a composition of Kolmogorov's method and smoothing suggested by Nash. The smoothing technique relies on inequalities proved by Kolmogorov, that estimate every derivative of a function

Now we know that in the simplest case of plane rotation you only need 3 derivatives, and this is just the limit, the critical number of derivatives. But in the beginning the number was 333. Kolmogorov told me that this was like a complete change of philosophy, because he was expecting (and even claiming in his Amsterdam talk) that the result should be wrong even in C^∞ and that one needs analyticity or something close to it, like the Gervais condition.

Moser regretted that a proof of the theorem in the case of analytic Hamiltonians was never published by Kolmogorov. I think that Kolmogorov was reluctant to write the proof, because he had other things to do in his remaining years of active work — which is a challenge, when you are 60. According to Moser, the first proof was published by Arnold. My opinion, however, is that Kolmogorov's theorem was proved by Kolmogorov.

Thank you for your attention.

Question (*J. Milnor*)**:** You have often told us about important mathematical work done in Russia which we did not know about, and you gave another example today. I wonder if you can explain to us how to locate something interesting in the literature starting with zero information.

Answer: First of all (it is especially important for the Americans), do not forget that some mathematical results appear in Russian, in French, in German, in Japanese... To learn the state-of-the-art in a domain which is new to me, I usually start with the German *Encyclopædia of Mathematical Sciences*, edited by Felix Klein and published around 1925. It contains an enormous amount of information. Then there are papers in the *Jahrbuch* which was published before the *Mathematical Reviews* and *Zentralblatt* have been organized — it is full of information. Then, I usually consult the collected works of Klein and Poincaré. In Klein's *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert* there's a lot of information on what happened in the nineteenth century and before.

Other books by Klein are also extremely informative. For instance, there you can find the articles by Emil Artin on continued fractions and on braid groups, and I think you can find there the 1918 article by Radon, which has never been published and which contains the first draft of the Berry phase theory. It is the theory of the adiabatic pendulum, slowly moving along a surface. According

---

on a cube via the function itself and its higher derivatives. The simplest of them (such as $|f'|^2 \leqslant C|f|\,|f''|$) had been proved earlier by Hadamard and Littlewood, and Kolmogorov's work on these inequalities contained fundamental ideas of what is now called the "theory of optimal control," in which investigation of these inequalities is naturally included as a particular (in fact crucial) case.

to Radon, the Levi-Civita connection can be defined by the theory of adiabatic invariants. You have the fast phase oscillations in a system, like a pendulum, located at a point of the surface. You slowly move the point along the surface, and the direction of the pendulum's oscillations is parallel transported according to the Levi-Civita connection. I think this is the most physical way to define the Levi-Civita connection which otherwise is mathematically a rather complicated thing in higher dimensions. The adiabatic transportation defines it as a physically natural object. I think this can't be found in any textbook, I only found it in Klein's book.

To one's regret, all these books are now thrown out of student libraries of Parisian universities (they told me there that viruses contained in these old editions were mortally dangerous for Bourbaki's books). Klein was thrown out together with L'Hôpital and Goursat, Darboux and Picard.

It is therefore not surprising when "modern mathematicians" keep on ascribing all discoveries to the person of their last acquaintance. I would mention, for example, the "WKBJ method" (which was systematically used by Kelvin and Green a century ago and published some more decades before that by Jacobi with a reference to Carlini's book of 1816).

Then, from the 1940s start the *Mathematical Reviews* and the *Zentralblatt*, and later the Russian *Referativnyi Zhurnal "Matematika"*. After that it's more or less satisfactory. Of course, the *MR* and *Zbl* are not sufficient, because if you are trying to find a Russian paper and if in the Russian paper it was written that *A implies B*, then in the translation and hence in the *MR* you will usually find that *A is implied by B*. However, if you understand the topic you can reconstruct the author's correct statements.

Also, in Russia mathematics has never been completely separated from physics, and especially mechanics. There were the same people doing mathematics, mechanics and physics. For example, in Kolmogorov's selected works there is a paper by Kolmogorov and Leontovich (who was a famous physicist) on the neighborhood of a Brownian trajectory. This is a paper by a mathematician and a physicist which consists of two parts; the mathematical part containing evaluations of integrals, asymptotics, Riemannian surfaces, monodromies, Picard–Lefschetz theorem etc., and the physical part containing the background equations and so on. And, *of course*, the mathematical part was written by Leontovich, and the physical part by Kolmogorov. This is very typical for Russia.

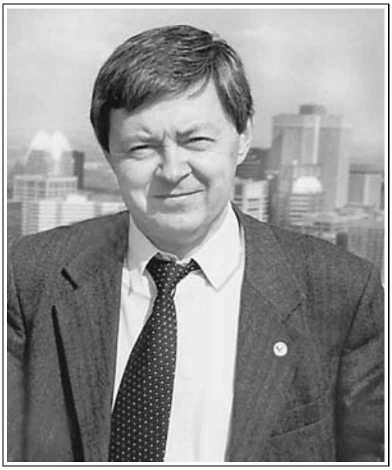
Another useful rule is that you can usually learn a lot about the state-of-the-art in some domain from your neighbors. Many times I have used the opportunity to pose silly questions to D. B. Fuchs, S. P. Novikov, Ya. G. Sinai,

D. V. Anosov, V. M. Alekseev, V. A. Rokhlin, and later to my own students. Once I asked the greatest number theorist I knew, whose works in many domains of mathematics I always admired, a question in number theory. His answer was, "Sorry, I have forgotten all of it, I am no longer a number theorist: several months ago, I have turned to another domain, logic." "Well," I said, "can you recommend to me a graduate student of yours still interested in number theory, to explain to me what is known?" "How naïve you are," he replied, "to think that my students may continue to be interested in number theory while I have turned to logic already three months ago!"

To facilitate the search for mathematical information, Russian mathematicians have tried to cover most of present day mathematics in the more than one hundred volumes of the series *Sovremennye Problemy Matematiki* (published by VINITI), several dozens of which have already been translated into English as *Encyclopædia of Mathematical Sciences*. The idea of this collection was to represent living mathematics as an experimental science, as a part of physics rather than the systematic study of corollaries of arbitrary sets of axioms, as Hilbert and Bourbaki had proposed. I hope that this Encyclopædia is useful as the source describing the real origins of mathematical ideas and methods (see, for instance, my paper on catastrophe theory in Volume 5). Unfortunately, in the Library of Congress, and hence in all libraries in the USA, the volumes of the *Encyclopædia of Mathematical Sciences* are scattered according to the author/subject alphabetical order, which makes its use as an encyclopedia extremely difficult. I have seen, however, the entire collection arranged on one shelf in certain European universities, for example, some in France.

Of course, in spite of all these precautions, you may discover too late that your result was known many years ago. It has happened to me to rediscover the results of many mathematicians. And I am especially grateful to Professor Milnor who explained to me the relation between the works of G. Tyurina and my paper of 1972 on the classification of A, D, E singularities, which is dedicated to her memory.

The present article is based on the first of three lectures delivered by the author at the conference held on the occasion of his 60th birthday at the Fields Institute (Toronto) in June 1997: "From Hilbert's superposition problem to dynamical systems." In: The Arnoldfest (eds. E. Bierstone, B. A. Khesin, A. G. Khovanskii and J. E. Marsden). Providence, RI: Amer. Math. Soc., 1999, 1–18 (Fields Inst. Commun., **24**). Some material has been added from the article "From superpositions to the KAM theory" originally published at the same time in: *Vladimir Igorevich Arnold. Selecta–60*. Moscow: PHASIS, 1997, 727–740 (Russian).



A. A. Bolibruch

Inverse Monodromy Problems of the Analytic Theory of Differential Equations

Translated by R. Cooke

1. Introduction

The foundations of the analytic theory of differential equations were laid in the work of nineteenth-century mathematicians: Cauchy, Briot and Bouquet, Fuchs, Picard, Painlevé, and others.

By the end of the first quarter of the twentieth century the main problems of this theory involving linear equations, such as Hilbert's 21st problem (the Riemann–Hilbert problem) or the problem on the Birkhoff standard form were considered to have been (positively) solved. In a certain sense, this mathematical discipline found itself on the periphery of the development of mathematics for a while.

However, after the discovery of the method of isomonodromic deformations in the early 1970s, the analytic theory of differential equations received a new powerful impetus for its development. It turned out that many famous nonlinear equations of mathematical physics can be interpreted as the equations of isomonodromic deformations of systems of linear differential equations. Here one can obtain important information on the behavior of solutions of these equations by studying suitable isomonodromic deformations of linear systems. But, to construct an isomonodromic family one must first solve the inverse problem of monodromy theory, the Riemann–Hilbert problem. Thus, this problem again wound up at the center of attention of many mathematicians.

In its most general formulation, the inverse monodromy problem can be stated as follows. The system

$$\frac{\mathrm{d}y}{\mathrm{d}z} = B(z)y \tag{1}$$

of differential equations with meromorphic coefficients and a set of singular points $D = \{a_1, \ldots, a_n\}$ on the extended complex plane $\overline{\mathbb{C}}$ defines *generalized monodromy data*, among which are:

- the monodromy matrices $G_1, \ldots, G_n$ of the system (the matrices that characterize the branching of solutions at singular points);
- the formal monodromy matrices and the exponential parts of formal solutions at singular points (that is, of "solutions" which are written in the form containing power series with zero radius of convergence, but which turn the equation into a true equality when substituted formally into it);
- the Stokes matrices at singular points (the matrices that show how the exponential asymptotics of the solutions of the system varies from one sector to another when encircling a singular point);
- the transition matrices between distinguished local bases at singular points;
- the orders R_i of the poles of the coefficient matrix of the system at singular points.

The generalized inverse monodromy problem is *to construct a system of linear differential equations* (1) *with given singular points and given generalized monodromy data.*

In this general formulation the inverse monodromy problem began to be studied intensively only in comparatively recent years (since the 1970s in connection with the appearance of the abovementioned method of isomonodromic deformations). The advantage of an approach like this is that the classical Riemann–Hilbert problem (Hilbert's 21st problem for linear Fuchsian systems) and the problem of reduction to standard Birkhoff form become special cases of this generalized problem and can be studied from a unified algebraic-geometric point of view.

In what follows we shall describe the tangled history of the study of these problems and the interesting applications that arose as a result of the study.

2. Hilbert's 21st Problem

Hilbert's 21st problem can be stated for a special class of linear differential equations in the complex plane, the so-called *Fuchsian equations*.

We recall that a linear differential equation

$$y^{(p)} + q_1(z)y^{(p-1)} + \cdots + q_p(z)y = 0 \tag{2}$$

with meromorphic coefficients is called *Fuchsian* at a point a_i if the order of the pole of the coefficient $q_i(z)$ at this point is i. The system (1) of linear differential equations is called Fuchsian at a_i if $B(z)$ has a simple pole at that point. An equation is called Fuchsian if it has only Fuchsian singular points in the extended complex plane $\overline{\mathbb{C}}$. Informally speaking, a Fuchsian equation is an equation having the most elementary kind of singularities at its singular points.

Fuchsian equations possess the following remarkable property: All of their solutions have at most power growth at singular points. (Since the solutions in this case are in general multi-valued functions, when we talk about power growth we must restrict ourselves to the case when the argument z tends to the singular point without "wiggling," always remaining in some sectorial neighborhood of the point.) In such a case we say that the corresponding singular points are *regular singular points* for the equation. Otherwise the singular points are called *irregular*.

A classical result of Fuchs says that, for a scalar differential equation (2), being Fuchsian is equivalent to being regular (see [Ha]). As for the system (1), the situation is different — the class of systems with regular singular points contains the class of Fuchsian systems, but does not coincide with it.

An important characteristic of the equation on the extended complex plane $\overline{\mathbb{C}}$ is its monodromy, which describes the character of the branching of solutions at singular points.

We choose a basis in the solution space X of Eq. (2) (resp. system (1)) in a neighborhood of a nonsingular point z_0 and continue it along some loop enclosing singular points. After such an extension the chosen basis becomes some (generally different) basis of the same space X. The transition matrix between these bases is called the monodromy matrix corresponding to the chosen loop. It turns out that this matrix depends only on the homotopy class of the loop (that is, the loop can be continuously deformed, avoiding singular points, without changing the monodromy matrix), and the monodromy matrices $G_1, \ldots, G_n$ corresponding to continuations along simple loops enclosing the singular points $a_1, \ldots, a_n$, satisfy *a priori* the single relation $G_1 \cdots G_n = I$, where I denotes the unit matrix.

When a different basis is chosen, all the monodromy matrices simultaneously conjugate by some nonsingular matrix S; that is, the monodromy equation is unique up to conjugation.

In 1857 Riemann [Ri] studied the problem of recovering a Fuchsian equation from its singular points and monodromy matrices. At the International Mathematical Congress in Paris in 1900, Hilbert included this problem as number 21 in his list of famous problems, stating it as follows (see [Hi]):

> *Show that there always exists a linear Fuchsian differential equation with prescribed singular points and a prescribed monodromy group.*

We should note the rather rigid categorical formulation of the 21st problem: Hilbert is not asking whether there exists such an equation (for example, *does there or does there not exist*..., but simply states the corresponding existence theorem.

What is the motivation for this problem? Why is it of interest? What brought it up? Hilbert himself writes very little about it. He merely notes that the solution of this problem would finish off the analytic theory of linear differential equations. That is, he advances a purely intramathematical motivation. But, as often happens with the Hilbert problems, even when formulated in such a narrowly specialized sense, they have subsequently turned out to be important both for mathematics as a whole and for its applications. We shall return to this question a little later. Right now, let us tell a little of the history of the study of this problem.

Historically speaking, the following three versions of Hilbert's 21st problem have been studied:

- for Fuchsian scalar equations;
- for systems of linear differential equations with regular singular points;
- for Fuchsian systems of linear differential equations.

It followed from the papers of Poincaré, published before the statement of Hilbert's 21st problem, that for scalar differential equations this problem has a negative solution, as follows from counting the number N_e of parameters in Eq. (2) and the number N_m of parameters in the set of monodromy conjugacy classes. For Eq. (2) with n singular points the difference $N_m - N_e$ is $N_d = (n-2)p(p-1)/2 - p + 1$ (see [Poi]), and therefore the construction of a scalar Fuchsian equation from a given monodromy, in the case when the number n of singular points is larger than 3 or when the order p of the equation is larger than 2, requires the introduction of N_d additional singular points, called "false" points; that is, points at which the coefficients of the equation have poles, but the solutions are holomorphic. The statement of the Riemann–Hilbert problem, however, does not admit the appearance of any new singular points.

The most difficult version of the 21st problem, and the most important for applications, was the one involving the construction of a Fuchsian system of differential equations, which is often traditionally called the Riemann–Hilbert problem. That is the problem we shall discuss from now on.

We begin by remarking that, as noted in the introduction, the Riemann–Hilbert problem is a special case of the generalized inverse monodromy problem: in this case all the orders R_i of poles are equal to 1, and there are no formal monodromy matrices, Stokes matrices, or exponential parts of formal solutions. (Every formal solution of a Fuchsian equation is a genuine solution — see [Wa] or [Ha].)

For a long time it was believed that the Riemann–Hilbert problem had been solved completely by J. Plemelj in the 1908 paper [Pl]. Plemelj's elegant paper, which relied on the use of the theory of Fredholm integral equations, was a pure existence theorem and gave a complete solution of the problem that had been posed.

However, in the early 1980s, gaps were found in his proof ([AI] and [Koh]). The method of solution proposed by Plemelj amounted to reducing the Riemann–Hilbert problem to the so-called homogeneous Hilbert boundary-value problem of the theory of singular integral equations. Using the theory of Fredholm integral equations, Plemelj constructed a system with regular singular points having the given singular points and a prescribed monodromy.

Plemelj went on to apply a certain procedure that made it possible to pass from the constructed system to a different one with the same monodromy and the same singular points, and which was now a Fuchsian system at all points with at most one exception. This part of Plemelj's proof evoked no objections. As for his assertion that the system could also be brought into Fuchsian form at this last point, there was no rigorous proof in his paper. However, Plemelj's reasoning can be completed if one of the monodromy matrices G_i is diagonalizable (see [AI]).

Thus the solvability of the Riemann–Hilbert problem was proved in Plemelj's paper for the case when one of the monodromy matrices G_i corresponding to a circuit of a point a_i around a "small" loop is diagonalizable. Plemelj was also the first to solve this problem in the larger class of systems with regular singular points.

After the publication of Plemelj's paper, the content of papers involving the Riemann–Hilbert problem shifted mainly into the area of effective construction of the matrices of a Fuchsian system from prescribed monodromy matrices $G_1, \ldots, G_n$. In the late 1920s I. A. Lappo-Danilevskii [LD] used the method of analytic functions of matrices that he had developed to represent the solutions of a Fuchsian system and the monodromy matrices $G_1, \ldots, G_n$ as convergent series of matrices in the coefficients of this system. The effective solution of the Riemann–Hilbert problem reduced in this case to inversion of the resulting series and the study of the question of convergence, which was solved positively in his paper for matrices $G_1, \ldots, G_n$ near the identity. Lappo-Danilevskii had thereby proved the solvability of the Riemann–Hilbert problem for monodromy matrices $G_1, \ldots, G_n$ near the identity.

In 1956, the solvability of the Riemann–Hilbert problem for 2×2 monodromy matrices ($p = 2$) was proved in the case of three singular points by B. L. Krylov [Kr], who constructed an effective solution of the problem. The analogous problem for four singular points was studied by N. P. Erugin [Er].

A new stage in the study of the Riemann–Hilbert problem was opened by the 1957 paper of H. Röhrl [Ro], who was the first to apply the methods of the theory of fiber bundles to it. Actually, considerations of this type go back to George Birkhoff [Bi1], who proved Plemelj's result over again in [Bi2] (and made the same mistake as Plemelj in the part involving Fuchsian systems). But at the time there was no adequate geometric language for a corresponding description.

From the monodromy matrices and singular points, Röhrl constructed the principal fiber bundle F on $\overline{\mathbb{C}} \setminus D$ with structure group $\mathrm{GL}(p;\mathbb{C})$. This fiber bundle turns out to be holomorphically trivial, so that the system of equations constructed from the holomorphic trivialization of this fiber bundle defines a system (1) with the prescribed singular points and monodromy. Röhrl then extended the fiber bundle F to the entire Riemann sphere $\overline{\mathbb{C}}$. The extended fiber bundle always has a meromorphic section, holomorphically invertible except at points of D. The system (1) constructed from this section has the given monodromy, and the points $a_1, \ldots, a_n$ are regular singular points for it.

Thus Röhrl had proved Plemelj's results over again, and also shown the solvability of the Riemann–Hilbert problem for a noncompact Riemann surface (see [Fo]). Moreover, Röhrl had proved that a problem analogous to the Riemann–Hilbert problem for an arbitrary Riemann surface was solvable in the class of systems with regular singular points. (Here, it is true, additional "false" singularities making no contribution to the monodromy arise in the system constructed.)

In 1979 a paper of W. Dekkers appeared [Dek], from whose results it follows that the Riemann–Hilbert problem is solvable for any set of points $a_1, \dots, a_n$ and any 2×2 monodromy matrices.

In general, after Plemelj's paper, studies in the area of Hilbert's 21st problem continued, but not very intensively. A new impetus to the study of this problem was given, as already noted in the introduction, by the discovery of the method of isomonodromic deformations in the papers of the American mathematicians Flaschka and Newell [FN] in the 1970s, and in the papers of the Japanese mathematicians Jimbo and Miwa [JM].

A new and powerful motive for the study of this problem had appeared, and many interesting new papers were written on these problems. In the late 1970s a seminar of the French mathematicians B. Malgrange, A. Douady, L. Boutet de Monvel (who is also well known as a mathematician who works in the area of theoretical physics) began in Paris, at which an intensive study was made of circle of problems just described: the Hilbert problem, and isomonodromic deformations.

Simultaneously, the seminar of Arnold and Il'yashenko was going on in Moscow and studying a similar set of topics. And at almost exactly the same time, in the early 1980s, both discovered the gaps in the proof of Plemelj (see [AI], [Koh]) mentioned at the beginning of this section. Numerous attempts were undertaken to recover the lost portion of the proof. However, it turned out to be not so easy to do this, despite the fact that, without exception, every mathematician working in this area had the distinct impression that this problem must have a positive solution, as stated by Hilbert.

It was therefore all the more surprising when a counterexample to Hilbert's 21st problem, discovered by the author, appeared in late 1989 [Bo4]. It turned out that the assertion in the problem is in general *false*, and that in fact *not all given monodromies can be realized by a system of Fuchsian equations*.

This event was preceded by the active work between 1973 and 1977 of the Moscow seminar on the analytic theory of differential equations on complex manifolds, whose participants included A. V. Chernavskii, V. A. Golubeva, V. P. Leksin, A. A. Bolibruch, and others, and at whose sessions we studied the papers of Röhrl [Ro], Gérard [Ge], Deligne [Del], and Katz [Ka], and generalizations of the Riemann–Hilbert problem to manifolds of higher dimension. The organizers of the seminar were V. A. Golubeva and A. V. Chernavskii, who got Leksin and me involved in this area and became our academic advisors (together with M. M. Postnikov, who taught us the basics of algebraic topology in his famous seminar at Moscow University).

The participants in the seminar discovered that there was no proof in Röhrl's paper for the case of Fuchsian systems, but did not find the gap in Plemelj's paper; that was not done until several years later in the papers [AI] and [Koh] already mentioned. After these papers appeared, starting in early 1988 I attempted to give a proof of the positive solvability of the Riemann–Hilbert problem for the case of 3×3 monodromy matrices (the smallest dimension monodromy matrices that had not yet been studied), which I succeeded in doing quite quickly for an irreducible monodromy. However, the reducible case turned out to be more complicated to study. As a result, and to my surprise (like other mathematicians, I originally had no doubt that the problem could be given a positive solution), I succeeded in obtaining a counterexample [Bo4].

The counterexample involves the case of four singular points and 3×3 monodromy matrices. This is the first dimension and the minimal number of singular points for which such an example is theoretically possible. The example itself is rather complicated to discuss: the required monodromy is defined implicitly in it, as the monodromy of a certain system of equations with regular singular points; it is then proved that this system of equations cannot be reduced to a Fuchsian system.

In addition it turns out that this counterexample is unstable in the following sense. For almost every small variation in the position of the singular points (preserving the monodromy matrices — that is, under an isomonodromic deformation of the non-Fuchsian system constructed) the new given monodromies (which differ from the original ones only in the location of the singular points) can be realized as the monodromy data of some Fuchsian system.

I am all the more grateful to D. V. Anosov, who took on himself the difficult task of checking this counterexample and even simplified its exposition somewhat [AB]. My close mathematical association with Anosov and other members of the Department of Ordinary Differential Equations at the Steklov Mathematics Institute of the Russian Academy of Sciences, where I was hired in 1990, has meant — and still means — a great deal to me.

The corresponding counterexample in dimension four [AB], [Bo7], which I obtained later, has a much simpler appearance. Here one can get by with three singular points and three monodromy matrices. Let us consider a special type of monodromy, which from now on we shall call a *B-monodromy* (the term is due to Il'yashenko [Il1]). A monodromy group is a B-monodromy if the set of monodromy matrices is reducible, and if the Jordan normal form of each monodromy matrix G_i consists of exactly one Jordan cell. We recall that a set of matrices is called reducible if the matrices have a common invariant subspace different from the zero subspace and the entire space $\mathbb{C}^p$.

The following proposition turns out to be true (see [Bo13]):

If a B-monodromy can be realized as the monodromy group of some Fuchsian system, then the product of the eigenvalues of the monodromy matrices $G_1, \ldots, G_n$ *must equal* 1.

Consider the matrices

$$G_1 = \begin{pmatrix} 1 & 1 & 0 & 0 \\ 0 & 1 & 1 & 0 \\ 0 & 0 & 1 & 1 \\ 0 & 0 & 0 & 1 \end{pmatrix}, \qquad G_2 = \begin{pmatrix} 3 & 1 & 1 & -1 \\ -4 & -1 & 1 & 2 \\ 0 & 0 & 3 & 1 \\ 0 & 0 & -4 & -1 \end{pmatrix},$$

$$G_3 = \begin{pmatrix} -1 & 0 & 2 & -1 \\ 4 & -1 & 0 & 1 \\ 0 & 0 & -1 & 0 \\ 0 & 0 & 4 & -1 \end{pmatrix}$$

and an arbitrary set of points a_1, a_2, a_3.

We remark that $G_1 \cdot G_2 \cdot G_3 = I$, that G_2 can be transformed into G_1, and that G_3 can be transformed into a Jordan cell with eigenvalue -1. Indeed, for G_2 we have

$$S_2^{-1} G_2 S_2 = \begin{pmatrix} 1 & 1 & 0 & 0 \\ 0 & 1 & 1 & 0 \\ 0 & 0 & 1 & 1 \\ 0 & 0 & 0 & 1 \end{pmatrix}, \qquad S_2 = \frac{1}{3} \begin{pmatrix} 3 & 0 & 0 & 0 \\ -6 & 3 & -3 & 4 \\ 0 & 0 & 1 & -1 \\ 0 & 0 & -2 & 3 \end{pmatrix},$$

while for G_3 we obtain

$$S_3^{-1} G_3 S_3 = \begin{pmatrix} -1 & 1 & 0 & 0 \\ 0 & -1 & 1 & 0 \\ 0 & 0 & -1 & 1 \\ 0 & 0 & 0 & -1 \end{pmatrix}, \qquad S_3 = \frac{1}{64} \begin{pmatrix} 0 & 16 & 4 & 3 \\ 64 & 0 & 0 & 0 \\ 0 & 0 & 0 & -4 \\ 0 & 0 & -16 & -12 \end{pmatrix}.$$

The matrices G_1, G_2, and G_3 have a common invariant subspace of dimension 2. Therefore the set of matrices under consideration satisfies the definition of a B-monodromy. But the product of the eigenvalues of these matrices is -1, and not 1. Hence, according to the above proposition, the set of these matrices cannot be the monodromy of any Fuchsian system. That is, *these monodromy data provide a counterexample to Hilbert's 21st problem.*

The method that succeeded in solving Hilbert's 21st problem came partly from algebraic geometry and was begun by the papers of Röhrl [Ro], Levelt [Le], Deligne [Del], and other excellent mathematicians working in this area.

It consists of the following. We first construct all possible holomorphic vector bundles on the Riemann sphere with a logarithmic connection having prescribed monodromy and given singular points. Thus the original problem reduces to the question whether at least one of the series of bundles constructed is trivial; for in a trivial bundle a logarithmic connection defines a Fuchsian system of differential equations. We then study the question of the triviality of the bundles constructed and obtain conditions for solvability of the Riemann–Hilbert problem.

The key point in this study is the fact that one can connect the asymptotics of the solutions of a Fuchsian system at singular points and the invariants of a certain holomorphic vector bundle (the so-called canonical extension) constructed from the original monodromy. This makes it possible to obtain necessary and sufficient conditions for realizability of the B-bundles noted above by a Fuchsian system in terms of these invariants.

The negative solution of the problem that was obtained did not mean that studies in this area were completely finished. This result rather posed a large number of new and interesting questions that were important for applications. For example, how can one describe the class of monodromy groups that are nevertheless realizable by Fuchsian systems? An answer (though incomplete) to that question was also obtained: thus, for example, the author and V. P. Kostov (independently) were able to prove that any monodromy data with an irreducible set of matrices can be realized as the monodromy data of some Fuchsian system (see [Bo5], [Ko1]). In [AB] and [Ko2] the codimension of the set of monodromy data not realizable by Fuchsian systems in the space of all monodromy data was found. It equals $(2n-1)(p-1)$, where n is the number of singular points and $p \times p$ is the dimension of the monodromy matrices.

At present, a whole series of sufficient conditions for realizability of a monodromy has been obtained, but in general none of these conditions can be "stretched" into necessary conditions (see [Bo7], [Bo10], [Bo15]).

The negative solution of the Riemann–Hilbert problem means that the generalized inverse monodromy problem also has a negative solution in general.

3. The Birkhoff Standard Form

Speaking informally, the Birkhoff standard form of a system of linear differential equations in a neighborhood of a singular point is the simplest form to which one can reduce the system by using a suitable change in the unknown function. This change is assumed to be either holomorphically invertible at a singular point, in which case one speaks of an *analytic transformation*, or meromorphic at a singular point, in which case one speaks of a *meromorphic transformation*.

Let us consider a system of linear differential equations in a neighborhood of infinity

$$z\frac{dy}{dz} = C(z)y, \tag{3}$$

with coefficient matrix $C(z)$ of dimension $p \times p$ and of the form

$$C(z) = z^r \sum_{n=0}^{\infty} C_n z^{-n}, \quad C_0 \neq 0, \quad r \geqslant 0, \tag{4}$$

where the series converges in some neighborhood $O_\infty = \{z \in \mathbb{C} : |z| > R\}$ of infinity. If $r > 0$, this singularity is, in general, an irregular singular point.

The series (4) can be separated into two parts:

$$C(z) = D_s + D_g, \quad \text{where} \quad D_s = z^r \sum_{n=0}^{r} C_n z^{-n},$$

and D_g is the part consisting of negative powers of z.

If the term D_s were missing, the point ∞ would be a nonsingular point of the system, which is easy to verify using the change of coordinates $z = 1/t$ and checking that the coefficient matrix of the transformed system is holomorphic at $t = 0$. By the same procedure, one can easily verify that the presence of any term of the polynomial D_s leads to a pole in the transformed coefficient matrix. In other words, the polynomial D_s is responsible for the singularity at infinity, and the term D_g is, at first sight, not responsible. A natural question arises: Can the nonsingular term D_g be removed by an analytic change of the unknown function, making the system (3) into a system with a polynomial coefficient matrix?

In 1913 Birkhoff gave a positive answer to this question [Bi3]. Since that time, the transformed system (3) with polynomial coefficient matrix has been called the *Birkhoff standard form* of the original system.

However, Birkhoff's proof turned out to have an error, and in the early 1950s Gantmacher [Ga] produced a counterexample to Birkhoff's proposition. As it turned out, Birkhoff's proof goes through only for the case when the monodromy matrix of the system (3) can be diagonalized at infinity. It was later established that the obstacle to the analytic reduction of a system to Birkhoff standard form is its reducibility.

A system (3) is *reducible* if it can be transformed analytically into a system (3) with coefficient matrix C having block upper-triangular form

$$C(z) = \begin{pmatrix} C' & * \\ 0 & C'' \end{pmatrix}. \tag{5}$$

Otherwise the system is *irreducible*.

Jurkat, Lutz, and Peyerimhoff [JLP] proved in 1976 that every irreducible system of two equations can be analytically transformed to Birkhoff standard form. In 1990, Balser [Ba2] obtained an analogous result for an arbitrary irreducible system of three equations. The present author succeeded in proving [Bo6] that

every irreducible system (3) *can be transformed to Birkhoff standard form using an analytic transformation.*

The method of proof is really the same as in the proof of the realizability of every irreducible monodromy by a Fuchsian system. This depends on the fact that the problem of Birkhoff standard form, which at first sight appears to be a local problem (both the original system and the required transformation are defined only locally in a neighborhood of infinity), is actually global in character, since the resulting system is now defined on the entire Riemann sphere. For that reason, it is natural to restate it in terms of fiber bundles and connections, after which the entire scheme of reasoning used in the case of an irreducible monodromy in the Riemann–Hilbert problem carries over (with some simplifications) to this case.

At present, numerous sufficient conditions have been obtained for reducible systems, under which they can be reduced to systems in Birkhoff standard form by an analytic transformation (see [Bo8], [Sa]).

A natural generalization of the preceding problem to a larger class of transformations is the question whether it is possible to reduce a system of linear differential equations to Birkhoff standard form using a meromorphic transformation not exceeding the order of the pole of the system at a singular point. (While an analytic transformation automatically preserves the order of a pole of a coefficient, a meromorphic transformation may change it, and a system with a higher-order pole at a singular point is in some sense “more complicated” than the original system, so that it would not make sense to do the transformation.)

This problem is of a great interest in connection with the fact that meromorphic transformations change neither the Stokes matrices, nor the monodromy of the system, nor the structure of the formal solution. The question, whether it is possible to reduce a system to Birkhoff standard form meromorphically, arises in the study of the inversion problem in differential Galois theory; in computing Stokes matrices; in the proof of the Painlevé properties for isomonodromic deformations of linear systems with irregular singular points; and the like.

In 1963, Turrittin [Tu] proved that if the eigenvalues of the matrix C_0 in (4) are pairwise distinct, the system (3) can be reduced to Birkhoff standard form by a meromorphic transformation without increasing the order of the pole at a singular point.

In 1976, Jurkat, Lutz, and Peyerimhoff proved [JLP] that the problem of reducing a system of equations to Birkhoff standard form by a meromorphic transformation always has a positive solution in the case of a system of two equations. In 1989, Balser [Ba2] proved an analogous result for a system of three equations, and Bryuno [Br] showed in 2000 that such a reduction is possible for every upper-triangular system.

For systems of four or five equations it has been proved that if the system (3), (5) consists of two irreducible blocks C', C'', then this problem also has a positive solution [Bo9]. However, in general, despite its being a problem of current interest and the simplicity of its statement, the problem of meromorphic transformation to Birkhoff standard form has not yet been solved.

It turns out that this problem is a special case of the generalized inverse monodromy problem. Indeed, consider the monodromy matrix G of the system (3); the formal monodromy; the Stokes matrices; the exponential part of the formal solution; and the order $r+1$ of a pole of the coefficient matrix of the system at infinity. To these generalized monodromy data, let us add a singular point at zero, and a monodromy matrix equal to G^{-1} at zero, and let us set $R_0 = 1$. It turns out that the original system can be brought into Birkhoff standard form if and only if the generalized inverse monodromy problem with these data has a positive solution. This follows from the result of Sibuya [Si], who proved that any two systems with the same generalized monodromy data at a singular point are locally locally meromorphically equivalent (in a neighborhood of the point).

4. Applications

Before passing to the applications of the result discussed in the preceding two sections, let us return for a while to the generalized inverse monodromy problem stated in the introduction. The method of studying this problem consists of the following.

For each singular point we realize the generalized monodromy data involving that point by a local system of differential equations (which can always be done according to a theorem of Sibuya [Si]). From the resulting family of local differential equations and the monodromy $G_1, \dots, G_n$ one can construct a vector bundle on the extended complex plane with a meromorphic connection having the same generalized monodromy data as the original problem. If the bundle so constructed is holomorphically trivial, this connection defines a system of equations that provides the solution of the problem.

The local system constructed from monodromy data is not unique. If we consider different local realizations of the generalized monodromy data, we will obtain a different nonequivalent bundle. Thus, one can construct a whole series of bundles with meromorphic connections having prescribed generalized monodromy data. Just as in the case of the classical Riemann–Hilbert problem, we obtain the following proposition:

> *The generalized inverse monodromy problem has a positive solution if and only if at least one of the bundles constructed is holomorphically trivial.*

Every vector bundle of rank p on the extended complex plane can be expanded as the direct sum of one-dimensional bundles, each of which in turn is a tensor degree of a bundle associated with the Hopf bundle [OSS]. Thus the holomorphic type of every vector bundle is determined completely by a set of p integers (tensor degrees) $c_1, \dots, c_p$ arranged in increasing order. This set is called the *splitting type of the bundle*. The bundle is holomorphically trivial if and only if all the numbers c_i are zero. The sum of the numbers c_i equals the degree of the bundle, and it is zero if and only if the bundle is topologically trivial. It is clear that holomorphically trivial bundles should be sought among the subset of constructed bundles of degree zero.

In algebraic geometry there is a concept of stability of a bundle with a connection (regarded as a pair), which turns out to be extremely useful in the study of inverse monodromy problems. For a bundle of degree zero this concept is defined as follows.

> *A fiber bundle F of degree zero with a connection ∇ is called* stable (as a pair) *if the degree of every subbundle F' of this bundle, stabilized by the connection, is negative.*

We recall that a subbundle F' is *stabilized* by a connection if all the covariant derivatives, constructed using the connection, map local sections of F' into local sections of the same subbundle F'.

One of the strongest sufficient conditions for a positive solvability of the generalized inverse monodromy problem is a direct generalization of the corresponding sufficient condition for the classical Riemann–Hilbert problem of [Bo15], and can be stated as follows.

> *Suppose that for at least one singular point the corresponding exponential part of a formal solution has no roots of z in an exponent. If there is at least one stable pair in the series of bundles with connections constructed from the monodromy data, then the corresponding inverse monodromy problem has a positive solution.*

It is interesting to trace how sufficient conditions for an irreducible monodromy in the case of the Riemann–Hilbert problem and sufficient conditions for an irreducible system in the case of Birkhoff standard form follow from this condition.

Both the irreducibility of the monodromy and the irreducibility of the system mean that every bundle constructed from the initial data (from the monodromy in the first case and from the system in the second) have no subbundles stabilized by a connection; that is, every bundle with connection constructed from these data is automatically stable (as a pair). Since in both cases there are points at which there are no formal solutions at all (in the first case all points have this property, in the second, the point 0 has it), we find ourselves in possession of the hypotheses of the proposition above, from which follow the abovementioned sufficient conditions in the Riemann–Hilbert problem and in the problem of Birkhoff standard form.

We remark here that if, in the generalized inverse monodromy problem, one of the conditions is weakened so that the required system has a pole of higher order at one of its singular points, such a problem will always have a positive solution. This proposition is related to the positive solvability of the classical Hilbert problem in the class of systems with regular singular points and follows from the fact that every holomorphic vector bundle on the extended complex plane has a meromorphic trivialization that is holomorphic except at any preassigned point a_i. The connection constructed along with the bundle from the original monodromy data will define in that trivialization a system of linear differential equations with all the required properties except one: the order of the pole of this system at a_i will, in general, be higher than the prescribed order. However, in many important applications the orders of the poles are rigidly fixed.

Let us now turn to the applications of the results presented above. The methods developed in solving the Riemann–Hilbert problem make it possible to prove the following proposition on the connection of Fuchsian systems and Fuchsian scalar differential equations [Bo7]:

> *From every Fuchsian equation on the Riemann sphere one can construct a Fuchsian system* (1) *with the same singular points and the same monodromy.*

The converse, of course, is not true. As already noted in Section 2, in the construction of a scalar equation from a prescribed monodromy, additional singular points unavoidably arise in general. How can one estimate the number of such points?

Let us consider, as was done in the study of the Riemann–Hilbert problem, the set $\mathscr{F}$ of bundles with logarithmic connections, constructed from a given irreducible monodromy. For an arbitrary bundle $F \in \mathscr{F}$ consider the number

$$\gamma(F) = pc_1 - \deg(F) = pc_1 - \sum_{i=1}^{p} c_i,$$

where the numbers $c_1, \ldots, c_p$ determine the type of splitting of the bundle F.

Let us call the number

$$\gamma_m = \sup_{F \in \mathscr{F}} \gamma(F)$$

the *maximal Fuchsian weight* of the given irreducible monodromy. According to [Bo7], this number does not exceed $(n-2)p(p-1)/2$. It turns out to be closely connected with the asymptotics of the solutions of Fuchsian systems with the given monodromy.

In terms of the maximal Fuchsian weight of a monodromy, one can express the minimum possible number of additional false singular points m^0 (counted according to multiplicities, which equal the orders of the zeros of the Wronskian of the corresponding "minimal" Fuchsian equation) that arise in constructing a scalar Fuchsian differential equation with irreducible monodromy [Bo7]:

The minimum possible number m^0 of additional false singular points that arise in the construction of a scalar Fuchsian differential equation with irreducible monodromy is

$$m^0 = \frac{(n-2)p(p-1)}{2} - \gamma_m.$$

Hence it follows that an irreducible monodromy can be realized by a scalar Fuchsian differential equation without any additional singular points if and only if the maximal Fuchsian weight assumes the largest possible value, equal to $(n-2)p(p-1)/2$.

Let us return to the counterexample to the Riemann–Hilbert problem given in Section 2. This counterexample is independent of the location of the singular points and works for any a_1, a_2, a_3. However, for the theory of isomonodromic deformations the "unstable counterexamples" (of the sort mentioned in Section 2 just before the detailed counterexample given there) are of particular interest because the locations of the singular points for which the given monodromy cannot be realized by a Fuchsian system are movable singularities for the Schlesinger equation of isomonodromic deformations.

We recall that a family of systems of equations

$$\frac{\mathrm{d}y}{\mathrm{d}z} = B(z,a)y, \tag{6}$$

depending analytically on a parameter a is *isomonodromic* if for all fixed values of the parameter the corresponding systems of equations all have the same monodromy. The property of isomonodromicity imposes certain requirements on the coefficients of the family (6), which are called *isomonodromic deformation equations*.

The best-known isomonodromic deformation equation is the Schlesinger equation. It is a compatibility (integrability) condition for the Fuchsian isomonodromic family

$$\frac{\mathrm{d}y}{\mathrm{d}z} = \left(\sum_{i=1}^{n} \frac{B_i(a)}{z-a_i}\right)y, \tag{7}$$

where the locations of the singular points $a_1,\dots,a_n$ are the parameters and where we use the notation $a=(a_1,\dots,a_n)$ for points of $\mathbb{C}^n$.

The Schlesinger equation has the following form:

$$\mathrm{d}B_i(a) = -\sum_{j=1,\, j\neq i}^{n} \frac{[B_i(a),B_j(a)]}{a_i-a_j}\,\mathrm{d}(a_i-a_j). \tag{8}$$

Together with the initial conditions

$$B_i(a_1^0,\dots,a_n^0) = B_i^0, \quad i=1,\dots,n,$$

which can be regarded as the coefficient matrices of the original Fuchsian system subject to deformation, Schlesinger's equation defines an integrable system of nonlinear differential equations.

The Schlesinger equation has been studied by Schlesinger himself [SH], Jimbo and Miwa [JM], Malgrange [Ma1], Its and Novokshenov [IN], Sibuya [Si], and other mathematicians, from various points of view.

It turns out [Ma1] that for all initial data in some neighborhood of the point $a^0=(a_1^0,\dots,a_n^0)$ there exists a unique solution $B_1(a),\dots,B_n(a)$ of this equation, and that solution is holomorphic in the given neighborhood.

The solutions $B_1(a),\dots,B_n(a)$ possess the following remarkable property: they can be extended over the entire universal covering of the space $\mathbb{C}^n \setminus \bigcup_{i\neq j}\{a_i-a_j=0\}$ as meromorphic functions of the argument a; that is, these extensions have elementary singularities, namely poles at the singular points ([JM], [Ma1]). In that case, we say that *the solutions of the Schlesinger* *equation have the Painlevé property*. The singular points of these extended functions are called *movable singularities*, since their location changes when the initial conditions are changed.

Miwa's theorem [JM] gives a description of the set Θ of movable points of the Schlesinger equation. It turns out that this set is defined by the zeros of a function τ such that

$$\mathrm{d}\tau = \frac{1}{2}\sum_{i,j,\ i\neq j} \frac{\operatorname{tr}\big(B_i(a)B_j(a)\big)\,\mathrm{d}(a_i - a_j)}{a_i - a_j}.$$

The singular set Θ can be also described as follows. As already noted, the initial data of the Schlesinger equation give a Fuchsian system with singular points $a_1^0,\dots,a_n^0$ and coefficient matrices $B_1^0,\dots,B_n^0$. This system can be regarded as a connection on the trivial bundle on the extended complex plane. Consider an isomonodromic deformation of this bundle with connection, where the singular points $a_1,\dots,a_n$ once again occur as the parameters of the deformation. Under small deformations the bundle remains trivial, and consequently the connection defines a family of Fuchsian systems whose coefficients are solutions of the Schlesinger equation. The set of singular points Θ can be interpreted as the set at whose points the deformed bundle becomes holomorphically nontrivial.

Consider a point a^* of Θ and splitting type $c_1,\dots,c_n$ of the deformed bundle at this point. It turns out that there is a remarkable connection between the numbers $c_1,\dots,c_n$ and the orders of the poles of solutions of the Schlesinger equation at the point a^*. Thus, for example, for a system of two equations *the orders of such poles do not exceed $c_1 - c_2$, which in turn is bounded by $n-2$* (see [Bo12], [Bo14]).

An isomonodromic deformation preserves not only monodromy, but also the asymptotics of the solutions at singular points. Therefore (see [Ma1], [Bo14]),

> *the movable singular points of the Schlesinger equation can be interpreted as the points for which the generalized Riemann–Hilbert problem, which consists of constructing a Fuchsian system with given monodromy and given asymptotics of the solution, has a negative solution.*

Everything that has been said about isomonodromic deformations of Fuchsian systems and the Schlesinger equation can be carried over to the case of isomonodromic deformations of linear systems of differential equations with irregular singular points (see [JM], [Ma2], [Pa]). Only here one must interpret monodromy data to mean generalized data, including formal monodromy matrices, Stokes matrices, and transition matrices between local solutions at singular points, along with ordinary monodromy matrices. The locations of the singular points and/or the exponents in the exponential parts of formal solutions at singular points enter here as the parameters of the deformation. In this case the movable singular points of equations of isomonodromic deformations are connected not with counterexamples to the classical Riemann–Hilbert problem, but with negative solutions of the corresponding generalized inverse monodromy problem, in which the exponential parts of formal solutions have been removed from the monodromy data.

Interest in isomonodromic deformations of linear systems is largely due to the circumstance that the famous Painlevé equations (six nonlinear ordinary second-order differential equations having the property that movable singular points of their solutions are poles) can be interpreted as isomonodromic deformation equations of linear systems of two equations. For that reason, the singular points of these equations and the orders of the poles of their solutions can be studied by the same procedure used to study the Schlesinger equation.

The second Painlevé equation turns out to be closely connected with the problem of Birkhoff standard form (as a special case of the generalized inverse monodromy problem), and the sixth Painlevé equation is connected with the classical Riemann–Hilbert problem with four singular points. For more details on the connection of the Painlevé equations with isomonodromic deformations, see [IN], [Iw], [Bo14]. For the connection of this problem with the theory of Feynman integrals, see [Go1].

5. Conclusion

The Riemann–Hilbert problem admits the following generalizations.

Instead of the extended complex plane, one can consider a compact Riemann surface of genus g. In this case, it follows from dimensional considerations that, in the construction of a Fuchsian system with prescribed monodromy, additional singular points necessarily arise in general. Estimating the number of such points is an interesting question.

Another way of generalizing the Riemann–Hilbert problem to a compact Riemann surface (proposed in [EV]) is to replace the Fuchsian system by a semi-stable bundle of degree zero with a logarithmic connection having prescribed singular points and prescribed monodromy. We recall that a vector bundle of degree zero is *semi-stable* if the degree of each of its subbundles is at most zero. This concept was introduced by Mumford in the 1960s and is more general than the later concept of stability for a bundle–connection pair, which was discussed in the preceding section.

On the extended complex plane every semi-stable bundle of degree zero has zero splitting type, that is, it is holomorphically trivial, and therefore a logarithmic connection in such a bundle defines a Fuchsian system with given monodromy data. Consequently one can indeed regard the problem of constructing a semi-stable bundle of degree zero, with a logarithmic connection having prescribed singular points and monodromy on a compact Riemann surface, as a generalization of the classical Riemann–Hilbert problem. Practically all the results on the Riemann–Hilbert problem carry over to this case, including the counterexamples presented in Section 2 above (see [EV], [EH], [Bo15]).

The papers of Röhrl [Ro] and Deligne [Del] gave an impetus to the formulation and study of the multi-dimensional Riemann–Hilbert problem, which consists of studying the question whether there exists a completely integrable Pfaffian system of Fuchsian type on a complex analytic manifold M^n, having a prescribed divisor of singularities D and a prescribed monodromy.

The first results obtained in this direction involved the investigation of trivial cases: a contractible Stein manifold (Gérard [Ge] proved the solvability of the Riemann–Hilbert problem in this case) and n-dimensional complex projective space with commutative monodromy (see [Bo3], [Go2], [Lek1]). In [Bo3] and [Lek2] the first examples of a negative solution of the multi-dimensional Riemann–Hilbert problem were obtained.

The positive solvability of the multi-dimensional Riemann–Hilbert problem was proved on a projective manifold and on a Stein manifold in the class of systems with additional false singularities [Su], and on a connected Stein manifold of complex dimension 2 with the condition $H^2(M^n, \mathbb{Z}) = 0$ [Ki]. Necessary conditions for solvability of the Riemann–Hilbert problem in terms of the structure of the fundamental group were obtained in [Hai]. One of the obstacles on the way to the study of the multi-dimensional Riemann–Hilbert problem is that the problem of describing the local solution space of a Pfaffian system of Fuchsian type has not yet been solved. (The structure of this space for a divisor D with normal intersections was studied in [YT], [Bo1], [Bo2].)

The nonlinear Riemann–Hilbert problem (which is nontrivial even in the case of a scalar nonlinear equation of first order) on the extended complex plane was investigated in [Il2].

In conclusion we state several unsolved problems that are of interest both for the analytic theory of differential equations and for its numerous applications:

- The problem of meromorphic transformation to Birkhoff standard form.
- The problem of determining whether it is possible to construct a system with regular singular points on a compact Riemann surface with prescribed

singular points and monodromy (and estimating the number of additional singular points that arise).

- The following problem is of great interest for the theory of isomonodromic deformations. Is it possible to construct, from an irreducible monodromy, a Fuchsian system with a given monodromy and preassigned given admissible values of the asymptotics of solutions at singular points? The answer to this question is negative in general [Bo11]. In this connection the following important question remains open: For which irreducible monodromy data is the number of such "forbidden" asymptotics finite? (Here sets of asymptotics are considered only up to the equivalence defined by simultaneously adding the same integer to all asymptotics at the same point, and subtracting that same integer from all asymptotics at some other point.)

- The following natural problem is also not yet solved. If the monodromy matrices of some Fuchsian system have block-diagonal form, is it true that this Fuchsian system is meromorphically equivalent to a direct sum of Fuchsian systems (that is, a system whose coefficient matrix has the same block-diagonal form)?

We remark that a problem close to the last has recently been solved by S. Malek [Ml], who proved that *if a reducible monodromy can be realized by a Fuchsian system, then it can also be realized by a Fuchsian system with block-diagonal coefficient matrix*. (The dimensions of the subblocks of the system need not coincide with the character of reducibility of the monodromy, and this prevents the direct application of this result to the solution of the problem stated above.)

Bibliography

[AB] D. V. Anosov, A. A. Bolibruch. *The Riemann–Hilbert Problem*. Braunschweig: Vieweg & Sohn, 1994 (Aspects Math., **E22**).

[AI] V. I. Arnold, Yu. S. Il'yashenko. Ordinary differential equations. Ordinary differential equations. In: *Ordinary Differential Equations and Smooth Dynamical Systems* (eds. D. V. Anosov and V. I. Arnold). Berlin: Springer, 1988, 1–148 (Dynamical Systems I; Encyclopædia Math. Sci., **1**).

[Ba1] W. Balser. Meromorphic transformation to Birkhoff standard form in dimension three. *J. Fac. Sci. Univ. Tokyo, Sect. IA Math.*, 1989, **36**, 233–246.

[Ba2] W. Balser. Analytic transformation to Birkhoff standard form in dimension three. *Funkcialaj Ekvacioj*, 1990, **33**(1), 59–67.

[BB] W. Balser, A. A. Bolibruch. Transformation of reducible equations to Birkhoff standard form. In: *Ulmer Seminare über Funktionalanalysis und Differentialgleichungen*, Heft 2, 1997, 73–81.

[BJL] W. Balser, W. B. Jurkat, D. A. Lutz. Invariants for reducible systems of meromorphic differential equations. *Proc. Edinburgh Math. Soc.*, 1980, **23**, 163–186.

[Bi1] G. D. Birkhoff. A theorem on matrices of analytic functions. *Math. Ann.*, 1913, **74**, 122–133.

[Bi2] G. D. Birkhoff. The generalized Riemann problem for linear differential equations. *Proc. Amer. Acad. Arts Sci.*, 1913, **33**(1), 531–568.

[Bi3] G. D. Birkhoff. *Collected Mathematical Papers*, Vol. 1. New York: Dover, 1968.

[Bo1] A. A. Bolibruch. On the fundamental matrix of a Pfaffian system of Fuchsian type. *Math. USSR, Izv.*, 1977, **11**, 1031–1054.

[Bo2] A. A. Bolibruch. Pfaffian systems of Fuchs type on a complex analytic manifold. *Math. USSR, Sb.*, 1977, **32**, 98–108.

[Bo3] A. A. Bolibruch. An example of an unsolvable Riemann–Hilbert problem on $\mathbb{C}P^2$. In: *Geometric Methods in Problems of Algebra and Analysis*, No. 2. Yaroslavl' State University, 1980, 60–64 (Russian).

[Bo4] A. A. Bolibruch. The Riemann–Hilbert problem. *Russ. Math. Surveys*, 1990, **45**(2), 1–58.

[Bo5] A. A. Bolibruch. On sufficient conditions for the positive solvability of the Riemann–Hilbert problem. *Math. Notes*, 1992, **51**(2), 110–117.

[Bo6] A. A. Bolibruch. On analytic transformation to standard Birkhoff form. *Russ. Acad. Sci. Dokl. Math.*, 1994, **49**(1), 150–153.

[Bo7] A. A. Bolibruch. The 21st Hilbert problem for linear Fuchsian systems. *Proc. Steklov Inst. Math.*, 1994, **206**, viii + 145 pp.

[Bo8] A. A. Bolibruch. On the Birkhoff standard form of a linear system of ODE. In: *Mathematics in St. Petersburg*. Providence, RI: Amer. Math. Soc., 1996, 169–179 (AMS Transl., Ser. 2, **174**).

[Bo9] A. A. Bolibruch. Meromorphic transformation to Birkhoff standard form in small dimensions. *Proc. Steklov Inst. Math.*, 1999, **225**, 78–86.

[Bo10] A. A. Bolibruch. On sufficient conditions for the existence of a Fuchsian equation with prescribed monodromy. *J. Dynam. Control Systems*, 1999, **5**(1), 453–472.

[Bo11] A. A. Bolibruch. On Fuchsian systems with given asymptotics and monodromy. *Proc. Steklov Inst. Math.*, 1999, **224**, 98–106.

[Bo12] A. A. Bolibruch. On orders of movable poles of the Schlesinger equation. *J. Dynam. Control Systems*, 2000, **6**(1), 57–74.

[Bo13] A. A. Bolibruch. *Fuchsian Differential Equations and Holomorphic Fiber Bundles*. Moscow Center for Continuous Mathematical Education, 2000 (Russian).

[Bo14] A. A. Bolibruch. Inverse problems for linear differential equations with meromorphic coefficients. In: *Isomonodromic Deformations and Applications in Physics* (Montréal, 2000). Providence, RI: Amer. Math. Soc., 2002, 3–25 (CRM Proc. & Lecture Notes, **31**).

[Bo15] A. A. Bolibruch. Stable vector bundles with logarithmic connections and the Riemann–Hilbert problem. *Russ. Acad. Sci. Dokl. Math.*, 2001, **64**(3), 298–301.

[Br] A. D. Bryuno. The meromorphic reducibility of a linear triangular ODE system. *Russ. Acad. Sci. Dokl. Math.*, 2000, **61**(2), 243–247.

[BV] D. G. Babbitt, V. S. Varadarajan. Local moduli for meromorphic differential equations. *Astérisque*, 1989, **169–170**, 217 pp.

[Dek] W. Dekkers. The matrix of a connection having regular singularities on a vector bundle of rank 2 on $P^1(\mathbb{C})$. In: *Équations différentielles et systèmes de Pfaff dans le champ complexe* (Strasbourg, 1975). Berlin: Springer, 1979, 33–43 (Lecture Notes in Math., **712**).

[Del] P. Deligne. *Équations différentielles à points singuliers réguliers*. Berlin – New York: Springer, 1970 (Lecture Notes in Math., **163**).

[Do] V. A. Dobrovol'skii. *Essays on the Development of the Analytic Theory of Differential Equations*. Kiev: Vyshcha Shkola, 1974 (Russian).

[EH] H. Esnault, C. Herling. Semistable bundles on curves and reducible representations of the fundamental group. *Internat. J. Math.*, 2001, **12**(7), 847–855; `http://www.arXiv.org/abs/math.AG/0101194`

[Er] N. P. Erugin. *The Riemann Problem*. Minsk: Nauka i Tekhnika, 1982 (Russian).

[EV] H. Esnault, E. Viehweg. Semistable bundles on curves and irreducible representations of the fundamental group. In: *Algebraic Geometry: Hirzebruch 70* (Warsaw, 1998). Providence, RI: Amer. Math. Soc., 1999, 129–138 (Contemp. Math., **241**).

[Fo] O. Forster. *Lectures on Riemann Surfaces*, 2nd edition. Berlin: Springer, 1981.

[FN] H. Flaschka, A. C. Newell. Monodromy and spectrum preserving deformations. *Commun. Math. Phys.*, 1980, **76**, 67.

[Ga] F. R. Gantmacher. *Theory of Matrices* (transl. K. A. Hirsch). New York: Chelsea, 1959.

[Ge] R. Gérard. Le problème de Riemann–Hilbert sur une variété analytique complexe. *Ann. Inst. Fourier* (Grenoble), 1977, **24**(9), 357–412.

[Go1] V. A. Golubeva. Some problems in the analytic theory of Feynman integrals. *Russ. Math. Surveys*, 1976, **31**(2), 139–207.

[Go2] V. A. Golubeva. On the recovery of a Pfaffian system of Fuchsian type from the generators of the monodromy group. *Math. USSR, Izv.*, 1981, **17**, 227–241.

[Ha] P. Hartman. *Ordinary Differential Equations*, 2nd edition. Boston, MA: Birkhäuser, 1982.

[Hai] R. M. Hain. On a generalization of Hilbert's 21st problem. *Ann. Sci. École Norm. Supér.* (4), 1986, **19**(4), 609–627.

[Hi] D. Hilbert. *Gesammelte Abhandlungen*. Bronx, NY: Chelsea, 1965.

[Il1] Yu. S. Il'yashenko. Riemann–Hilbert problem and box dimension of attractors, to appear.

[Il2] Yu. S. Il'yashenko. The nonlinear Riemann–Hilbert problem. *Proc. Steklov Inst. Math.*, 1996, **213**, 6–29.

[IN] A. Its, V. Novokshenov. *The Isomonodromic Deformation Method in the Theory of Painlevé Equations*. Berlin: Springer, 1986 (Lecture Notes in Math., **1191**).

[Iw] K. Iwasaki, H. Kimura, S. Shimomura, M. Yoshida. *From Gauss to Painlevé. A Modern Theory of Special Functions*. Braunschweig: Vieweg & Sohn, 1991 (Aspects Math., **E16**).

[JLP] W. B. Jurkat, D. A. Lutz, A. Peyerimhoff. Birkhoff invariants and effective calculations for meromorphic differential equations, I; II. *J. Math. Anal. Appl.*, 1976, **53**, 438–470; *Houston J. Math.*, 1976, **2**, 207–238.

[JM] M. Jimbo, T. Miwa. Monodromy preserving deformations of linear ordinary differential equations, I; II. *Physica D*, 1981, **2**, 306–352; 407–448.

[Ka] N. M. Katz. An overview of Deligne's work on Hilbert's twenty-first problem. *Proc. Sympos. Pure Math.*, 1976, **28**, 537–559.

[Ki] M. Kita. The Riemann–Hilbert problem and its applications to analytic functions of several complex variables. *Tokyo J. Math.*, 1979, **2**(1), 1–27.

[Koh] A. Treibich Kohn. Un résultat de Plemelj. In: *Mathematics and Physics* (Paris, 1979–1982). Boston: Birkhäuser, 1983, 307–312 (Progr. Math., **37**).

[Ko1] V. P. Kostov. Fuchsian systems on CP^1 and the Riemann–Hilbert problem. *C. R. Acad. Sci. Paris, Sér. I Math.*, 1992, **315**, 143–148.

[Ko2] V. P. Kostov. Stratification of the space of monodromy groups of Fuchsian linear systems on $\mathbb{C}P^1$. In: *Complex Analytic Methods in Dynamical Systems* (IMPA, January 1992). *Astérisque*, 1994, **222**, 259–283.

[Kr] B. L. Krylov. Solution in finite form of the Riemann problem for a Gaussian system. *Trudy Kazan. Aviats. Inst.*, 1956, **31**, 203–445 (Russian).

[LD] I. A. Lappo-Danilevskii. *Application of Functions of Matrices to the Theory of Linear Systems of Ordinary Differential Equations*. Moscow: GTTI (State Technical-Theoretic Publishing House), 1957 (Russian).

[Le] A. H. M. Levelt. Hypergeometric functions. *Nederl. Akad. Wetensch. Proc., Ser. A*, 1961, **64**, 361–401.

[Lek1] V. P. Leksin. Meromorphic Pfaffian systems on complex projective spaces. *Math. USSR., Sb.*, 1987, **57**, 211–227.

[Lek2] V. P. Leksin. On Fuchsian representations of the fundamental group of complex manifolds. In: *Qualitative and Approximate Methods of Studying Operator Equations*. Yaroslavl' State University, 1979, 109–114 (Russian).

[Ma1] B. Malgrange. Sur les déformations isomonodromiques. I. Singularités régulières. In: *Mathematics and Physics* (Paris, 1979–1982). Boston, MA: Birkhäuser, 1983, 401–426 (Progr. Math., **37**).

[Ma2] B. Malgrange. Sur les déformations isomonodromiques. II. Singularités irrégulières. In: *Mathematics and Physics* (Paris, 1979–1982). Boston, MA: Birkhäuser, 1983, 427–438 (Progr. Math., **37**).

[Ml] S. Malek. Systèmes fuchsiens à monodromie réductible. *C. R. Acad. Sci. Paris, Sér. I Math.*, 2001, **332**, 691–694.

[OSS] C. Okonek, M. Schneider, H. Spindler. *Vector Bundles on Complex Projective Spaces*. Boston, MA: Birkhäuser, 1980.

[Pa] J. Palmer. Zeros of the Jimbo, Miwa, Ueno tau function. *J. Math. Phys.*, 1999, **40**(12), 6638–6681.

[Pl] J. Plemelj. *Problems in the Sense of Riemann and Klein.* New York: Wiley Interscience, 1964.

[Poi] H. Poincaré. Sur les groupes des équations linéaires. *Acta Math.*, 1883, **4**, 201–312.

[Ri] B. Riemann. *Gesammelte mathematische Werke* (ed. R. Narasimhan). Berlin – New York: Springer, 1990.

[Ro] H. Röhrl. Das Riemann–Hilbertsche Problem der Theorie der linearen Differentialgleichungen. *Math. Ann.*, 1957, **133**, 1–25.

[Sa] C. Sabbah. Frobenius manifolds: isomonodromic deformations and infinitesimal period mappings. *Expos. Math.*, 1998, **16**(1), 1–57.

[SH] L. Schlesinger. Über Lösungen gewisser Differentialgleichungen als Funktionen der singulären Punkte. *J. Reine Angew. Math.*, 1905, **129**, 287–294.

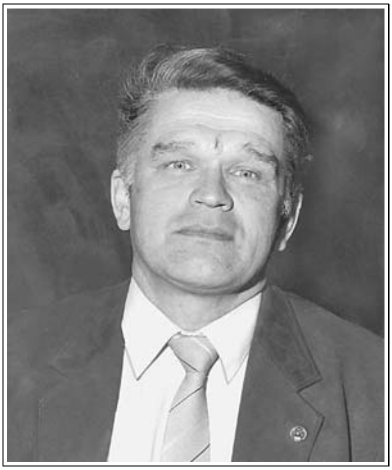
[Si] Y. Sibuya. *Linear Differential Equations in the Complex Domain: Problems of Analytic Continuation*. Providence, RI: Amer. Math. Soc., 1990 (Transl. Math. Monographs, **82**).

[Su] O. Suzuki. The problems of Riemann and Hilbert and the relations of Fuchs in several complex variables. In: *Équations différentielles et systèmes de Pfaff dans le champ complexe* (Strasbourg, 1975). Berlin: Springer, 1979, 325–364 (Lecture Notes in Math., **712**).

[Tu] H. L. Turrittin. Reduction of ordinary differential equations to the Birkhoff canonical form. *Trans. Amer. Math. Soc.*, 1963, **107**, 485–507.

[Wa] W. Wasow. *Asymptotic Expansions for Ordinary Differential Equations*. Huntington, NY: Robert E. Krieger, 1976; reprinted by Dover, 1987.

[YT] M. Yoshida, K. Takano. Local theory of Fuchsian systems. *Proc. Japan Acad.*, 1975, **51**(4), 219–223.



L. D. Faddeev

What Modern Mathematical Physics Is Supposed to Be

Translated by R. Cooke

When someone wants to know what my area of research is, I call myself a specialist in mathematical physics. Since I have been studying science for more than 40 years, I have formed a certain interpretation of the phrase *mathematical physics*. Cynics or purists sometimes claim that it is neither mathematics nor physics, and add comments of varying degrees of acerbity. Naturally, these comments demand a response, and in this short article I hope to expound briefly on my understanding of the question, thereby adding something to the discussion.

The situation is complicated by the fact that the term *mathematical physics* (which we shall frequently abbreviate to MPh below) is used in very different senses and may have several completely different meanings. This meaning changes over time and depends on the location and personal interests of the person using the phrase.

I have not studied the history of science very thoroughly but, to my knowledge, early in the twentieth century the term MPh was practically equivalent to the concept of theoretical physics. Both Henri Poincaré and Albert Einstein were called mathematical physicists. The newly created departments of theoretical physics in Britain and Germany were being called departments of mathematical physics. It is apparent from documents in the archives of the Nobel Committee that MPh had the right to participate fully in the text of the nominations and evaluations of candidates for the Nobel Prize in physics [1]. Roughly speaking, theoretical papers using mathematical formulas were regarded as MPh.

However, in the course of the unprecedented blossoming of theoretical physics in the 1920s and 1930s, an essential separation of the terms theoretical and mathematical physics occurred. For many MPh came to be reduced to the important but ancillary course “Methods of Mathematical Physics,” with a collection of useful mathematical techniques. The classical example is the text of P. Morse and H. Feshbach [2], which was aimed at a wide circle of physicists and engineers.

In the meantime, mathematical physics began to be understood in the mathematical sense as the theory of partial differential equations and calculus of variations. The monographs of R. Courant and D. Hilbert [3] or S. L. Sobolev [4] are outstanding examples of such an interpretation of MPh. Existence and uniqueness theorems based on variational principles, *a priori* estimates and embedding theorems for function spaces comprise the main content of this area. Being a student of O. A. Ladyzhenskaya, I was immersed in this environment from the third year on in the Department of Physics at Leningrad University. My classmate N. N. Ural’tseva now heads the Chair of Mathematical Physics at the university with precisely that understanding of its meaning.

The sources of problems for MPh in the sense just described are mainly geometry and branches of classical continuum mechanics such as fluid dynamics and elasticity theory. Closely related in spirit, but not in methods, is that part of MPh generated by problems of quantum theory, which has been developed actively and independently since the 1960s. Here the principal machinery is functional analysis, including the spectral theory of operators in Hilbert space and the mathematical theory of scattering, as well as Lie groups and their representations. The main object of investigation is the Schrödinger operator. And although the specific content of this part of mathematical physics differs strongly from the content of the classical version, the methodological aspects remain the same. We see searches for rigorous mathematical theorems concerning results that physicists often understand already in their own language.

I was born as a scholar in just such an environment, having been educated in the Chair of Mathematical Physics at Leningrad University. This unique chair was founded in the 1930s by V. I. Smirnov with the active support of V. A. Fock, who had a wide circle of mathematical interests. Originally the chair (possibly the first of its kind in the world) played an auxiliary role in the department, providing all the mathematical courses. In 1955 it received permission to offer a major, and I was in the first group of students majoring in mathematical physics. As I mentioned above, O. A. Ladyzhenskaya was our main professor. Although her own interests concerned mainly the theory of nonlinear partial differential equations and fluid dynamics, she ventured to point me toward quantum theory. I was charged with reading the book *Mathematical Aspects of Quantum Field Theory* by Kurt Friedrichs and reporting on it to a special seminar of our group, which consisted of five people. At that time my colleagues (the students of the chair of theoretical physics) were avidly reading the first monograph on quantum electrodynamics by A. Akhiezer and V. Berestetskii. The difference in language and priorities was obvious, but it gave me the chance to learn two approaches to quantum theory at once.

Ladyzhenskaya, who remained my advisor when I was a graduate student, gave me complete freedom in the choice of topics and reading matter. As a result, I read mathematical papers on the direct and inverse scattering problem by I. M. Gel'fand and B. M. Levitan, V. A. Marchenko, M. G. Krein, and A. Ya. Povzner and also physics papers on the formal theory of scattering by M. Gell-Mann, M. Goldberger, J. Schwinger, and H. Eckstein. The papers of I. Segal, L. van Hove, and R. Haag complemented the early impressions of quantum field theory that I had acquired from the book of Friedrichs. In the course of all this self-education my understanding of the nature and goals of mathematical physics gradually diverged from the prevailing views in Smirnov's chair. I acquired the conviction that instead of proving existence theorems it would be more worthwhile to do something that my colleagues in theoretical physics didn't know about. My first paper on uniqueness of the inverse scattering problem for the multi-dimensional Schrödinger operator was duplicated in the USA a couple of years later. Later on, I worked on two aspects of scattering theory — the multi-dimensional inverse problem and the three-particle problem — starting practically in a void.

This attitude to mathematical physics was only strengthened when I began to work on quantum field theory in the mid-1960s. As a result, the following understanding of the essence of mathematical physics formed in my mind: its main purpose is to use mathematical intuition to derive genuinely new results in basic physics. In that sense, mathematical physics and theoretical physics are rivals. Their striving to discover structural laws of matter coincide. However, the methods and even the comparative estimate of importance of the results may be significantly different.

Here it is appropriate to explain in what sense I am using the term *basic physics*. The adjective *basic*, when applied to classical science, has a large number of interpretations. In its broadest sense it is used to characterize research into new regularities in the world around us. In the narrow sense it applies only to the search for fundamental laws to which these regularities can be reduced.

For example, all chemical regularities are theoretically derivable from the Schrödinger equation for a system of electrons and nuclei. In other words, the fundamental basis of chemistry has already been discovered, in the narrow sense. This, of course, does not deprive chemistry of the right to call itself a basic science in the broader meaning of the term. The same can be said about mechanics and the modern physics of condensed matter. Although the majority of physical research is concentrated in the latter area at present, it is clear that all of its successes, including the theory of superfluidity and superconductivity, the theory of Bose–Einstein condensation, and the quantum Hall effect, have as a basis nonrelativistic multi-particle quantum theory.

Elementary particle physics remains an unfinished basic problem in the narrow sense. This places that area of physics in a special position. And it is here that modern MPh has the greatest chance for a breakthrough.

Up to the present, all of physics has developed in accordance with a traditional cycle: experiment – theoretical interpretation – new experiment. This is true also of elementary particle physics.

Traditionally, theory came after experiment. That fact alone imposed severe restrictions on theoretical work. Even the most brilliant idea, if it received no support from the then-current level of experiment, was immediately declared false and rejected. It is typical that the role of censor was often played by the theoreticians themselves; and, as far as I can judge, the great L. D. Landau and W. Pauli were the strictest among them. There were, of course, weighty grounds for all this.

On the other hand, the development of mathematics, on which applications indisputably exerted a strong influence, has always had its own internal logic. Ideas were esteemed not in connection with their practical importance, but on the basis of aesthetic criteria. In that sense we can speak of totalitarianism in physics and democracy in mathematics and its inherent intuition. And it is precisely this freedom that may turn out to be useful for elementary particle physics, which up to now has been based on progress in accelerator technology. The high cost and limited potential of the latter will soon become an insuperable obstacle to further development. And here mathematical intuition may become an adequate substitute. Famous theoretical physicists with mathematical inclinations have spoken of this many times.

Here, for example, is what P. Dirac wrote in the early 1930s [5]:

> There are at present fundamental problems in theoretical physics awaiting solution, *e.g.*, the relativistic formulation of quantum mechanics and the nature of atomic nuclei (to be followed by more difficult problems such as the problem of life), the solution of which problems will presumably require a more drastic revision of our fundamental concepts than any that have gone before. Quite likely it

> will be beyond the power of human intelligence to get the necessary new ideas by direct attempts to formulate expermental data in mathematical terms. The theorist in the future will therefore have to proceed in a more indirect way. The most powerful method of advance that can be suggested at present is to employ all the resources of pure mathematics in attempts to perfect and generalise the mathematical formalism that forms the existing basis of theoretical physics, and *after* each success in this direction, to try to interpret the new mathematical features in terms of physical entities . . .

Similar views have been expressed more recently by C. N. Yang. I have not found a suitably compact quotation, but the spirit of his commentaries on his work, included in the collection [6], undoubtedly expresses this attitude. It has permeated all the private discussions that I have had with him since the early 1970s.

I believe that the dramatic story of the confirmation of gauge fields, as the basic means of describing interactions in quantum field theory, is a good illustration of the influence of mathematical intuition on the development of basic physics. Gauge fields, or Yang–Mills fields, were introduced in 1954 in a brief paper of Yang and Mills [7] devoted to the principle of gauge invariance for a generalized electromagnetic field. Geometrically, the meaning of this principle for an electromagnetic field was explained back in the 1920s in papers of V. A. Fock and H. Weyl ([8], [9]). They established an analogy between the gauge invariance (gradient invariance in the language of Fock) of electrodynamics and the principle of equivalence in the Einstein theory of gravitation. The gauge group in electrodynamics is commutative; it corresponds to multiplication of the complex field of a charged particle by a phase factor depending on the space-time coordinates. The Einstein theory of gravitation provides an example of a much more complicated gauge group, namely a group of diffeomorphisms. Then, describing spinors in the theory of gravity, Fock [8] and Weyl [9] independently used the formalism of a moving frame, in which the spin connection associated with local Lorentz rotations appeared. Thus the Lorentz group became an example of the first noncommutative gauge Lie group. In [8] one can, in fact, see all the important formulas of the theory of non-Abelian gauge fields. However, in contrast to the electromagnetic field, the spin connection is associated with space-time, not with the intrinsic space corresponding to an electric charge. It was the idea of such an intrinsic space that was first stated clearly by Weyl [9], who called electrodynamics the general theory of relativity in a charge space.

An alternative line of geometrization was based on the idea of T. Kaluza and O. Klein of a space-time of more than four dimensions. In the late 1930s, in connection with the appearance of the meson theory of nuclear interactions, Klein put forth a theory containing many elements of non-Abelian gauge fields [10]. By the 1950s this idea had been extended by W. Pauli. However, the prediction of the existence of charged vector fields with massless excitations contradicted experiment, and Pauli, faithful to his role as a censor, did not attempt to publish his results.

Thus, there is nothing surprising in the fact that Yang was sharply criticized by Pauli when he presented his paper at a seminar in Princeton. The dramatic description of this event can be found in the commentaries in [6]. Pauli was in the audience and immediately raised the question of the mass of quanta of the charged components of a multiplet of gauge fields. As explained above, Pauli was well acquainted with the differential geometry of non-Abelian vector fields, but did not permit himself to speak of them. And we now know well that Yang's boldness and his aesthetic sensitivity were rewarded. One now has good grounds for saying that Yang was acting in accord with mathematical intuition.

In 1954 the paper of Yang and Mills had not yet reached the forefront of high-energy theoretical physics. However, the idea of a charged space with a noncommutative symmetry group was becoming more and more popular in connection with the appearance of a steadily increasing number of new elementary particles and searches for a universal scheme of classifying them. It was at this stage that mathematical intuition and aesthetic considerations played the decisive role in promoting Yang–Mills fields.

In the early 1960s, R. Feynman was studying how to carry over his scheme of quantization of electrodynamics to the Einstein theory of gravitation. A purely technical obstacle — the large number of tensor indices — was delaying the work. On the advice of Gell-Mann he used the simpler case of Yang–Mills fields to work out the technical side of the quantization and noticed its fundamental difference from the case of electrodynamics with a commutative gauge group. Indeed, the naive generalization of the diagrammatic technique of perturbation theory, which had been developed for quantum electrodynamics, failed in the case of Yang–Mills fields. The unitarity of the S-matrix was violated. The result depended nontrivially on the longitudinal part of the propagator. Feynman restored unitarity in the single-loop approximation, by reconstructing the full scattering amplitude from its imaginary part. The result differed from the one obtained in the naive scheme and admitted an interpretation as the subtraction of the contribution from a certain fictitious particle. However, the extension of the scheme to the leading loops encountered combinatorial complications, and Feynman decided not to struggle with them (see [11]). His scheme was gradually developed by B. DeWitt in a very complicated paper [12]. For us it is important to emphasize that the apparent meaninglessness of the Yang–Mills field did not discourage Feynman from working with it. This also can be laid down to the influence of mathematical intuition.

Feynman's paper [11] became one of the points of departure for my work in quantum field theory, which I began in the mid-1960s together with Viktor Popov. Another, no less important, stimulus was the monograph of A. Lichnerowitz [13], which is devoted to the theory of connections in vector bundles. It could be seen from the book of Lichnerowitz that the Yang–Mills field has a clear geometric meaning: it defines a connection in a bundle whose base is space-time and whose fiber is the linear space of representations of a charge group. Thus the Yang–Mills field took its natural place among the fields having geometric origins, between the electromagnetic field, which is a special case of it with a one-dimensional charge, and the Einstein gravitational field, which deals with bundles associated with the tangent bundle to the Riemannian manifold of space-time.

It became clear to me that the problem discovered by Feynman presented an opportunity not to be missed; and, despite the unsolved problem of the mass of quanta of a Yang–Mills field, it should be actively studied.

The geometric nature of the Yang–Mills fields indicated a natural route to overcome the difficulties with the diagrammatic rules. The formulation of quantum field theory in terms of the continuous Feynman integral turned out to be the most suitable from the technical point of view.

Indeed, to take into account the principle of gauge equivalence one has only to integrate over classes of equivalent fields rather than over all field configurations. Once this idea is recognized, the technical implementation of it presents no difficulty. As a result, in late 1966 Popov and I stated modified diagrammatic rules adapted for all orders of perturbation theory. The field of fictitious particles arose as an auxiliary variable in the integral representation of a nontrivial functional determinant that belonged to a measure on the set of gauge orbits.

Our results, which were published in mid-1967 ([14], [15]), did not immediately attract the attention of physicists. Besides, the time when our paper was written was not favorable. Quantum field theory was effectively forbidden, especially in the Soviet Union, as the result of the influence of Landau. The phrase "Hamiltonian methods are dead," from his paper devoted to Pauli's 60th birthday, illustrates very well his extreme views. The grounds for this attitude were quite solid. The theory was not based on experiment, but on the study of the effects of renormalization, which led Landau and his colleagues to the conviction that the renormalized physical charge equals zero for all types of local interactions. Thus Popov and I had no chance of publishing a detailed article in the leading Soviet journals. We were able to take advantage of the possibility of printing a brief communication [14] in the European journal *Physics Letters* (such possibility was a new thing at the time) and were favored by another new occasion to publish our detailed exposition as a preprint [15] of the recently organized Institute of Theoretical Physics in Kiev. Subsequently, after Yang–Mills fields had become solidly established in the world of physics, this preprint was translated into English (as a preprint of the Fermi Laboratory in the USA). From the preface to this publication written by B. Li, it follows that our preprint was known in the West as early as 1968.

The decisive role in establishing our diagrammatic rules in physics was played by papers of G. 't Hooft [16] (who has recently been awarded a Nobel Prize in physics), devoted to models that contain the scalar field of Higgs along with Yang–Mills fields, and the discovery of dimensional transmutation (a term due to S. Coleman [17]). The problem of mass in the first case was solved by spontaneous symmetry breaking. The second concept was based on the discovery of asymptotic freedom. A considerable literature has been devoted to the dramatic story of this development. I cite the recent papers of 't Hooft [18] and Gross [19], in which the participants in the story share their reminiscences of all these developments. We note that it was the Yang–Mills theory that seems to have been the only counterexample to Landau's claim about the null-charge.

The result of this development was the standard model of elementary particle interaction, which from the mid-1970s to the present has been the basic foundation of high-energy physics. What is important for our discussion is that the paper [14], based on mathematical intuition, preceded the papers made in the traditions of theoretical physics.

The standard model did not complete the fundamental basis of high-energy physics. Gravitational interaction (having, as noted above, a different geometric interpretation) does not fit into it. The unification of quantum principles, relativity, and gravitation, has still not been accomplished. We have every reason to believe that modern mathematical physics and its intuition will play a leading role here. Indeed, the new generation of theoretical physicists has received an incomparably higher mathematical education and has not been subjected to the pressure of authorities defending the purity of physical thought and/or terminology. Moreover, many professional mathematicians, enchanted by the beauty of physical problems and the methods applied, have come over to the side of mathematical physics.

Let us present here a quotation from a manifesto issued by Robert MacPherson while preparing the "Year of Quantum Field Theory" of the Mathematics School at the Institute for Advanced Study in Princeton. After enumerating the divisions of mathematics and physics that do not form part of the program of the School, MacPherson writes: “Rather, the goal is to develop the sort of intuition common among physicists for those who are used to thought processes stemming from geometry and algebra.” I believe that this type of intuition can legitimately be called the intuition of modern mathematical physics.

The combination of the new generation of theoretical physicists and mathematicians attracted by physical problems is an unusual intellectual force. In the new century we shall learn whether its activity can replace the traditional experimental basis of the development of physics, and its inherent intuition.

Bibliography

[1] B. Nagel. The discussion concerning the Nobel Prize for Max Planck. In: *Science, Technology, and Society in the Time of Alfred Nobel*. Oxford: Pergamon Press, 1982.

[2] P. M. Morse, H. Feshbach. *Methods of Theoretical Physics*. New York: McGraw-Hill, 1953.

[3] R. Courant, D. Hilbert. *Methods of Mathematical Physics*. New York: Interscience, 1953.

[4] S. L. Sobolev. *Some Applications of Functional Analysis in Mathematical Physics*. Leningrad State University, 1950 (Russian).

[5] P. Dirac. Quantized singularities in the electromagnetic field. *Proc. Roy. Soc. London*, 1931, **A133**, 60–72.

[6] C. N. Yang. *Selected Papers, 1945–1980, with Commentary*. San Francisco: Freeman & Co., 1983.

[7] C. N. Yang, R. Mills. Conservation of isotopic spin and isotopic gauge invariance. *Phys. Rev.*, 1954, **96**, 191–195.

[8] V. Fock. L’équation d’onde de Dirac et la géométrie de Riemann. *J. Phys. Radium*, 1929, **70**, 392–405.

[9] H. Weyl. Electron and gravitation. *Z. Phys.*, 1929, **56**, 330–352.

[10] O. Klein. On the theory of charged fields. *Surveys High Energy Phys.*, 1986, **5**, p. 269.

[11] R. Feynman. Quantum theory of gravitation. *Acta Phys. Pol.*, 1963, **24**, p. 697.

[12] B. DeWitt. Quantum theory of gravity. II. The manifestly covariant theory. *Phys. Rev.*, 1967, **162**, 1195–1239.

[13] A. Lichnerowitz. *Théorie globale des connections et des groupes d’holonomie*. Rome: Cremonese, 1955.

[14] L. Faddeev, V. Popov. Feynman diagrams for the Yang–Mills fields. *Phys. Lett.*, 1967, **B25**, p. 30.

[15] V. Popov, L. Faddeev. Perturbation theory for gauge-invariant fields. Preprint, Institute for Theoretical Physics, No. 67-36, Kiev, 1967 (Russian).

[16] G. 't Hooft. Renormalizable Lagrangians for massive Yang–Mills fields. *Nucl. Phys. B*, 1971, **35**(1), 167–188.

[17] S. Coleman. Secret symmetries: an introduction to spontaneous symmetry breakdown and gauge fields. In: *Laws of Hadronic Matter*. International School of Subnuclear Physics (Erice, 1973). New York: Academic Press, 1975, 138–223 (Subnuclear Ser., **11**).

[18] G. 't Hooft. When was asymptotic freedom discovered? Rehabilitation of quantum field theory. Preprint, HEP-TH, 98-08-154.

[19] D. Gross. Twenty years of asymptotic freedom. Preprint, HEP-TH, 98-09-060.

R. V. Gamkrelidze

Discovery of the Maximum Principle

Presented by the author

1. Initial Formulation of the Maximum Principle

In mid-fifties Lev Semenovich Pontryagin abandoned topology, never to return back to it, and completely devoted himself to purely engineering problems of mathematics. He organized at the Steklov Mathematical Institute a seminar in applied problems of mathematics, often inviting theoretical engineers as speakers, since he considered a professional command over the purely engineering part of the problem under investigation to be mandatory for its adequate mathematical development.

The activity in the seminar culminated very soon in the formulation of two major mathematical problems. One of them developed into the general theory of singularly perturbed systems of ordinary differential equations. The second problem brought us to the discovery of the Maximum Principle and to the emergence of optimal control theory.

L. S. was led to the formulation of the general time-optimal problem by an attempt to solve a concrete fifth-order system of ordinary differential equations with three control parameters related to optimal maneuvers of an aircraft, which was proposed to him by two Air Force colonels during their visit to the Steklov Institute in the early spring of 1955. Two of the control parameters entered the equations linearly and were bounded, hence from the beginning it was clear that they could not be found by classical methods, as solutions of the Euler equations. The problem was highly specific, and very soon L. S. realized that some general guidelines were needed in order to tackle the problem. I remember he even said half-jokingly, "we must invent a new calculus of variations." As a result, the following general time-optimal problem was formulated.

Consider a controlled object represented in the n-dimensional state space of points

$$x = \begin{pmatrix} x^1 \\ \vdots \\ x^n \end{pmatrix} \in \mathbb{R}^n$$

by a system of n autonomous differential equations with r control parameters $u^1, \dots, u^r$:

$$\dot{x}^i = f^i(x^1, \dots, x^n; u^1, \dots, u^r) = f^i(x, u), \quad i = 1, \dots, n. \tag{1}$$

Initially it was supposed that the control vector

$$u = \begin{pmatrix} u^1 \\ \vdots \\ u^r \end{pmatrix}$$

attains its values from an open set $U \subset \mathbb{R}^r$. The most interesting case for control problems, that of a closed set U, was considered later. To denote control parameters, the letter "u" was chosen, as the first letter of the Russian word for "control" — *upravlenie*.

Formulation of the problem:

Given initial and terminal states $x_0, x_1 \in \mathbb{R}^n$, find a control function $u(t) \in U$ $\forall t \in [t_0, t_1]$, such that it minimizes the transition time of the state point x, moving from x_0 to x_1 according to the non–autonomous system

$$\dot{x}^i = f^i(x(t), u(t)), \quad i = 1, \dots, n.$$

Thus, we come to the time-optimal control $u(t)$ and the corresponding time-optimal trajectory $x(t)$, $t_0 \leqslant t \leqslant t_1$, which satisfies the boundary value problem

$$\dot{x}^i(t) = f^i(x(t), u(t)), \quad x(t_0) = x_0, \quad x(t_1) = x_1,$$

and minimizes the transition time,

$$t_1 - t_0 = \min.$$

It should be noticed that the general optimal problem with an arbitrary integral-type functional is easily reduced to the formulated time-optimal problem, so that by solving the time-optimal problem with fixed boundary conditions we actually overcome all essential difficulties inherent in the general case.

The first and the most important step toward the final solution was made by L. S. right after the formulation of the problem, during three days, or better to say, during three consecutive sleepless nights. He suffered from severe insomnia and very often used to do math all night long in bed. As a result, he completely disrupted his sleep in his later years and systematically took sleeping pills in great quantities.

Thanks to his wonderful geometric insight, he derived, from very simple duality considerations about the first order variational equation, the initial version of necessary conditions, introducing an auxiliary covector-function $\psi(t) = (\psi_1(t), \ldots, \psi_n(t))$ subject to the adjoint system of differential equations,

$$\frac{\mathrm{d}\psi_i}{\mathrm{d}t} = -\sum_{\alpha=1}^{n} \psi_\alpha \frac{\partial f^\alpha}{\partial x^i}(x,u), \quad i = 1,\ldots,n. \tag{2}$$

This was the first appearance in optimal control theory of the adjoint system, which turned out to be of crucial importance for the whole subject. Actually, L. S. constructed for the first time, for the needs of optimization, what is usually called the *Hamiltonian lift* of the initial family of vector fields on the state space of the problem into its cotangent bundle, the phase space of the problem, see Section 5.

The initial formulation of necessary conditions reported by L. S. at the seminar right after they were derived, is expressed in formulas

$$\frac{\mathrm{d}x^i(t)}{\mathrm{d}t} = f^i(x(t),u(t)), \qquad x(t_0) = x_0, \quad x(t_1) = x_1, \tag{3.1}$$

$$\frac{\mathrm{d}\psi_i(t)}{\mathrm{d}t} = -\sum_{\alpha=1}^{n} \psi_\alpha(t)\frac{\partial f^\alpha}{\partial x^i}(x(t),u(t)) = -\psi(t)\frac{\partial f}{\partial x^i}(x(t),u(t)), \tag{3.2}$$

$$\sum_{\alpha=1}^{n} \psi_\alpha(t)\frac{\partial f^\alpha}{\partial u^j}(x(t),u(t)) = \psi(t)\frac{\partial f}{\partial u^j}(x(t),u(t)) = 0, \tag{3.3}$$

$$u(t) \in U, \ \forall t \in [t_0,t_1], \quad i = 1,\ldots,n; \ j = 1,\ldots,r.$$

They assert that if $x(t), u(t)$, $t_0 \leqslant t \leqslant t_1$, *is an optimal solution, then there exists a nonzero covector-function* $\psi(t)$ *such that* $\psi(t), x(t), u(t)$, $t_0 \leqslant t \leqslant t_1$, *is a solution of the system of differential equations* (3.1)–(3.2)*, and along the solution, for every* t*,* r *"finite" equations* (3.3) *are satisfied.*

This formulation supposes that the set U of admissible values of control is open, though, as I already mentioned above, from the very beginning it was clear to L. S. that the ultimate result should be applicable to closed sets as well.

I shall describe now Pontryagin's very simple and straightforward geometric arguments which directly led to the equations (3).

Consider an arbitrary admissible variation of the optimal control $u(t)$,

$$\delta u(t) = \begin{pmatrix} \delta u^1(t) \\ \vdots \\ \delta u^r(t) \end{pmatrix}, \quad u(t) + \delta u(t) \in U, \quad t_0 \leqslant t \leqslant t_1,$$

and the corresponding perturbation $\Delta x(t)$, $t_0 \leqslant t \leqslant t_1$, of the optimal trajectory $x(t)$,

$$\begin{aligned} \frac{\mathrm{d}}{\mathrm{d}t}\Delta x^i(t) &= f^i(x(t)+\Delta x(t), u(t)+\delta u(t)) - f^i(x(t),u(t)) \\ &= \sum_{\alpha=1}^{n} \frac{\partial f^i}{\partial x^\alpha}(x(t),u(t))\Delta x^\alpha(t) + \sum_{\beta=1}^{r} \frac{\partial f^i}{\partial u^\beta}(x(t),u(t))\delta u^\beta(t) + \cdots, \\ &\qquad \Delta x(t_0) = 0, \quad i = 1,\dots,n. \end{aligned}$$

If we ignore quadratic and higher order terms in the right–hand side, we obtain the *first* (*linear*) *variation*

$$\delta x(t) = \begin{pmatrix} \delta x^1(t) \\ \vdots \\ \delta x^n(t) \end{pmatrix}, \quad t_0 \leqslant t \leqslant t_1,$$

of the optimal trajectory, which satisfies the standard *linear variational system*

$$\frac{\mathrm{d}}{\mathrm{d}t}\delta x^i(t) = \sum_{\alpha=1}^{n} \frac{\partial f^i}{\partial x^\alpha}(x(t),u(t))\delta x^\alpha + \sum_{\beta=1}^{r} \frac{\partial f^i}{\partial u^\beta}(x(t),u(t))\delta u^\beta, \tag{4}$$
$$\delta x(t_0) = 0, \quad i = 1,\dots,n.$$

The mapping $\{\delta u(t), t_0 \leqslant t \leqslant t_1\} \mapsto \delta x(t_1)$ is a linear operator from the space of variations $\delta u(t), t_0 \leqslant t \leqslant t_1$ into the state space $\mathbb{R}^n$. Since the set of admissible values of u is supposed to be open, the admissible variations $\delta u(t)$ are arbitrary (piecewise continuous) functions. Hence the set

$$L = x(t_1) + \{\delta x(t_1) \mid \delta x(t_0) = 0\} = x(t_1) + \Gamma \tag{5}$$

is a plane through $x(t_1)$ in $\mathbb{R}^n$, Γ is the corresponding subspace of $\mathbb{R}^n$. Since $x(t),\ t_0 \leqslant t \leqslant t_1$, is optimal, we obtain the relation

$$\dim L = \dim \Gamma \leqslant n-1,$$

which is easily derived from the implicit function theorem. Hence, there exists a (nonzero) covector $\chi = (\chi_1, \dots, \chi_n)$ orthogonal to Γ,

$$\sum_{\alpha=1}^{n} \chi_\alpha \delta x^\alpha(t_1) = \chi \delta x(t_1) = 0 \quad \forall\, \delta x(t_1) \in \Gamma.$$

To express $\delta x(t_1)$ through $\delta u(t)$, we must integrate the variational equations (4). For this purpose we introduce the fundamental matrix $\Phi(t)$ of the corresponding homogeneous system,

$$\frac{\mathrm{d}}{\mathrm{d}t}\delta x^i = \sum_{\alpha=1}^{n} \frac{\partial f^i}{\partial x^\alpha}(x(t),u(t))\delta x^\alpha, \quad i = 1,\dots,n,$$

and the inverse $\Psi(t) = \Phi^{-1}(t)$. They satisfy matrix differential equations

$$\frac{\mathrm{d}}{\mathrm{d}t}\Phi = \left\| \frac{\partial f^i}{\partial x^j}(x(t),u(t)) \right\| \Phi, \qquad \frac{\mathrm{d}}{\mathrm{d}t}\Psi = -\Psi \left\| \frac{\partial f^i}{\partial x^j}(x(t),u(t)) \right\|. \tag{6}$$

The solution of (4) with the initial condition $\delta x(t_0) = 0$ is represented as

$$\delta x(t) = \Phi(t) \int_{t_0}^{t} \Psi(\tau) \left\| \frac{\partial f^i}{\partial u^j}(x(\tau),u(\tau)) \right\| \begin{pmatrix} \delta u^1(\tau) \\ \vdots \\ \delta u^r(\tau) \end{pmatrix} \mathrm{d}\tau, \quad t \in [t_0, t_1],$$

hence

$$\left.\begin{aligned} \chi \delta x(t_1) = \chi \Phi(t_1) \int_{t_0}^{t_1} \Psi(\tau) \left\| \frac{\partial f^i}{\partial u^j}(x(\tau),u(\tau)) \right\| \begin{pmatrix} \delta u^1(\tau) \\ \vdots \\ \delta u^r(\tau) \end{pmatrix} \mathrm{d}\tau = 0 \\ \forall\, \delta u(\tau) = \begin{pmatrix} \delta u^1(\tau) \\ \vdots \\ \delta u^r(\tau) \end{pmatrix}. \end{aligned}\right\} \tag{7}$$

The n-dimensional covector

$$\psi(t) = (\psi_1(t), \ldots, \psi_n(t)) = \chi\Phi(t_1)\Psi(t), \quad t_0 \leqslant t \leqslant t_1,$$

is nonzero and satisfies, according to (6), the vector differential equation

$$\frac{\mathrm{d}}{\mathrm{d}t}\psi(t) = -\psi(t)\left\|\frac{\partial f^i}{\partial x^j}(x(t),u(t))\right\|,$$

which coincides with the adjoint system (3.2). Finally, the equation (7) attains the form

$$\int_{t_0}^{t_1} \psi(\tau)\left\|\frac{\partial f^i}{\partial u^j}(x(\tau),u(\tau))\right\|\delta u(\tau)\,\mathrm{d}\tau = 0,$$

or

$$\int_{t_0}^{t_1} \sum_{\beta=1}^{r}\sum_{\alpha=1}^{n} \psi_\alpha(\tau)\frac{\partial f^\alpha}{\partial u^\beta}(x(\tau),u(\tau))\,\delta u^\beta(\tau)\,\mathrm{d}\tau = 0.$$

Since the control variations $\delta u^\beta(\tau)$, $t_0 \leqslant t \leqslant t_1$, are arbitrary functions, we obtain the equations (3.3) and come to the optimality conditions (3) formulated above. They easily imply the Euler–Lagrange equations for the Lagrange problem of the classical calculus of variations.

2. The Second Variation

As soon as the equations (3) were obtained, L. S. recognized the decisive role of the covector-function $\psi(t)$ and the adjoint system (2) for the whole problem. He considered, in the generic case, r finite equations (3.3) as conditions, which eliminate r control parameters $u^1, \ldots, u^r$ from system (3), thus making it possible to solve uniquely the $2n$-th order system of differential equations (3.1)–(3.2) with a given initial condition $x(t_0) = x_0$ and an arbitrary (nonzero) initial condition for ψ. All such solutions were declared as *extremals of the problem*, from which the optimal solutions were to be derived.

Pontryagin's idea about a universal procedure of elimination of control parameters, which reduces the problem of determining extremals to solving ordinary differential equations with given boundary conditions, found its ultimate realization in the maximum principle, which was formulated by him several months later after his first report at the seminar, and was supported by the subsequent advancements obtained meanwhile at the seminar.

After his talk in the seminar, Pontryagin suggested to V. Boltyanskii and me, his former students and close collaborators at that time, to join him in his investigations of the problem. V. Boltyanskii held a formal position at the

Steklov Institute as Pontryagin's assistant, helping him in everyday computations and manuscript editing; I was a young member of the department of the Steklov Institute headed by Pontryagin.

Pontryagin's vision of the problem at this early stage of development could be described as follows.

Instead of considering the boundary value problem with fixed endpoints for the controlled system (3.1), we should only fix the initial point x_0, take an arbitrary initial value $\psi(t_0) = \psi_0 \neq 0$, and solve the system of $2n+r$ equations (3.1)–(3.3) with $2n+r$ unknowns x^i, ψ_j, u^k, proceeding along an arbitrary extremal emanating from the point x_0. This should be possible, since the r control parameters u^k are, "in general," successfully eliminated by r conditions (3.3), hence, $2n$ unknown parameters are left, x^i, ψ_j, subject to the system of $2n$ differential equations (3.1)–(3.2) and the initial conditions $x(t_0) = x_0$, $\psi(t_0) = \psi_0$. Since the adjoint system (3.2) is linear in ψ, the function $\psi(t)$ is defined up to a nonzero constant factor. Hence we can normalize the initial value $\psi(t_0)$, obtaining thus the $(n-1)$-dimensional sphere of the initial values of ψ, which should generate an $(n-1)$-parameter family of extremal trajectories of the problem, emanating from the point x_0.

According to the given picture, the final goal of the program, as initially formulated by Pontryagin, supposed to express in a reasonable way extremals $\psi(t), x(t)$, $x(t_0) = x_0$, as solutions of the system (3.1)–(3.3), through the initial value ψ_0. Today we recognize in the given formulation the problem of controllability in its simplest setting. Certainly at that stage, before the maximum principle was not even formulated, it was practically impossible to obtain in this direction any nontrivial results.

I was fascinated by Pontryagin's geometric approach and got an idea how to apply this picture to investigate the problem up to the second order approximation. So, we decided to split the investigation in two directions, Pontryagin, together with Boltyanskii, pursued the problem in the controllability direction, I started to investigate the second variation of the problem. As it turned out, this latter direction led to the formulation of the maximum principle.

Necessary conditions of optimality, expressed by equations (3), are derived from purely first–order approximation. They are independent of "general position" considerations, which were used by L. S. only to support his view on "finite equations" (3.3) as a regular elimination procedure.

My second order considerations required, from the very beginning, general position assumptions, which were overcome only in the final version of Boltyanskii's proof. The set of admissible values of the control parameters was still supposed open.

Take an arbitrary "generic" solution of the optimal problem, $x(t), u(t)$, $t_0 \leqslant t \leqslant t_1$, which means that the plane L in (5) is of maximal dimension,

$$\dim L = \dim \Gamma = n-1,$$

and the trajectory $x(t)$ intersects L at $x(t_1)$ transversally (is not tangent to L). Hence, L divides $\mathbb{R}^n$ in distinguishable half-spaces, $\mathbb{R}^n_-$ before $x(t)$ intersects L, $\mathbb{R}^n_+$ after the intersection. Every variation $\delta u(t)$ displaces the endpoint $x(t_1)$ in the first order into the hyperplane L, $x(t_1) + \delta x(t_1) \in L$. The real displacement $\Delta x(t_1)$ is certainly nonlinear in δu and, generally, stays off the hyperplane Γ,

$$x(t_1) + \Delta x(t_1) \in \mathbb{R}^n_- \quad \text{or} \quad x(t_1) + \Delta x(t_1) \in \mathbb{R}^n_+.$$

Denote from now on the first variation $\delta x(t)$ by $\delta_1 x(t)$, and let K be the kernel of the linear operator from the space of control variations into the space of first variations of $x(t)$ for $t = t_1$, given by

$$\{\delta u(t),\ t_0 \leqslant t \leqslant t_1\} \mapsto \delta_1 x(t_1) = \Phi(t_1) \int\limits_{t_0}^{t_1} \Psi(\tau) \left\| \frac{\partial f^i}{\partial u^j}(x(\tau), u(\tau)) \right\| \delta u(\tau)\, \mathrm{d}\tau.$$

Define the *second variation* $\delta_2 x(t)$, $t_0 \leqslant t \leqslant t_1$, of $x(t)$ as the solution of the linear nonhomogeneous equation

$$\begin{aligned}
\frac{\mathrm{d}}{\mathrm{d}t}\delta_2 x(t) = {} & \left\| \frac{\partial f^\alpha}{\partial x^\beta}(x(t), u(t)) \right\| \delta_2 x + \delta u(t)^* \left\| \frac{\partial^2 f}{\partial u^\alpha \partial u^\beta}(x(t), u(t)) \right\| \delta u(t) \\
& + \delta_1 x(t)^* \left\| \frac{\partial^2 f}{\partial x^\alpha \partial u^\beta}(x(t), u(t)) \right\| \delta u(t) + \delta_1 x(t)^* \left\| \frac{\partial^2 f}{\partial x^\alpha \partial x^\beta}(x(t), u(t)) \right\| \delta_1 x(t), \\
\delta_2 x(t_0) = 0, \quad & \delta u(t)^* = (\delta u^1(t), \ldots, \delta u^r(t)), \quad \delta_1 x(t)^* = (\delta_1 x^1(t), \ldots, \delta_1 x^n(t)),
\end{aligned}$$

which differs from (4) only by the nonhomogeneous part, quadratic in δu. The displacement of the endpoint $x(t_1)$ up to the second order is given by the vector $\delta_1 x(t_1) + \delta_2 x(t_1)$.

The key geometric fact for a generic optimal trajectory $x(t)$, $t_0 \leqslant t \leqslant t_1$, consists in the assertion that the second order displacement of its endpoint, considered on the kernel K, belongs to the half-space $\mathbb{R}^n_-$,

$$x(t_1) + \delta_1 x(t_1) + \delta_2 x(t_1) \in \mathbb{R}^n_- \iff x(t_1) + \delta_2 x(t_1) \in \mathbb{R}^n_- \quad \forall\, \delta u \in K.$$

Hence, we come to the conclusion that, additionally to the system (3.1)–(3.3), as a necessary condition of the second order, the following integral quadratic form in δu is nonpositive, provided the covector $\psi(t_1)$, which is transversal to L, is correctly normalized (directed toward the half-space $\mathbb{R}^n_+$),

$$\psi(t_1)\delta_2 x(t_1) = \sum_{i=1}^{n} \int_{t_0}^{t_1} \psi_i(\tau) \left\{ \delta u(\tau)^* \left\| \frac{\partial^2 f^i}{\partial u^\alpha \partial u^\beta}(x(\tau),u(\tau)) \right\| \delta u(\tau) \right.$$
$$+ \delta_1 x(\tau)^* \left\| \frac{\partial^2 f^i}{\partial x^\alpha \partial u^\beta}(x(\tau),u(\tau)) \right\| \delta u(\tau)$$
$$\left. + \delta_1 x(\tau)^* \left\| \frac{\partial^2 f^i}{\partial x^\alpha \partial x^\beta}(x(\tau),u(\tau)) \right\| \delta_1 x(\tau) \right\} \mathrm{d}\tau \leqslant 0 \quad \forall \delta u \in K.$$

After some elaborate investigation of this integral quadratic form, I came to the conclusion that its nonpositivity on K implies the nonpositivity of its singular part on K, hence the pointwise nonpositivity of the following $r \times r$ matrix $\forall t \in [t_0, t_1]$:

$$\sum_{i=1}^{n} \psi_i(t) \left\| \frac{\partial^2 f^i}{\partial u^\alpha \partial u^\beta}(x(t),u(t)) \right\| = \left\| \sum_{i=1}^{n} \psi_i(t) \frac{\partial^2 f^i}{\partial u^\alpha \partial u^\beta}(x(t),u(t)) \right\|.$$

Thus, we come to the final form of the second order optimality condition, which is satisfied, together with the first order conditions (3), by every generic optimal solution,

$$v^* \left\| \sum_{i=1}^{n} \psi_i(t) \frac{\partial^2 f^i}{\partial u^\alpha \partial u^\beta}(x(t),u(t)) \right\| v \leqslant 0, \qquad \forall v \in \mathbb{R}^r,\ \forall t \in [t_0, t_1]. \tag{8}$$

3. Formulation of the Maximum Principle in its Final Form

Collecting all necessary conditions (3.1)–(3.3), (8) together, we immediately recognize that a certain stable combination of symbols reappears in all of them, the scalar function of three arguments ψ, x, u,

$$H(\psi, x, u) = \sum_{\alpha=1}^{n} \psi_\alpha f^\alpha(x,u) = \psi f(x,u). \tag{9}$$

It enables us two rewrite the system (3.1)–(3.2) as a Hamiltonian system (10.1) with the Hamiltonian function (9), together with additional conditions (3.3), (8), written as (10.2)–(10.3):

$$\left.\begin{aligned} \frac{\mathrm{d}x^i(t)}{\mathrm{d}t} &= \frac{\partial H}{\partial \psi_i}(\psi(t),x(t),u(t)), \\ \frac{\mathrm{d}\psi_i(t)}{\mathrm{d}t} &= -\frac{\partial H}{\partial x^i}(\psi(t),x(t),u(t)), \\ i &= 1,\dots,n, \end{aligned}\right\} \tag{10.1}$$

$$\frac{\partial H}{\partial u^j}(\psi(t),x(t),u(t)) = 0, \quad \forall t \in [t_0,t_1],\ j = 1,\dots,r, \tag{10.2}$$

$$\delta u^* \left\| \frac{\partial^2 H}{\partial u^\alpha \partial u^\beta}(\psi(t),x(t),u(t)) \right\| \delta u \leqslant 0 \quad \forall\, \delta u \in \mathbb{R}^r. \tag{10.3}$$

They assert that generic extremals are solutions of the Hamiltonian system (10.1)*, and, according to* (10.2)*, their points are stationary points of the Hamiltonian* (9) *with respect to the control parameters u^i. Furthermore, according to* (10.3)*, along regular extremals, for which the form* (10.3) *is definite, the function H attains its local maximum with respect to u.*

We can combine two independent conditions (10.2)–(10.3) into one condition and write

$$H(\psi(t),x(t),u(t)) = \max_{u \in O_t} H(\psi(t),x(t),u), \tag{10.4}$$

where O_t is a neighborhood of $u(t)$. Furthermore, the equations (10.1)–(10.2) imply,

$$\frac{\mathrm{d}H}{\mathrm{d}t}(\psi(t),x(t),u(t)) = \sum_{\alpha=1}^{n}\left(\frac{\partial H}{\partial \psi_\alpha}\frac{\mathrm{d}\psi_\alpha}{\mathrm{d}t} + \frac{\partial H}{\partial x^\alpha}\frac{\mathrm{d}x^\alpha}{\mathrm{d}t}\right) + \sum_{\beta=1}^{r}\frac{\partial H}{\partial u^\beta}\frac{\mathrm{d}u^\beta}{\mathrm{d}t} \equiv 0.$$

It is also easy to show that $H(\psi(t),x(t),u(t))$, as a function of t, is continuous, even if the control function $u(t)$ has jumps. Hence, taking into account the generic character of the solution — the trajectory $x(t)$ is transversal to L at $x(t_1)$, we obtain

$$H(\psi(t),x(t),u(t)) \equiv \mathrm{const} = \psi(t_1)f(x(t_1),u(t_1)) > 0. \tag{11}$$

After the equations (10.1)–(10.3) were written, L. S. realized that the universal elimination method of the control parameters, he was searching for, was found. He replaced the local maximum condition (10.4) by the global maximum over the whole set U, the *"Pontryagin maximum condition"* (12), which made any restrictive assumptions about the admissible set U superfluous,

$$H(\psi(t),x(t),u(t)) = \max_{u \in U} H(\psi(t),x(t),u) \equiv \mathrm{const} \geqslant 0 \tag{12}$$

Thus, he came to the final formulation of the maximum principle, combining the Hamiltonian system (10.1) with the maximum condition (12) and dropping off any assumptions about genericity of the solutions or the nature of the admissible set U.

The Maximum Principle. *Suppose a controlled equation is given,*

$$\dot{x} = f(x,u), \quad x = \begin{pmatrix} x^1 \\ \vdots \\ x^n \end{pmatrix} \in \mathbb{R}^n, \quad u = \begin{pmatrix} u^1 \\ \vdots \\ u^r \end{pmatrix} \in U \subset \mathbb{R}^r,$$

where the admissible set U is arbitrary. We introduce the Hamiltonian function of the problem,

$$H(\psi,x,u) = \psi f(x,u) = \sum_{\alpha=1}^{n} \psi_\alpha f^\alpha(x,u), \tag{13.1}$$

which depends on three arguments — the covector $\psi = (\psi_1,\ldots,\psi_n)$ and the vectors x, u. If $u(t)$, $t_0 \leqslant t \leqslant t_1$, is a time-optimal control, $x(t)$, $t_0 \leqslant t \leqslant t_1$, is the corresponding time-optimal trajectory,

$$\frac{\mathrm{d}}{\mathrm{d}t}x(t) = f(x(t),u(t)), \quad t_0 \leqslant t \leqslant t_1; \quad t_1 - t_0 = \min,$$

then there exists a nonzero covector function $\psi(t)$ such that the triple

$$\psi(t), x(t), u(t), \quad t_0 \leqslant t \leqslant t_1,$$

is a solution of the Hamiltonian system (13.2)*, and the maximum condition* (13.3) *holds,*

$$\left.\begin{aligned} \frac{\mathrm{d}x(t)}{\mathrm{d}t} &= \frac{\partial H}{\partial \psi}(\psi(t),x(t),u(t)) \\ \frac{\mathrm{d}\psi(t)}{\mathrm{d}t} &= -\frac{\partial H}{\partial x}(\psi(t),x(t),u(t)) \end{aligned}\right\} \tag{13.2}$$

$$H(\psi(t),x(t),u(t)) = \max_{u\in U} H(\psi(t),x(t),u) \equiv \text{const} \geqslant 0, \quad \forall t \in [t_0,t_1]. \tag{13.3}$$

In this formulation, the maximum condition (13.3) could be viewed not only as a universal elimination method, but also as a generalization of the Legendre transformation from the state-space variables (x,u) to the phase-space variables (ψ,x).

4. Proof of the Maximum Principle

It took approximately a year before a full proof of the maximum principle was found. The final formulation of the maximum principle given above was published as a short notice in [1], long before its complete proof, though the plausibility of the conjecture was strongly supported by all further developments in the field.

Meanwhile, I developed in [2], [3], the theory of linear time-optimal systems of the form

$$\frac{dx}{dt} = Ax + Bu, \quad u \in U, \tag{14}$$

U is a compact polyhedron, A and B are constant matrices. For arbitrary systems (14) the maximum principle was proved and the existence theorem of optimal solutions established. The notion of nondegenerate linear systems (14) was introduced, an effective criterion for nondegeneracy established, and a complete investigation of attainable sets given. Several years later, under the name of controllability condition, this criterion was exploited by R. Kalman in his investigations on linear control systems.

For nondegenerate linear systems (14) the equivalence of the global maximum principle to the local maximum condition was established. Every local maximum in u of the expression

$$H(\psi, x, u) = \psi Ax + \psi Bu, \quad u \in U,$$

is attained in the vertices of U and at the same time it is the global maximum. Hence, the solution $u(t)$ of the equation

$$H(\psi(t), x(t), u(t)) = \psi(t)Ax(t) + \max_{u \in U} \psi(t)Bu$$

is locally constant, coinciding with the vertices of U and having jumps from one vertex into another. The time moments of jumps, as well as the destination vertices, are uniquely indicated by the maximum condition.

Despite all these advances, there was no real progress in proving the maximum principle in the general nonlinear case, until Boltyanskii introduced the "needle variations" of the control function. Such variations are zero everywhere on the time-interval, except on several segments with a small total length, where they can attain arbitrary admissible values. And they have an important property of admitting an operation of convex combination, regardless of the shape of U. These variations made possible to prove the maximum principle in full generality, as formulated above. This was Boltyanskii's major contribution to the subject. He made the first publication of the proof separately, in [4].

In the initial version of Boltyanskii's proof both types of variations were used simultaneously, the needle variations and usual variations, which are small on the whole time interval under consideration.

After Boltyanskii's first report, Pontryagin immediately recognized the power of the needle variations and nonnecessity for the proof of the usual variations. In this final form the whole subject was published in our joint paper [5], and later, in Pontryagin's talk at the International Mathematical Congress in Edinburgh, [6]. Since then, the needle variations and their generalizations are used as a standard tool for proving the maximum principle and the higher order necessary conditions of optimality.

Several years later after the first proof, I discovered a new proof of the maximum principle, based not on needle variations, but rather on a completely different idea of the chattering control state, [7]. A detailed exposition of this proof is given in my textbook on optimal control, [8].

5. Some Final Remarks about the Maximum Principle

Though formulated in 1955, the maximum principle was never changed, nor slightly improved, since then. All (first order) advancements were directed toward generalizations of the optimal problem itself, especially toward developing nonsmooth optimization, with corresponding first-order necessary conditions shaped after the maximum principle.

This could be explained, as I understand it, by the very nature of the maximum principle. Despite its seemingly purely analytic nature, it is deeply geometric and completely symplectic invariant already in its initial formulation. It prescribes a canonical transition from the initial problem to its reformulation on the cotangent bundle, mathematically much more flexible. Thus, the maximum principle could be viewed as kind of a *"symplectization functor"* from the initial optimal problem, defined on the state space, to its symplectic reformulation on the phase space, which is much richer by its mathematical implications.

To support this viewpoint, let me rewrite the initial control system in the "state-invariant" form (15) on a smooth manifold M,

$$\left.\begin{aligned} &\frac{\mathrm{d}x}{\mathrm{d}t} = f(x,u), \\ &f_u : x \mapsto f(x,u) \in TM, \ \ x \in M, \ \ u \in U, \end{aligned}\right\} \tag{15}$$

and consider the family of vector fields f_u as a family of scalar-valued functions H_u on the cotangent bundle T^*M, which are linear on fibers,

$$f_u \approx H_u = H_u(\xi) \in C^\infty(T^*M), \quad \xi \in T^*M, \quad H_u \text{ is linear on fibers.} \tag{16}$$

In the Pontryagin formulation, the family H_u is given by (13.1).

We obtain on T^*M a family of Hamiltonian vector fields $\vec{H}_u$, (17), or the Hamiltonian system (13.2) of the maximum principle. The field $\vec{H}_u$ is the canonical lift into the cotangent bundle T^*M of the field f_u, defined on the base manifold M,

$$\left.\begin{array}{c} \vec{H}_u \in \operatorname{Vect} T^*M, \ u \in U, \quad \text{is the Hamiltonian lift of } f_u; \\ \pi\colon T^*M \longrightarrow M, \quad \pi_*\vec{H}_u = f_u. \end{array}\right\} \tag{17}$$

The maximum principle asserts that if $x(t), u(t), t_0 \leqslant t \leqslant t_1$, *is an optimal solution, then there exists a trajectory* $\xi(t), t_0 \leqslant t \leqslant t_1$, *of the nonstationary Hamiltonian vector field* $\vec{H}_{u(t)}$, *covering the trajectory* $x(t)$, (18.1), *such that the maximum condition* (18.2) *holds*:

$$u(t), x(t), \ t_0 \leqslant t \leqslant t_1, \text{ is an optimal pair} \Longrightarrow \exists\, \xi(t) \in T^*_{x(t)}M, \ t_0 \leqslant t \leqslant t_1,$$

$$\frac{\mathrm{d}\xi(t)}{\mathrm{d}t} = \vec{H}_{u(t)}(\xi(t)), \quad \pi\xi(t) = x(t), \tag{18.1}$$

$$H_{u(t)}(\xi(t)) = \max_{u \in U} H_u(\xi(t)) = \text{const} \geqslant 0, \quad t_0 \leqslant t \leqslant t_1. \tag{18.2}$$

If the maximum condition

$$H_u(\xi) = \max_{v \in U} H_v(\xi), \quad \xi \in T^*M,$$

eliminates the parameter u from the family of Hamiltonians H_u, and as a result of this elimination, we obtain a *smooth* scalar-valued function (without parameters) H on T^*M, the *master-Hamiltonian* of the problem, then the whole optimal problem is reduced to studying trajectories of a fixed Hamiltonian vector field $\vec{H}$:

$$\frac{\mathrm{d}\xi(t)}{\mathrm{d}t} = \vec{H}(\xi(t)), \quad \pi\,\xi(t) = x(t); \qquad H(\xi(t)) = \text{const} \geqslant 0.$$

Regular problems of the calculus of variations are typical examples of this situation. Actually, this picture was envisaged by Pontryagin in his initial attempt to solve the problem.

It is remarkable that the *Pontryagin functor*, if we may call so the procedure prescribed by the maximum principle, permits us, practically in all interesting cases, including nonregular cases, to construct canonically a uniquely defined nonlinear connection on T^*M, which produces new important infinitesimal invariants of the optimal problem that are nontrivial already in the regular case. In particular, we can obtain the curvature tensor of the optimal problem, (see [9]).

If we try to derive from here global invariants of the state manifold M, for example, try to express its Euler characteristic through the curvature of the optimal problem (a possible generalization of the Gauss–Bonnet–Chern formula), we should inevitably come to generalizations of some classical relations concerning characteristic classes due to Pontryagin and Chern in case, where the usual Riemannian length on the manifold M is minimized. Thus, two major achievements of L. S. Pontryagin, based on completely different ideas and obtained in different periods of his activity, might be intimately related.

Bibliography

[1] V. G. Boltyanskii, R. V. Gamkrelidze, L. S. Pontryagin. On the theory of optimal processes *Dokl. Akad. Nauk SSSR*, 1956, **110**, 7–10 (Russian).

[2] R. V. Gamkrelidze. On the theory of optimal processes in linear systems *Dokl. Akad. Nauk SSSR*, 1957, **116**, 9–11 (Russian).

[3] R. V. Gamkrelidze. Time optimal processes in linear systems. *Izv. Akad. Nauk SSSR, Ser. Mat.*, 1958, **22**, 449–474 (Russian).

[4] V. G. Boltyanskii. The maximum principle in the theory of optimal processes. *Dokl. Akad. Nauk SSSR*, 1958, **119**, 1070–1073 (Russian).

[5] V. G. Boltyanskii, R. V. Gamkrelidze, L. S. Pontryagin. The theory of optimal processes. I. The maximum principle. *Izv. Akad. Nauk SSSR, Ser. Mat.*, 1960, **24**, 3–42 (Russian).

[6] L. S. Pontryagin. Optimal processes of regulation. In: *Proceedings of the International Congress of Mathematicians* (Edinburgh, 1958). Cambridge University Press, 1960, 182–202.

[7] R. V. Gamkrelidze. On sliding optimal regimes. *Dokl. Akad. Nauk SSSR*, 1962, **143**, 1243–1245 (Russian).

[8] R. V. Gamkrelidze. *Principles of Optimal Control Theory*, revised edition. New York: Plenum Press, 1978.

[9] A. A. Agrachev, R. V. Gamkrelidze. Feedback-invariant optimal control theory and differential geometry. I. Regular extremals. *J. Dynam. Control Systems*, 1997, **3**(3), 343–389.

Reprinting of the author's article in slightly modified form, which was first published on the occasion of the 90th birthday of L. S. Pontryagin in *Journal of Dynamical and Control Systems*, 1999, **5**(4), 437–451, Plenum Press. With kind permission of Springer Science and Business Media.

Yu. S. Il'yashenko

The Qualitative Theory of Differential Equations in the Plane

Translated by R. Cooke

Poincaré invented the qualitative theory as an approach to the study of differential equations not through formulas for their solutions — such formulas do not exist, as a rule — but directly through their right-hand sides. As a result, a new discipline arose on the border between geometry and analysis. Poincaré gave the qualitative study of the three-body problem as motivation for his work. However, the natural geometric questions turned out to be nontrivial even for equations in the plane. He began his investigation with them.

At present the geometric theory of differential equations consists of many branches. From it have arisen Hamiltonian mechanics, along with a new branch known as KAM theory; the multi-dimensional theory of dynamical systems, also called differential dynamics; bifurcation theory; holomorphic dynamics, which studies iterations of rational mappings of the Riemann sphere onto itself; equations on surfaces; the theory of relaxation oscillations; and the qualitative theory of differential equations in the plane, both real and complex.

These theories study principally similar questions:

— What is the local behavior of solutions (near a singular point)?

— What are the global properties of solutions (in the whole phase space and over an infinite time)?

— How do these properties get modified (bifurcate) in systems that depend on a parameter when the parameter varies?

These questions have been much better studied in the theory of differential equations in the real plane than in other divisions of the subject; some of them have been studied to nearly exhaustive completeness.

The main unsolved question remains the second part of Hilbert's 16th problem:

What can be said about the number and location of limit cycles of a polynomial vector field of degree n on the plane?

We shall describe the development of the qualitative theory from the point of view of its connections with this problem. In reality, only a small part of this development was motivated by the 16th problem. A significant part of the theory arose from applications. This holds in particular for the research of A. Andronov and his school [AVKh]. However, looking at the qualitative theory from a single point of view makes it possible to see a unified picture where the actual development took place over independent research areas.

We begin our survey with the theory of normal forms of vector fields near singular points; local studies, as usual, precede global. In agreement with the general idea of Poincaré, one is not to solve a differential equation, but to make changes of variables that bring the equation into a simple form. For elementary singularities the normalizing series (series that lead to an integrable normal form) converge. This part of the theory is discussed in the first section. To study more complicated singular points we apply the *method of resolution of singularities*. *Resonance saddles* and *saddle nodes* occupy an intermediate position: they cannot be simplified using resolution of singularities, but the normalizing series for them diverge. In the analytic classification of such singular points, *functional moduli* (which were discovered only recently) arise. All these areas combine in the research devoted to Hilbert's 16th problem. This research, in turn, motivated one of the central problems of the theory of nonlocal bifurcations — the Hilbert–Arnold problem. It also motivated the development of the theory of *foliations on analytic curves* of the complex projective plane, and the so-called *infinitesimal Hilbert 16th problem*, which lies on the boundary between differential equations and algebraic geometry.

The present survey is devoted to this research — its basic results and open problems.

Normal Forms

The local structure of a vector field in a neighborhood of a nonsingular point is always standard: by a smooth local change of coordinates the field becomes a constant. The study of the structure of a vector field near a singular point in multi-dimensional space is an inexhaustible problem. Only in the two-dimensional case has it been studied almost to completion.

Poincaré began the study of singular points with a natural question:

How do the phase portrait of a vector field and its linearization resemble each other at a singular point?

It is natural to begin with *nondegenerate* singular points — those for which zero is not an eigenvalue of the linearization. Poincaré divided the phase portraits of nondegenerate linear systems into saddles, nodes, foci, and centers. Despite its simplicity, or perhaps precisely because of it, these portraits became the symbol of the new theory; they are even displayed on the cover of the book *Poincaré* in the series *Lives of Remarkable People*. [1]

The preceding question can now be stated as follows:

Does there exist a change of coordinates that transforms the original vector field into its linear part at a singular point?

Depending on the class of the change of variable being sought — homeomorphism, diffeomorphism, or analytic diffeomorphism — three branches of the theory arise: topological, smooth, and analytic. Here and below, unless the contrary is stated, smoothness means infinite smoothness.

The first theory developed was the analytic. Poincaré proved that in the absence of so-called *resonances* (integer linear relations between the eigenvalues) a focus and a node are analytically equivalent to their linear parts [P].

The attainment of an analogous result for a nonresonance saddle (for which the ratio of the eigenvalues of the linearization is irrational) was hampered by the so-called *small denominators*. The difficulty associated with this was overcome only 60 years later by Siegel [Si]. He proved that saddles with *Diophantine* eigenvalues (whose ratios are badly approximated by rational numbers) are analytically equivalent to their linear parts. Siegel's theorem was strengthened by Bryuno [B71], who exhibited two conditions very close to each other, one of which is necessary and the other, slightly stronger, is also sufficient for a saddle to be equivalent to its linear part. As above, these conditions are imposed on the ratio of the eigenvalues of the linear part.

[1] A. A. Tyapkin, A. S. Shibanov. *Poincaré*, 2nd edition. Moscow: Molodaya Gvardiya, 1982 (Russian). — *Eds.*

Yoccoz [Y] proved the necessity of Bryuno's sufficient conditions and thereby closed the problem of analytic linearization of a saddle. We remark that nonlinearizable analytic saddles are sometimes called *wild*.

The papers of Siegel, Bryuno, and Yoccoz constitute one of the high points in the qualitative theory of differential equations. The Fields Medal was awarded to Yoccoz in 1994 essentially for this work.

Commenting on Bryuno's paper, Arnold (1969) assumed that for singular points whose linear part is pathologically close to a countable number of resonances, there is a topological reason for the divergence of the normalizing series. However, the difficulties are hidden from a researcher looking at the real picture, and become visible only when one comes out onto the complex domain. Under a small perturbation of a resonance saddle from the coordinate cross in the complex plane, a complex phase curve (Riemann surface) separates off that is homeomorphic to a cylinder. Such phase curves do not exist for nonresonance linear saddles. Arnold called the generation of this cylinder the *materialization of resonance*. For a saddle that is pathologically near to a countable number of resonances, the cylinders, generated when they are materialized, accumulate at the singular point and prevent the normalizing series from converging.

Sufficient conditions for divergence of the normalizing series evoked by materialization of resonances have been studied by Pyartli [Pya72], [Pya78]. They turned out to be significantly stronger than the Bryuno–Yoccoz conditions; that is, they required a more rapid convergence of the rational approximations to the ratio of the eigenvalues. Subsequently, R. Perez-Marco [P-M] proved that there exists a gap between the conditions for divergence of the normalizing series and the condition for materialization of resonance. Thus, there remain nonresonance saddles that are analytically nonnequivalent to their linear parts, for which this nonequivalence is not explained by geometric reasons.

Let us return to Poincaré's research on "Curves defined by differential equations" (see [P1]). After discussing nodes and foci, Poincaré turned to centers. A center is an *atypical* singularity. Its linearization is determined by a condition involving *equality* of eigenvalues: their real parts must be equal to zero (but not their imaginary parts). Poincaré considered the question:

Can one determine from the linear parts whether all phase curves of a planar vector field having a center are closed?

He exhibited an algorithm that reduced to arithmetic operations on the Taylor coefficients of the field being studied. In the case when the algorithm terminates, the answer to the preceding question is negative: the singularity is a focus. If the algorithm does not terminate, the answer is positive — it is a center. For polynomial vector fields of degree n there exists a point $N(n)$ beyond which the Poincaré algorithm cannot terminate. This follows from the Hilbert basis theorem. Thus, the necessary and sufficient condition for the presence of a center at the point 0 in a polynomial vector field is a finite set of polynomial equations for the coefficients of the polynomials. For *quadratic* vector fields (polynomials of degree 2) these conditions were obtained by Dulac [D08]. For vector fields of degree 3 conditions for a center have still not been obtained, due primarily to the cumbersome nature of the computations involved.

Quadratic vector fields play a special role: they are the simplest (though still very complicated) class of polynomial vector fields which may possibly be studied completely (that is, down to a complete listing of all possible phase portraits). We shall return to this point more than once.

Let us consider a vector field with a nondegenerate singular point of center or focus type. By a linear change of variable, this field can be brought into the form

$$\dot{x} = y + \alpha x + p, \qquad \dot{y} = -x + \alpha y + q, \tag{1}$$

where p and q are nonzero polynomials of degree 2. By means of dilations one can arrange that the set λ of six coefficients of the polynomials p and q belong to the unit sphere. Dulac exhibited polynomials V_1, V_2, V_3 in λ such that Eq. (1) has a center if and only if

$$\alpha = 0, \qquad V_1 = V_2 = V_3 = 0. \tag{2}$$

The precise form of the polynomials V_k is not important for our present purposes.

Resolution of Singularities

The study of complicated singularities of planar vector fields reduces (partly) to the study of *elementary* singularities using the procedure of *resolution of singularities*. (A singularity is *elementary* if at least one of the eigenvalues of its linearization is nonzero.)

By means of a homeomorphic change of coordinates and, if necessary, time reversal, an elementary singular point can be reduced to a normal form from the following list: a saddle, a node, a focus, a center, or a *standard saddle-node*: $\dot{x} = x^2$, $\dot{y} = -y$. This topological description of the singular points goes back to Bendixson [B]. A lucid exposition can be found in [I85].

The simplest description of one step in the resolution of singularities (one *inflation*) can be given using plane polar coordinates (r, φ).

Consider an analytic vector field v with an isolated singular point. Let us move this point to the origin and map a deleted neighborhood of it onto an annulus:

$$(r,\varphi)\mapsto(r+1,\varphi),\ \ \{(r,\varphi)\,|\,r\in(0,\varepsilon),\,\varphi\in S^1\}\to\{(r,\varphi)\,|\,r\in(1,1+\varepsilon),\,\varphi\in S^1\}.$$

The original vector field transforms to a new field $\tilde{v}$, which can be analytically continued across the *glued-on circle* $r=1$ to the annulus $r\in(1-\varepsilon,1+\varepsilon)$, $\varphi\in S^1$. In general, $\tilde{v}=0$ at all points of the pasted circle. Dividing the field $\tilde{v}$ by a suitable power of the difference $r-1$, we obtain a new vector field having only a finite number of singular points on S^1. It is this field that is the result of inflating the field v at 0. If the resulting singular points remain nonelementary, the process can be repeated.

Theorem 1 (Resolution of singularities)**.** *Suppose the complexification of a real-analytic vector field has an isolated singularity. Then by a finite number of inflations this singular point can be decomposed into a finite number of elementary singular points.*

The hypothesis of the theorem is not burdensome: if it does not hold, the vector field can be divided by a real function such that the quotient will satisfy the hypothesis of the theorem. The composition of inflations mentioned in the theorem is called a *good inflation*.

The proof of this theorem has a long history. Bendixson [B] stated the theorem, but did not propose any proof. Complete proofs for the analytic case were given by Seidenberg [Se] and Lefschetz [Lef]; the generalization to the smooth case was obtained by Dumortier [Dum]. The first transparent proof was given by van den Essen and expounded in [MM] and [Kl]. The theorem has not yet appeared in any textbook.

A simple proof of the theorem on resolution of singularities can be obtained once an *integer* invariant is associated with the singular point, that is, an integer that decreases with each step in the resolution of singularities. This invariant will also majorize the number of steps within which the good inflation is obtained. The *multiplicity of the singular point* — the maximal number of singular points into which the original point may decay under a small perturbation, if one counts the singular points thereby generated not only in the real plane but also in the complex plane — turned out to be such an invariant. The multiplicity decreases when a singular point with a *zero* linear part is inflated. The case of a nonzero linear part can be studied by direct computation: the multiplicity of a nonelementary singular point of such a type decreases after at most three inflations. Van den Essen's proof is constructed following this route.

In the late 1960s, Arnold and Thom formulated the concept of *algebraically solvable local problems of analysis*. A local problem is *algebraically solvable* if the answer to the question can always be obtained in a finite number of arithmetic operations on the Taylor coefficients of the original data, except for the degenerate cases of codimension infinity.

The simplest examples of local problems are the following: *Does a given function have a maximum at zero*? *Is the singular point* 0 *of a vector field Lyapunov stable*? *Does a vector field have a phase curve that enters a singular point along a given direction*? *And so forth.*

The theorem on resolution of singularities sometimes makes it possible to do a complete study of the topology of the phase portrait near a complicated singular point of a planar vector field [Dum], [An].

More specifically, a singular point of a planar vector field is *characteristic* if it has a phase curve that enters it along a certain direction in direct or reversed time. A singular point is *monodromic* if the orbits revolve around it and the Poincaré mapping is defined in some half-interval with vertex at the point.

An elementary theorem [AI, 5.3.1] asserts that *every nonplanar germ of a smooth vector field at a singular point in the plane is either characteristic or monodromic*.

It follows from [Dum] that the following problems are algebraically solvable:

— *distinguish monodromic and characteristic singular points*;

— *describe the topology of the phase portrait of a vector field near a characteristic singular point*.

Dumortier has proved that *there exists a topologically sufficient jet* of a vector field at a characteristic point, except for cases of infinite codimension [Dum]. We recall that the n-jet of a vector field at a point is the class of vector fields (*representatives of the jet*) that coincide up to a term that decreases faster than the nth power of the distance from the point. The number n is called the *order* of the jet. A jet is *topologically sufficient* if all its representatives are orbitally topologically equivalent near the singular point.

The order of a topologically sufficient jet at a characteristic singular point is at most two larger than the double of the multiplicity of this point [Kl].

In regard to monodromic singular points, we note that a center is a degeneracy of infinite codimension. Moreover, except for cases of infinite codimension, a monodromic singular point has a very simple phase portrait: it is either a stable focus or an unstable focus. But the problem of stability for monodromic singular points is *algebraically unsolvable* [I72]. Nevertheless, there is hope that the problem of stability of singular points of planar vector fields is *analytically solvable*. (Analytic solvability is defined like algebraic solvability, except that the phrase "arithmetic operations" in the definition needs to be replaced by "analytic functions.") An approach to the study of this problem based on *Dulac's theorem* stated below can be found in [MMa].

The proof of analytic solvability of the problem of stability of monodromic singular points in the plane *completely closes the local theory of differential equations in the plane* (in a certain sense).

The *global topological study* of phase portraits of vector fields in the plane was performed by Andronov and his students [ALGM66]. This study, and especially the concept of *structural stability* introduced by Andronov and Pontryagin [AP] formed the point of departure for the rapid development of the multi-dimensional theory of dynamical systems that occurred in the 1960s.

Elementary Singularities. The Écalle–Voronin and Martinet–Ramis Moduli

The theorem on resolution of singularities shows that elementary singular points deserve detailed investigation. The topological classification of these points is simple and was described above. The smooth orbital classification is also simple. Except for the cases of infinite codimension, elementary singular points can be brought into a polynomial normal form that is integrable in terms of elementary functions by a smooth change of coordinates and multiplication by a nonzero function. This result summarizes the research of many authors: Sternberg, Chen, Bryuno, Takens, and others. The main contribution was made by Bogdanov, who was the first to write a complete list of normal forms, and who discovered that they are integrable [Bo]; complete proofs and references can be found in [I85].

The finite-smoothness classification of deformations of elementary singularities was given in [IYa]. The normal forms of the corresponding local families also turned out to be integrable; they are used in the theory of nonlocal bifurcations, in particular in the studies described below of the Hilbert–Arnold problem. This classification is compounded from the papers of Takens, Belitskii, Samovol, Kostov, Roussarie, and also Il'yashenko and Yakovenko [IYa], where the necessary references are given.

The analytic classification of elementary singular points turned out to be surprising; it has functional moduli and is connected with the nonlinear Stokes phenomenon discovered by Voronin [Vo] and Malgrange [Ma]. This phenomenon arises in the problem of analytic classification of the germs of conformal mappings $(\mathbb{C},0) \to (\mathbb{C},0)$ with the identity as linear part:

$$f\colon z \mapsto z + \alpha z^{k+1} + \cdots, \qquad \alpha \neq 0. \tag{3}$$

Such germs are called *parabolic*. The problem is the following: When are two parabolic germs analytically conjugate, that is, when does there exist a germ of a biholomorphic mapping $h\colon (\mathbb{C},0) \to (\mathbb{C},0)$ such that $h \circ f = g \circ h$?

The germ (3) is formally equivalent to a shift in unit time along trajectories of the equation

$$\dot{z} = z^{k+1} + \beta z^{2k+1},$$

where the complex number β is an invariant of the formal classification. It turns out that the formal series that conjugate the germs f and g diverge, as a rule. However, there is a geometric object associated with them — the so-called *Fatou coordinate* or *normalizing coordinate*. This coordinate is defined not in an entire neighborhood of the fixed point, but in some *sector with vertex at that point* and, in general, cannot be extended to a sector of opening larger than $2\pi/k$. The sectors corresponding to *different* Fatou coordinates form a covering of the deleted neighborhood of zero. The existence of such coordinates is the subject of the *sectorial normalization theorem*.

The normalizing charts defined in these neighborhoods, taken together, form a normalizing atlas. Particular charts of this atlas were known as early as the nineteenth century [Le]; however, the transition functions for it were studied only comparatively recently. *These functions determine a complete set of invariants of the analytic classification of parabolic germs* called *Écalle–Voronin* moduli. These moduli range over an infinite-dimensional functional space [Vo], [Ma].

In the general case, the resonance germs of mappings on the complex line (the multiplier is a root of unity) and also the resonance singularities — centers, saddle nodes, and resonance saddles — generate a *normalizing atlas* in a deleted neighborhood of the equilibrium position. The transition functions of this atlas also range over a rich functional space. It is this effect that is called the *nonlinear Stokes phenomenon*. A detailed discussion of it can be found in [I93a].

The functional moduli of the analytic classification of complex saddle nodes and resonance saddles were discovered by J. Martinet and J.-P. Ramis [MR82], [MR83]. We note that in the complex plane a center in the linear terms is a particular case of a resonance saddle. Thus, the analytic classification of all elementary singular points, except for "wild" saddles, is complete.

The epithet in the name *elementary* singular points now assumes a double sense. On the one hand, all arbitrarily complicated singular points are constructed out of these points, just as molecules are made up of atoms. This follows from the theorem on resolution of singularities. On the other hand, the elementary singular points are indeed "elementary": the smooth and analytic classifications of them have been studied in detail.

Significantly earlier Bryuno ([B71], [B72]) discovered necessary and sufficient conditions for formal equivalence of the germs of resonance vector fields at a singular point to imply analytic equivalence. For resonance saddles, Bryuno's condition requires the absense of nonlinear terms in the formal orbital normal form. For saddle nodes this condition implies that the singular point be of infinite multiplicity: The formal orbital normal form must be $\dot{x} = 0$, $\dot{y} = -y$. Thus, except for the cases of infinite codimension described above, the analytic classification of resonance saddles and saddle nodes is immeasurably richer than the formal classification; and it is the Martinet–Ramis functional moduli that give this classification.

The sectorial normalization theorem for parabolic germs describes a new class of local objects of one-dimensional complex analysis, the so-called *functional cochains*. Roughly speaking, a functional cochain is a set consisting of a finite number of holomorphic functions defined in sectors that form a covering of a deleted neighborhood of zero; in the intersection of adjacent sectors the difference of the two functions decreases *exponentially*. All functions of the set have a common asymptotic Taylor series. The exact definition connects the angle of opening of the sectors with the rate of exponential decrease.

Functional cochains should be regarded not as a "disconnected" set consisting of a finite number of holomorphic functions, but as a unified object. Like a holomorphic germ, such a cochain is uniquely determined by a specific (asymptotic) Taylor series [IKh]. Functional cochains can be added and multiplied, and under certain restrictions one can consider their composition. There arises a *functional cochain calculus* of which essential use is made in the nonlocal investigations described below. Functional cochains as the sums of divergent series arose earlier in the work of Ramis and Sibuya [RS].

The analytic classification of parabolic germs was obtained independently by Écalle using a completely new analytic approach. He developed the theory of *resurgent functions*, which was then applied to the proof that the number of limit cycles is finite [E81a], [E81b], [E85], [E92].

The Hundred-Year History of Hilbert's Sixteenth Problem

As is characteristic of the Hilbert problems, the study of the question posed in the second part of the 16th problem required results from many divisions of mathematics: complex analysis, algebraic geometry, and topology. The problem, in turn, generated new areas and problems: the theory of foliations on analytic curves, the problem of zeros of Abelian integrals, the problem of cyclicity of separatrix polygons, and many others. This research and these problems will be discussed below.

Hilbert's 16th problem can be divided into three problems; a positive solution of each would imply a solution of its predecessor.

Problem 1 (The finiteness problem)**.** *Does a polynomial vector field in the plane have only a finite number of limit cycles*?

Problem 2. *Is the number of limit cycles of a polynomial vector field bounded above by a constant depending only on the degree of the polynomials*?

The upper bound in Problem 2 is usually called the *Hilbert number* and denoted $H(n)$ (where n is the degree of the polynomial). Linear vector fields do not have limit cycles, so that $H(1) = 0$. It is not yet known whether there exists a number $H(2)$.

Problem 3. *Give an explicit upper bound for* $H(n)$.

Only the first of these three problems has been solved (positively) by Il'yashenko [I91] and Écalle [E92]. Problems 1 and 2 have analytic analogs:

Problem 4. *Does an analytic vector field on the two-dimensional sphere have only a finite number of limit cycles*?

An *analytic family* of vector fields is a finite-parameter family of analytic vector fields that depends analytically on the parameters.

Problem 5. *Is it true that for every analytic family of vector fields on the two-dimensional sphere with a compact base* (*parameter space*) *the number of limit cycles of the equations of the family is uniformly bounded*?

Problem 4 was solved by the same authors that solved Problem 1. Actually, both proofs are for the case of analytic vector fields; the polynomial case is obtained as a corollary of the analytic case.

A positive answer in Problem 5 would imply an analogous answer in Problems 1, 2, and 4. This is obvious for Problem 4 and can be proved using the Poincaré compactification for Problems 1 and 2. Problems 3 and 5 are independent.

The question whether there exists a uniform estimate of the number of limit cycles is closely connected with bifurcation theory. Indeed, the function "number of limit cycles," which is defined on the space of coefficients of polynomial vector fields, has a discontinuity at the points corresponding to structurally unstable equations. Limit cycles may arise when these equations bifurcate. They are generated out of *polycycles* — the separatrix polygons, defined as the connected union of a finite number of singular points and the phase curves joining them. The *cyclicity of a polycycle in a family of equations* is defined as the maximal number of limit cycles generated from a polycycle in the family.

Problem 6. *Is it true that a polycycle arising in an analytic family of vector fields on the plane always has finite cyclicity in that family*?

A positive answer to this question would imply a positive solution of Problem 5.

The second part of Hilbert's 16th problem has a dramatic history, which is far from over. In 1923 Dulac published a positive solution of the finiteness problem; more precisely, of Problems 1 and 4 [D23]. This paper was not understood for nearly 60 years. In 1980, a Russian translation of it was published as a separate book, in the preface of which it was stated that it was the best paper on the qualitative theory of differential equations during the preceding 50 years.

In 1955 and 1957 Petrovskii and Landis published a complete solution of Problem 3: $H(2) = 3$, $H(n) \leqslant P_3(n)$ (a polynomial of degree 3 in n) [PL55], [PL57]. This paper was based on passing to the complex domain — the method by which Petrovskii had already obtained a number of excellent results. Among them was a significant advance in the *first* part of Hilbert's 16th problem and the answer to one of the questions posed in it.

The strategy proposed by Petrovskii consisted of the following. All typical polynomial differential equations in the complex plane have the same structure in a certain sense. Atypical equations correspond to a set of *complex* codimension one in the parameter space. Therefore the set of typical equations is arcwise connected. Consequently, to estimate the number of limit cycles of an arbitrary polynomial vector field of degree n, it suffices to perform three steps:

— determine an invariant of the complex polynomial differential equations of degree n that majorizes the number of limit cycles in real equations of this class (Petrovskii and Landis called it the *genre*);

— prove that homotopies preserve genre;

— study the genre of equations that are nearly integrable.

Afterward, the number of limit cycles can be estimated as follows. A typical equation is joined by a path consisting of typical equations with an equation that is nearly integrable. The genre of the equation at the end of the path is estimated at Step 3. Its preservation along the path is proved at Step 2. Consequently, the genre is also estimated for the initial point of the path. Step 1 gives an estimate of the number of limit cycles at the beginning of the path. Typical equations are dense; therefore a uniform estimate of the number of limit cycles for typical equations implies an analogous estimate for all equations.

* * *

In 1960 Kronrod, Landis, and Gerver organized a seminar "for the young." The participants were first-year students (I recall Anatolii Katok and Aleksandr Chetaev) and high-school students (Sergei Gel'fand, Osip Bernshtein, Dima Kazhdan, and Grigorii Margulis). The seminar was structured according to a system of instruction described by Landis when he recalled his first steps: "They give you an exercise — you solve it — then they give you a problem."

The seminar began with problems involving permutations of a countable set that may change the sum of a conditionally convergent series. Depending on the effectiveness of such permutations, Kronrod called them *powerful* or *harmful*. I later learned that these very permutations had formed the content of his first student paper, written at the suggestion of A. O. Gel'fond. (The terminology in the paper was not so vivid.) Thus Kronrod was attempting to rear his students on the same problems that he himself grew up on. The seminar did the participants a lot of good, but they did not solve the problems in the theory of functions of a real variable and they went into other areas. But the seminar had a quite surprising continuation.

The next year it was renewed as a seminar on differential equations. Its ultimate aim was to extend the ideas of Petrovskii–Landis from the two-dimensional case to the multi-dimensional. Along the way it would involve reconstructing the Petrovskii–Landis paper "in exercises." A year later only three persons remained in the seminar: Landis, Gerver, and I. In the spring of 1963, while trying to solve the problems that make up Step 2 (the theorem of conservation of genre), I encountered difficulties that made me doubt the effectiveness of the proposed methods. I asked Landis some questions which he thought about for a long time and couldn't answer. In a manuscript that was proposed to be published as a book containing the solution of the 16th problem, Landis included a new proof of the theorem on conservation of genre, in which I also found a gap.

In the autumn of 1963, S. P. Novikov gave a series of talks devoted to the Petrovskii–Landis paper in Gel'fand's seminar. Just before the third talk, in which he was to discuss the conservation of genre, a student came up to Novikov, whom Novikov may have known by sight, but whose name he clearly did not know. "Serezha," said the student, "there is a mistake in the theorem that you are about to discuss." "What is it?" It took Novikov a minute to understand what the mistake was. After that, the seminar began as follows.

"Well, now," said Novikov, pacing back and forth in front of the blackboard, "we've looked over the theorem and seen a mistake in the paper." "Who is this *we*?" asked Gel'fand. "Us," said Novikov, making a wide gesture with his arm at the large room full of people. "Who is *we*?" repeated Gel'fand. Same answer, same gesture. "Who is *we*?" asked Gel'fand, beginning to get annoyed, and finally turning to the audience. Then the student (that the author of these lines was then — some fourty years ago), shyly got up and stood there. The talk was canceled.

After that I spent two years looking for ways to repair the gap and refuted every one of them myself. I discussed some of the routes with Novikov on the way to the metro after Gel'fand's seminar. Novikov met with Landis and refuted his new attempts to prove conservation of genre. In the autumn of 1965 Landis said to me, "You and I are stuck at the starting gate." And in 1967 there appeared a letter in *Matematicheskii Sbornik* from Petrovskii and Landis in which they retracted their proof.

Although the paper of Petrovskii and Landis did not achieve the desired goal, it had a great influence on the subsequent development of the theory of differential equations in the complex domain. It laid out the fundamental concepts of the theory — complex limit cycle, complex Poincaré mapping — and described their basic properties. This part was further developed in the theory of foliations on analytic curves, which is discussed below.

The third part led to the problem of the zeros of Abelian integrals. The integrable equation studied by Petrovskii and Landis at Step 3 had a rational first integral with rational level curves — Riemann spheres with a finite number of punctures. The appearance of limit cycles under perturbations was due to the zeros of the integrals of rational 1-forms over uncontractible loops on these level curves. These integrals were calculated using the residues at the punctures. However, by 1965 it became clear that to obtain a realistic picture one would have to study so-called Abelian integrals — integrals of polynomial 1-forms over cycles on algebraic curves of arbitrarily high genus. This realization was featured in the papers [I69a] and [I69b], in which new results on the number and location of limit cycles were obtained as a corollary of the Riemann–Roch theorem and the Picard–Lefschetz theorem. After that, the connection of the problem of perturbation of integrable equations with algebraic geometry and topology was used constantly.

In 1981, at the suggestion of Arnold, I wrote a commentary on the papers of Petrovskii and Landis for a posthumous two-volume edition of the works of Petrovskii. At Arnold's insistence, I included in this survey a modern exposition of the famous memoir of Dulac. With the use of the modern theory of normal forms and the theorem on resolution of singularities, this exposition appeared very simple. I enthusiastically told this proof to one of my students (A. A. Shcherbakov). Then next day I woke up in horror: the proof I had talked about the day before was good for infinitely smooth fields, for which the theorem on finiteness of the number of limit cycles was demonstrably false! After that, I easily found the mistake in my own proof and in the more difficult ones of Dulac. The basic correct result of Dulac's memoir (the so-called *Dulac's theorem*) consisted of a description of the *asymptotic series* for the Poincaré mapping corresponding to a polycycle (separatrix polygon) of an analytic vector field. This theorem had been waiting to be recognized for a long time, and a transparent proof of it was published only 60 years after Dulac's work [I82], [I85]. The proof of this theorem occupied practically the entire volume of Dulac's sizeable memoir. The finiteness theorem was derived from Dulac's theorem on a single page. And there was the mistake: Dulac had treated asymptotic series as if they were convergent.

Thus in 1981 somewhat less was known about Hilbert's 16th problem than had been known 81 years earlier, when it was posed. In the same year I gave a talk at a session of the Moscow Mathematical Society devoted to two different topics: Dulac's memoir and Écalle–Voronin moduli. To motivate the combination of such different themes in a single talk, I attempted to think up a connecting phrase on the fly. "We have seen what the real theory of normal forms can contribute to the study of singularities; let us now see what the complex theory can contribute." Even before I finished speaking, I realized that this was not merely an elegant connection between the two parts — it was a research program.

Later J.-P. Ramis told me that when he and Martinet undertook the study of moduli of the analytic classification of complex saddle nodes (which are now called Martinet–Ramis moduli), they understood clearly that they were creating the machinery for storming the finiteness problem.

The finiteness theorem was proved by Écalle [E92] and the author [I91] using completely different methods. As indicated above, Écalle used the theory of resurgent functions. The proof given in [I91] is based on the computation of functional cochains and *superexact asymptotic series*. These series make it possible to take account simultaneously of the power terms and the exponentially small terms of the asymptotics, paradoxical though that sounds.

* * *

The second part of Hilbert's 16th problem remains one of the most inaccessible in the famous Hilbert list, second only to the Riemann Hypothesis on the zeros of the ζ-function. Smale included both problems in his list of "problems for the next century" [S].

In regard to the 16th problem, he noted that it would be natural first to solve a simplified version of it, replacing the class of all polynomial vector fields by a more comprehensible family, for example Liénard equations with a polynomial of odd degree n. In that form the problem is also not solvable. Other "simplified versions" — problems involving the number of limit cycles of Abel's equation, or of quadratic (polynomial of degree 2) vector fields — also remain unsolved. However, if additional restrictions are imposed on the equations (for example, bounding the coefficients in Liénard's equation), the number of limit cycles can be estimated. The estimate is expressed not only in terms of the degree of the polynomials on the right-hand side, but also in terms of a constant that determines the additional restrictions.

Theorem 2 [I00]. *Consider Abel's equation*

$$\frac{\mathrm{d}y}{\mathrm{d}x} = \mathrm{e}^n + \sum_{j=0}^{n-1} a_j(x) y^j, \quad y \in \mathbb{R}^1, \quad x \in S^1, \tag{4}$$

with continuous coefficients a_j. Let

$$|a_j| < C. \tag{5}$$

Then the number A of limit cycles of Eqs. (4) *and* (5) *is bounded above as follows*:

$$A < \mathrm{e}^{\mathrm{e}^{C^{3n}}}.$$

Theorem 3 [IP]. *Consider the Liénard equation*

$$\dot{x} = y - F_n(x), \quad \dot{y} = -x, \quad F_n(x) = x^n + \sum_{j=1}^{n-1} a_j x^j, \quad n \text{ odd}. \tag{6}$$

Suppose

$$|a_j| < C, \quad C \geqslant 4. \tag{7}$$

Then the number L of limit cycles of Eqs. (6) *and* (7) *is bounded above as follows*:

$$L \leqslant \mathrm{e}^{\mathrm{e}^{C^{14n}}}.$$

Consider the set of quadratic vector fields (1). Suppose the scale in the plane $\mathbb{R}^2$ is chosen so that $|\lambda| = 1$. (We recall that λ is the set of coefficients in the quadratic terms of Eq. (1).) Denote by $\Delta(\delta)$ the set of fields (1) that are δ-far away from the set of centers in the following sense:

$$|\alpha| + \sum_{k=1}^{3} |V_k(\lambda)| \geqslant \delta,$$

where V_k are the polynomials (2). Denote by $H(2,\delta)$ the least upper bound of the number of limit cycles of equations of class $\Delta(\delta)$ that are at least δ-far away from all singular points of the equation lying in the disk $x^2 + y^2 \leqslant \delta^{-1}$.

Theorem 4 [IL1*].

$$H(2,\delta) \leqslant \mathrm{e}^{\mathrm{e}^{100\delta^{-1}}}.$$

This theorem shows that the main difficulties in the Hilbert problem for quadratic vector fields are connected with the limit cycles that pass close to the singular points, either finite or infinitely distant.

Local and Nonlocal Bifurcations in the Plane. The Hilbert–Arnold problem

The theory of local and nonlocal bifurcation was created by Andronov and his students in the 1930s. They studied the bifurcation of the generation of a cycle when the singular point loses stability (obstinately called the Hopf bifurcation in the West), and the generation of a cycle from a loop of a separatrix and from a homoclinic curve of a saddle node. The first of these bifurcations takes place in the neighborhood of a singular point and is *local*. The other two occur in a neighborhood of a *polycycle* (the separatrix polygon consisting of the singular points and the phase curves joining them) and are *nonlocal*. The polycycle may also consist of one singular point or a closed phase curve. The list given above is an exhaustive list of the most interesting bifurcations of polycycles occurring in typical one-parameter families of planar vector fields. This research was summarized in the monograph [ALGM67].

Over the next thirty years, bifurcations in families with two or more parameters were hardly studied at all. We note only a remarkable result of E. A. Leontovich:

The cyclicity of a separatrix loop of a hyperbolic saddle arising in a typical finite-parameter family does not exceed the number of parameters of the family.

A sketch of the proof of this theorem is given in [L]; the complete proof can be found in [R].

The systematic development of the multi-parameter theory of local bifurcations was begun by Arnold in 1972. It is based on the concept of general position in the Thom transversality theorem. Arnold revamped the very vocabulary of bifurcation theory. He brought into use the terms *versal families* (a sort of topological normal form for deformations of the germ of a vector field with fixed type of degeneracy) and *bifurcation diagrams* (sets in the parameter space corresponding to structurally unstable equations of a versal family).

The first of the nontrivial two-parameter local families — a perturbation of a singular point with zero nilpotent linear part — was studied by Bogdanov in his student paper carried out under Arnold's direction. This paper became a sort of standard for subsequent research. Three effects were revealed in it that are typical of almost all problems of the multi-parametric local theory of bifurcations:

— renormalization (change of scale in the space of variables and parameters accompanied by a change of time) reduces the problem of bifurcation theory to the study of a perturbation of an integrable system;

— the basic difficulty is the study of the limit cycles generated under this perturbation; it reduces to estimating the number of zeros of a special integral over trajectories of an integrable system;

— *local* bifurcations in typical k-parameter families are accompanied by *nonlocal* bifurcations occurring in $(k-1)$-parameter families.

Bifurcations in two-parameter families of vector fields in a phase space of dimension larger than 2 sometimes reduce to bifurcations in special families in the plane. Using some procedure of *averaging* and *factorization*, this reduction can be carried out for the following classes:

— bifurcations of singular points with two pairs of purely imaginary eigenvalues;

— bifurcations of limit cycles with multipliers $(1,1)$; $(-1,-1)$; $(\mathrm{e}^{\pm 2\pi \mathrm{i}/3})$; $(\mathrm{e}^{\pm \pi \mathrm{i}/2})$ (the cases just enumerated are called *strong resonances*).

The corresponding families in the plane were studied by Arnold [A77] and his students — Khorozov [Khor] and Żołądek [Z83], [Z87], and also in later papers of Berezovskaya, Khibnik, Rousseau, Krauskopf, and others; references can be found in the survey [AAISh], and also in the book [CLW], which summarizes the preceding 20-year period in the development of the local theory of bifurcation.

Local bifurcations in three-parameter families of vector fields on the plane are studied almost completely in [DRS] and [DRSZ]. An equally detailed study of four-parameter families seems hopeless. The local theory of bifurcation of planar vector fields seems to be finished to a large degree.

In 1986, Arnold outlined a program for the development of a *theory of non-local bifurcations on the plane in families of more than one parameter* [AAISh, § 3.2]. The first part of the program included the study of typical families with a small number of parameters $k \leqslant 3$. A complete list of polycycles arising in such families was compiled by A. Yu. Kotova [KS] — the so-called *Kotova zoo*. The bifurcations of these polycycles have been studied by many authors: Dumortier, Mourtada, Roussarie, Rousseau, Stanzo, Trifonov, and others. However, these studies are far from complete.

The second part involved multi-parameter families and consisted of a number of questions. We shall exhibit here just one of them.

The finiteness problem for bifurcation diagrams. *Is it true that for every k there exists only a finite number of topologically distinct germs of bifurcation diagrams corresponding to typical k-parameter families of planar vector fields*?

An affirmative answer would immediately imply a positive solution of the following problem:

The Hilbert–Arnold problem [I93b]**.** *Is it true that for every k the cyclicity of the polycycle that arises in a typical k-parameter family does not exceed some constant depending only on k* (*it is denoted* $B(k)$)?

Indeed, polycycles of different cyclicity correspond to topologically different germs of bifurcation diagrams. If the cyclicity were unbounded, the number of distinct germs would be infinite, which is a contradiction.

However, the answer to the finiteness problem for bifurcation diagrams turned out to be negative. To be specific, a polycycle was discovered in the Kotova zoo that belongs to a continuous family of polycycles — the so-called "lip" ensemble. The polycycle consists of two saddle nodes with a common separatrix of two hyperbolic sectors (this part is common to all polycycles of the ensemble) and the phase curve going from a parabolic sector of one saddle node to a parabolic sector of the other. This phase curve belongs to a continuous family of trajectories, which also go from one saddle node to another. Together these "parabolic ties" resemble lips; hence the name. For every L a field with the "lip" ensemble can be constructed so that when it bifurcates in a typical three-parameter family, more than L cycles are generated. Moreover, more than

L polycycles of the family generate at least one limit cycle apiece. Each polycycle of the ensemble has cyclicity no larger than 3; therefore this example does not give a negative answer to the Hilbert–Arnold problem.

The Hilbert–Arnold problem is a natural analog of Problem 6 of the preceding section and is now the central problem of the multi-parametric theory of bifurcations on the plane. In the study of this problem the following results have been obtained. We call a polycycle *elementary* if all its singular points are elementary. We denote by $E(k)$ the maximal cyclicity of an elementary polycycle that can occur in a typical k-parameter family of smooth vector fields on the plane.

Theorem [IYa95]**.** *For every k, the quantity $E(k)$ exists.*

Theorem [K]**.** *For every k, the inequality $E(k) \leqslant 2^{25k^2}$ holds.*

The proof of both theorems uses the normal forms of local families [IYa] and the theory of Khovanskii fewnomials [Kh91]. The proof of the last theorem also uses the theory of stratifications perfected by Kaloshin.

Another application of the theory of nonlocal bifurcations is the attempt undertaken by Dumortier, Roussarie, and Rousseau to prove the existence of the Hilbert number $H(2)$ by solving the problem of finite cyclicity for quadratic vector fields. In [DRR] there is a list of 121 polycycles that can arise in a family of quadratic vector fields. At present, the finite cyclicity of 82 of these polycycles has been proved (see [I02], Section 5.2, and the literature there). There is reason to hope that the existence of $H(2)$ will be proved following this route. However, an analogous proof of existence for $H(3)$ appears completely unrealistic.

In conclusion, we note a famous result of Bautin from the local theory of bifurcations of quadratic vector fields.

Theorem [Ba]**.** *The cyclicity of a center singularity in the space of quadratic planar vector fields is at most* 3.

The circumstance that the number of conditions for a center given in (2) exceeds its cyclicity by 1, does not seem to be coincidental.

Problem. *Is it true that the cyclicity of a center singularity in a family of polynomial vector fields of fixed degree is* 1 *less than the number of independent conditions for a center in that family*?

Foliations on Analytic Curves

Hilbert's 16th problem involves the equations

$$\frac{\mathrm{d}y}{\mathrm{d}x} = \frac{P_n(x,y)}{Q_n(x,y)}, \quad (x,y) \in \mathbb{R}^2, \tag{8}$$

where P_n and Q_n are real polynomials of degree at most n. Restricted versions of Hilbert's problem for the equations of Abel and Liénard, and for quadratic vector fields (see Theorems 2–4) have been solved by passing to the complex domain. These solutions use concepts introduced by Petrovskii and Landis; among them are *complex cycle*, *complex limit cycle*, and *complex Poincaré mapping*.

Let us complexify Eq. (8):

$$\frac{\mathrm{d}w}{\mathrm{d}z} = \frac{P_n(z,w)}{Q_n(z,w)}, \quad (z,w) \in \mathbb{C}^2. \tag{9}$$

Polynomials P_n and Q_n will be considered with not just real coefficients, but also complex ones. Equation (9) defines a field of complex lines (real two-dimensional planes). The integral surfaces of this field are holomorphic curves (Riemann surfaces). The topology of the integral curves of the real equation (8) is simple: they are either straight lines or circles. The topology of the complex integral curves of Eq. (9) is much more complicated: they may be Riemann surfaces of arbitrarily high genus. A *complex cycle* is a nontrivial class of freely homotopic loops on the (real two-dimensional) integral curves of Eq. (9). At each nonsingular point of Eq. (9) (where the numerator and denominator do not vanish simultaneously) there exists a neighborhood in which the integral curves of the equation can be rectified. This means that there exists a mapping of the neighborhood onto a bidisk (Cartesian product of two disks) that maps the intersections of a pre-image neighborhood with integral curves of the equation into disks parallel to the first factor. Such a locally rectifiable partition of the domain into pairwise disjoint holomorphic curves is called a *foliation on analytic curves*.

The partition into integral curves of a neighborhood of a loop γ lying on an integral curve can be described as follows. Cover γ by neighborhoods U_j, $j = 1, \ldots, m$, in which the direction field (9) can be rectified and number them cyclically, so that each neighborhood has a nonempty intersection with its successor and the last coincides with the first. Choose a transversal section Γ_j in each neighborhood U_j lying on a complex line intersecting the loop γ in a point O_j. The transition functions from one rectifying chart to another generate germs of biholomorphic mappings $f_j\colon (\Gamma_j, O_j) \to (\Gamma_{j+1}, O_{j+1})$. The composition of m such germs is the germ of a mapping of the first transversal onto itself:

$$\Delta_\gamma\colon (\Gamma_1, O_1) \to (\Gamma_1, O_1), \quad \Delta_\gamma = f_m \circ \cdots \circ f_1.$$

This germ is called the *complex Poincaré mapping* or *holonomy transformation* of the loop γ. This mapping either has an isolated fixed point O_1 or is the identity mapping. (This follows from the uniqueness theorem for analytic functions.) The Poincaré mappings of freely homotopic loops are analytically conjugate; for that reason either they all simultaneously have an isolated singular point, or they are all the identity mapping. In the first case the corresponding complex cycle is called a *limit cycle*, in the second it is called *an identity cycle*. A natural question arises:

What are the global properties of the phase portraits of holomorphic differential equations on the complex projective plane?

It turns out that the properties of typical complex equations are strikingly unlike the analogous properties of real equations. Instead of a finite number of limit cycles, there is a countable number; instead of structural stability, there is absolute structural instability (a property opposite to structural stability and defined below).

An equation α is called *absolutely structurally unstable* if there exists a neighborhood of α in the class (9) and a neighborhood of the identity mapping in the space of homeomorphism of the complex plane onto itself having the following property: every equation of the first neighborhood that is topologically equivalent to α and conjugate to α via a homeomorphism of the second neighborhood is *affinely equivalent* to α.

Theorem 5. *For each $n \geqslant 2$ in the class of equations* (9) *with fixed n there exists a real algebraic subset of codimension* 1 *outside which every equation has the following properties*:

— denseness, which in another terminology is minimality (*all the integral curves except the line at infinity are dense in* $\mathbb{C}^2$);

— absolute structural instability;

— the presence of a countable number of complex limit cycles.

This theorem sums up forty years of development of the theory. The property of denseness for typical equations (9) was discovered by M. G. Khudai-Verenov in 1962 [Kh-V]. Absolute structural instability and the countable number of cycles for typical equations were revealed in [I78]. In all these theorems "typicality" means "outside an exceptional set of Lebesgue measure 0 in the parameter space." This "bad" exceptional set was replaced by a Zariski-closed set as a result of papers of A. A. Shcherbakov, I. Nakai, L. Ortiz, and E. Rosales ([Shch], [N], [SRO]).

It is possible to prove Theorem 5 because the typical equation (9) has a solution with a rich fundamental group: the line at infinity with points at infinity removed. The Poincaré mappings corresponding to loops that enclose these points form the *monodromy group at infinity*, which is generated by a finite number of germs of conformal mappings. For such groups it is possible to prove the analogs of the properties of denseness, absolute structural instability, and countability of the number of cycles, after which these properties are carried over to Eq. (9).

In conclusion, we present a number of problems.

One may consider a special subset of equations on the projective plane that belong to the class (9), which in a certain sense is more natural than that class. This subset consists of equations of the form (9) for which the line at infinity *is not* an integral curve. For such equations there does not exist a distinguished affine neighborhood. They are connected with the famous problem of *minimal sets* posed by Camacho [Ca].

A *minimal set* of a foliation with singularities on the complex projective plane is a *closed, invariant, nonempty* subset of the plane not containing any proper subset having the same three properties. Invariance means that the set either consists of one singular point (and in this case it is called *trivial*) or contains no singular points at all, and together with each its point the set also contains the entire integral curve passing through that point.

Problem 7 ([Ca], on minimal sets)**.** *Do there exist foliations with singularities on* $\mathbb{C}P^2$ *having a nontrivial minimal set*?

The following problem goes back to the paper of Petrovskii and Landis.

Problem 8 (Conservation of a cycle)**.** *Consider a complex limit cycle of Eq.* (9). *Can it be continuously extended to a family of complex limit cycles over a typical curve in the parameter space*?

A related problem:

Problem 9. *To what limits can one extend a monodromy transformation of a complex limit cycle of Eq.* (9)?

This problem is closely connected with simplified versions of Hilbert's 16th problem for the equations of Abel and Liénard.

The Infinitesimal 16th Hilbert Problem

The problem in the title of this section involves the zeros of Abelian integrals depending on parameters, and arises in the theory of perturbations of integrable polynomial differential equations.

Consider a real polynomial H of degree $n+1$ in two variables. A connected component of the level curve $H=t$ diffeomorphic to the circle is denoted $\gamma(t)$ and called an *oval* of the polynomial H. These ovals form a family parameterized by the value of the polynomial. Let ω be a real 1-form whose coefficients are polynomials of degree at most m. Set

$$I(t)=\int_{\gamma(t)}\omega. \tag{10}$$

Problem 10. *Find an upper bound $V(m,n)$ for the number of real zeros of the integral* (10). *The estimate must be uniform relative to the choice of the polynomial H, the family of ovals $\{\gamma(t)\}$, and the form ω. It is to depend only on the degrees m and n.*

Theorem 6 ([V], [Kh84]). *For any m and n, the upper bound $V(m,n)$ in the infinitesimal 16th Hilbert problem exists.*

The proof of this theorem is based on a theorem of Hironaka on resolution of singularities and on Khovanskii's theory of fewnomials.

The connection between this problem and differential equations is given by the *Pontryagin criterion* [Po]. Consider a perturbation of the integrable system

$$\mathrm{d}H+\varepsilon\omega=0. \tag{11}$$

We will say that an oval of the polynomial H *generates a limit cycle* under the perturbation (11) if for all sufficiently small ε there exists a family of closed curves depending continuously on ε and having the following property: For $\varepsilon\neq 0$ the curve of the family is a limit cycle of Eq. (11) and for $\varepsilon=0$ it coincides with the oval $\gamma(t)$.

The Pontryagin Criterion. *If an oval $\gamma(t)$ of the polynomial H generates a limit cycle under the perturbation* (11), *then $I(t)=0$. On the other hand, if $I(t)=0$ and $I'(t)\neq 0$, then the oval $\gamma(t)$ generates a limit cycle of Eq.* (11).

The study of Abelian integrals as branching functions of a *complex* variable t made it possible to obtain purely real results on the *location* of limit cycles of planar polynomial vector fields and to give a lower bound on the number of them. To be specific, suppose the polynomial H of degree $n+1$ satisfies certain typicality requirements. Take N *arbitrary* ovals $\gamma_1, \dots, \gamma_N$ of this polynomial, where $N = \frac{1}{2}n(n+1) - 1$. These ovals may belong to *different* families of ovals of the polynomial H.

Theorem ([I69a], [Push]). *For the polynomial H, the number N, and the ovals $\gamma_1, \dots, \gamma_N$ described above, there exists a perturbation* (11) *having N limit cycles located near these ovals.*

"Near" means that the Hausdorff distance between the limit cycle and the oval corresponding to it is small.

A number of precise results on the zeros of an Abelian integral over ovals on elliptic curves (Riemann surfaces diffeomorphic to a torus) have been obtained by G. S. Petrov [Pe] and applied in bifurcation theory [Mar]. Several theorems on the growth of the number $V(m,n)$ as a function of m are proved in the papers of Il'yashenko and Yakovenko, D. I. Novikov and Yakovenko, and also in an unpublished paper of Petrov–Khovanskii. In this last paper it is asserted that $V(m,n)$ increases linearly with respect to m: $V(m,n) \leqslant A(n)m + B(n)$. The number $A(n)$ has a polynomial upper bound. The existence of $B(n)$ can be derived from the theorem of Varchenko–Khovanskii; this derivation is a pure existence theorem. On the other hand, it is the estimate of $B(n)$ that is of interest for application to Hilbert's 16th problem.

For *quadratic* equations (11) the problem has been solved (see [Ga], [CLY]):

$$V(2,2) = 2.$$

The *bounded* version of the infinitesimal 16th problem is to make a uniform estimate of the number of zeros of the integral (10) uniformly, not over all polynomials H but only over those belonging to a compact set described below. In the space of all polynomials H of degree $N+1$, we distinguish the *discriminant set* consisting of polynomials H for which a certain explicitly stated requirement of typicality is violated. A parameter δ is determined that characterizes the distance from the polynomial H to the discriminant set. The compact set mentioned above consists of all polynomials H lying at distance at least δ from the discriminant set. The estimate for the number of real zeros of the integral (10) can then be expressed as a function of n and δ.

Novikov and Yakovenko [NYa01] have obtained an estimate of the number of zeros of the integral (10) as a tower of four exponentials of n and δ. This estimate is based on a theory developed by the authors that makes it possible, in particular, to estimate the "wiggling" of phase curves of a polynomial vector field [NYa99]. In a paper of A. A. Glutsyuk and Yu. S. Il'yashenko, the analogous number of zeros is bounded above by an exponential of a polynomial.

Results and Problems

Let us briefly summarize the results of the development of the qualitative theory of differential equations and give a list of the main unsolved problems. Detailed statements for these can be found above. It goes without saying that this résumé is subjective.

Main Results

— The theory of normal forms of elementary singular points.

— Resolution of singularities and the study of complex singular points.

— The global topological classification of vector fields on the plane and the two-sphere.

— Functional moduli of the analytic classification of resonance singularities.

— The theory of local and nonlocal bifurcations of planar vector fields.

— The finiteness theorem for limit cycles.

— The existence of a uniform estimate of the number of zeros of Abelian integrals.

The Main Unsolved Problems

— Hilbert's 16th problem.

This problem is central for the future development of the theory.

— The Hilbert–Arnold problem.

— The infinitesimal 16th Hilbert problem.

— The creation of a program for development of a multi-parametric theory of nonlocal bifurcations on the plane.

— Analytic solvability of the problem of stability of singular points in the plane.

For equations in the complex domain we note the following two problems.

— The problem of conservation of complex limit cycles.

— The minimal-set problem.

Bibliography

[A77] V. I. Arnold. Loss of stability of self-oscillations close to resonance and versal deformations of equivariant vector fields. *Funct. Anal. Appl.*, 1977, **11**(2), 85–92.

[AAISh] V. I. Arnold, V. S. Afraimovich, Yu. S. Il'yashenko, L. P. Shil'nikov. *Bifurcation Theory and Catastrophe Theory*. Berlin: Springer, 1994 (Dynamical Systems V; Encyclopædia Math. Sci., **5**).

[AI] V. I. Arnold, Yu. S. Il'yashenko. Ordinary differential equations. In: *Ordinary Differential Equations and Smooth Dynamical Systems*. Berlin: Springer, 1988, 1–148 (Dynamical Systems I; Encyclopædia Math. Sci., **1**).

[An] A. F. Andreev. *Singular Points of Differential Equations*. Minsk: Vysheishaya Shkola, 1979 (Russian).

[ALGM66] A. A. Andronov, E. A. Leontovich, I. I. Gordon, A. G. Maier. *Qualitative Theory of Second-Order Dynamic Systems*. New York: Halsted Press, 1973.

[ALGM67] A. A. Andronov, E. A. Leontovich, I. I. Gordon, A. G. Maier. *Theory of Bifurcations of Dynamic Systems on a Plane*. New York: Halsted Press, 1973.

[AP] A. A. Andronov, L. S. Pontryagin. Structurally stable systems. *Dokl. Akad. Nauk SSSR*, 1937, **14**, 247–250 (Russian).

[AVKh] A. A. Andronov, A. A. Vitt, S. E. Khaikin. *Theory of Oscillators*. New York: Dover, 1987.

[B] I. Bendixson. Sur les courbes définies par des équations différentielles. *Acta Math.*, 1901, **24**, 1–88.

[B71] A. D. Bryuno. Analytic forms of differential equations, I. *Trudy Mosk. Matem. Obshch.*, 1971, **25**, 119–262 (Russian).

[B72] A. D. Bryuno. Analytic forms of differential equations, II. *Trudy Mosk. Matem. Obshch.*, 1972, **26**, 199–239 (Russian).

[Ba] N. N. Bautin. On the number of limit cycles which appear with the variation of coefficients from an equilibrium position of focus or center type. *Matem. Sb.*, 1952, **30**, 181–196 (Russian); *AMS Transl.*, 1954, **100**, 19 pp.

[Bo] R. I. Bogdanov. Local orbital normal forms of vector fields on the plane. *Trudy Semin. im. I. G. Petrovskogo*, 1979, **5**, 51–84 (Russian).

[Ca] C. Camacho. Problems on limit sets of foliations on complex projective spaces. In: *Proceedings of the International Congress of Mathematicians*, Vol. I, II (Kyoto, 1990). Tokyo: Math. Soc. Japan, 1991, 1235–1239.

[CLW] S.-N. Chow, Ch. Zh. Li, D. Wang. *Normal Forms and Bifurcation of Planar Vector Fields*. Cambridge: Cambridge University Press, 1994.

[CLY] S.-N. Chow, Ch. Zh. Li, Y. Yi. The cyclicity of period annuli of degenerate quadratic Hamiltonian systems with elliptic segment. *Ergodic Theory Dynam. Systems*, 2002, **22**(2), 349–374.

[D08] H. Dulac. Détermination et intégration d'une certaine classe d'équations différentielles ayant pour point singulier un centre. *Bull. Soc. Math. France*, 1908, **32**, 230–252.

[D23] H. Dulac. Sur les cycles limites. *Bull. Soc. Math. France*, 1923, **51**, 45–188.

[Dum] F. Dumortier. Singularities of vector fields on the plane. *Equations*, 1977, **23**(1), 53–106.

[DRR] F. Dumortier, R. Roussarie, C. Rousseau. Hilbert's 16th problem for quadratic vector fields. *J. Diff. Equations*, 1994, **110**(1), 86–133.

[DRS] F. Dumortier, R. Roussarie, J. Sotomayor. Bifurcations of cuspidal loops. *Nonlinearity*, 1997, **10**(6), 1369–1408.

[DRSZ] F. Dumortier, R. Roussarie, J. Sotomayor, H. Żołądek. *Bifurcations of Planar Vector Fields. Nilpotent Singularities and Abelian Integrals*. Berlin: Springer, 1991 (Lecture Notes in Math., **1480**).

[E81a] J. Écalle. *Les fonctions résurgentes.* Tome I. *Les algèbres de fonctions résurgentes.* Orsay: Université Paris-Sud, 1981.

[E81b] J. Écalle. *Les fonction résurgentes.* Tome II. *Les fonctions résurgentes appliquées à l'itération*. Orsay: Université Paris-Sud, 1981.

[E85] J. Écalle. *Les fonctions résurgentes.* Tome III. *L'équation du pont et la classification analytique des objets locaux*. Orsay: Université Paris-Sud, 1985.

[E92] J. Écalle. *Introduction aux fonctions analysables et preuve constructive de la conjecture de Dulac*. Paris: Hermann, 1992.

[Ga] L. Gavrilov, The infinitesimal 16th Hilbert problem in the quadratic case. *Invent. Math.*, 2001, **143**(3), 449–497.

[I69a] Yu. S. Il'yashenko. The origin of limit cycles under perturbation of the equation $\mathrm{d}w/\mathrm{d}z = -R_z/R_w$, where $R(z,w)$ is a polynomial. *Math. USSR, Sb.*, 1969, **7**, 353–364.

[I69b] Yu. S. Il'yashenko. An example of equations $\mathrm{d}w/\mathrm{d}z = P_n(z,w)/Q_n(z,w)$ having a countable number of limit cycles and arbitrarily large Petrovskii–Landis genus. *Math. USSR, Sb.*, 1969, **9**, 365–378.

[I72] Yu. S. Il'yashenko. Algebraic nonsolvability and almost algebraic solvability of the center–focus problem. *Funct. Anal. Appl.*, 1972, **6**(3), 197–202.

[I78] Yu. S. Il'yashenko. Topology of phase portraits of analytic differential equations on the complex projective plane. *Trudy Semin. im. I. G. Petrovskogo*, 1978, **4**, 83–136 (Russian).

[I82] Yu. S. Il'yashenko. Singular points and limit cycles of differential equations in the real and complex plane. Preprint No. 38, Research Computing Center of the USSR Academy of Sciences. Pushchino, 1982 (Russian).

[I85] Yu. S. Il'yashenko. Dulac's memoir 'On limit cycles' and related questions of the local theory of differential equations. *Uspekhi Mat. Nauk*, 1985, **40**(6), 41–78 (Russian).

[I91] Yu. S. Il'yashenko. *Finiteness Theorems for Limit Cycles*. Providence, RI: Amer. Math. Soc., 1991.

[I93a] Yu. S. Il'yashenko, ed. *Nonlinear Stokes Phenomena*. Providence, RI: Amer. Math. Soc., 1993.

[I93b] Yu. S. Il'yashenko. Local dynamics and nonlocal bifurcations. In: *Bifurcations and Periodic Orbits of Vector Fields* (Montreal, 1992). Dordrecht: Kluwer Acad. Publ., 1993, 279–319.

[I00] Yu. S. Il'yashenko. Hilbert-type numbers for Abel equations, growth and zeros of holomorphic functions. *Nonlinearity*, 2000, **13**(4), 1337–1342.

[I02] Yu. S. Il'yashenko. Centennial history of Hilbert's 16th problem. *Bull. Amer. Math. Soc.*, 2002, **39**(3), 301–354.

[IKh] Yu. S. Il'yashenko, A. G. Khovanskii. Galois groups, Stokes operators, and a theorem of Ramis. *Funct. Anal. Appl.*, 1990, **24**(4), 286–296.

[ILl*] Yu. S. Il'yashenko, J. Llibre. Restricted Hilbert problem for quadratic vector fields, to appear.

[IP] Yu. S. Il'yashenko, A. Panov. Some upper estimates of the number of limit cycles of planar vector fields with applications to the Liénard equation. *Moscow Math. J.*, 2001, **1**(4), 583–599.

[IYa] Yu. S. Il'yashenko, S. Yu. Yakovenko. Finitely-smooth normal forms of local families of diffeomorphisms and vector fields. *Russ. Math. Surveys*, 1991, **46**(1), 1–43.

[IYa95] Yu. S. Il'yashenko, S. Yu. Yakovenko. Finite cyclicity of elementary polycycles in generic families. In: *Concerning the Hilbert 16th Problem*. Providence, RI: Amer. Math. Soc., 1995, 21–95 (AMS Transl., Ser. 2, **165**).

[K] V. Yu. Kaloshin. The existential Hilbert 16th problem and an estimate for cyclicity of elementary polycycles. *Invent. Math.*, 2003, **151**(3), 451–512.

[Kh84] A. G. Khovanskii. Real analytic varieties with the finiteness property and complex Abelian integrals. *Funct. Anal. Appl.*, 1984, **18**(2), 119–127.

[Kh91] A. G. Khovanskii. *Fewnomials*. Providence, RI: Amer. Math. Soc., 1991 (Transl. Math. Monographs, **88**).

[Kh-V] M. G. Khudai-Verenov. On a property of the solutions of a differential equation. *Matem. Sb.*, 1962, **56**, 301–308 (Russian).

[Khor] E. I. Khorozov. Versal deformations of equivariant vector fields in the case of symmetries of order 2 and 3. *Trudy Semin. im. I. G. Petrovskogo*, 1979, **5**, 163–192 (Russian).

[Kl] O. Kleban. On the order of topologically sufficient jets of the germ of a characteristic vector field in the plane. In: *Concerning the Hilbert 16th Problem*. Providence, RI: Amer. Math. Soc., 1995, 125–153 (AMS Transl., Ser. 2, **165**).

[KS] A. Yu. Kotova, V. V. Stanzo. On few-parameter generic families of vector fields on the two-dimensional sphere. In: *Concerning the Hilbert 16th Problem*. Providence, RI: Amer. Math. Soc., 1995, 155–201 (AMS Transl., Ser. 2, **165**).

[L] E. A. Leontovich. On the appearance of limit cycles of the loop of a separatrix. *Dokl. Akad. Nauk SSSR*, 1951, **78**, 641–644 (Russian).

[Le] L. Leau. Étude sur les équations fonctionelles à une ou plusieurs variables. *Ann. Fac. Sci. Toulouse*, 1897, **11**, E1–E110.

[Lef] S. Lefschetz. On a theorem of Bendixson. *J. Diff. Equations*, 1968, **4**, 66–101.

[Ma] B. Malgrange. Travaux d'Écalle et de Martinet–Ramis sur les systèmes dynamiques. In: Séminaire Bourbaki. *Astérisque*, 1982, **92–93**, 59–73.

[Mar] P. Mardešić. *Chebyshev Systems and the Versal Unfoldings of the Cusp of Order n.* Paris: Hermann, 1998.

[MM] J.-F. Mattei, R. Moussu. Holonomie et intégrales premières. *Ann. Sci. École Norm. Supér.* (4), 1980, **13**(4), 469–523.

[MMa] N. B. Medvedeva, E. V. Mazaeva. A sufficient focus condition for a monodromic singular point. *Trudy Mosk. Matem. Obshch.*, 2002, 77–103 (Russian).

[MR82] J. Martinet, J.-P. Ramis. Problèmes de modules pour des équations différentielles non linéaires du premier ordre. *Publ. Math. IHES*, 1982, **55**, 63–164.

[MR83] J. Martinet, J.-P. Ramis. Classification analytique des équations différentielles non-linéaires résonantes du premier ordre. *Ann. Sci. École Norm. Supér.* (4), 1983, **16**(4), 571–621.

[N] I. Nakai. Separatrices for nonsolvable dynamics on $(\mathbb{C},0)$. *Ann. Inst. Fourier* (Grenoble), 1994, **44**(2), 569–599.

[NYa99] D. I. Novikov, S. Yu. Yakovenko. Trajectories of polynomial vector fields and ascending chains of polynomial ideals. *Ann. Inst. Fourier* (Grenoble), 1999, **49**(2), 563–609.

[NYa01] D. I. Novikov, S. Yu. Yakovenko. Redundant Picard–Fuchs systems for Abelian integrals. *J. Diff. Equations*, 2001, **177**(2), 267–306.

[P] H. Poincaré. Sur les propriétés des fonctions définies par les équations aux différences partielles. Thèse, 1879.

[P1] H. Poincaré. Memoire sur les courbes définies par une équation différentielle. *J. Math. Pures et Appl.*, 1881, **7**, 251–296.

[P-M] R. Perez-Marco. Fixed points and circle maps. *Acta Math.*, 1997, **179**(2), 243–294.

[Pe] G. S. Petrov. Elliptic integrals and their nonoscillation. *Funct. Anal. Appl.*, 1986, **20**(1), 37–40.

[PL55] I. G. Petrovskii, E. M. Landis. On the number of limit cycles of the equation $\mathrm{d}y/\mathrm{d}x = P(x,y)/Q(x,y)$, where P and Q are polynomials of degree 2. *Matem. Sb.*, 1955, **37**, 209–250 (Russian).

[PL57] I. G. Petrovskii, E. M. Landis. On the number of limit cycles of the equation $\mathrm{d}y/\mathrm{d}x = P(x,y)/Q(x,y)$, where P and Q are polynomials of degree 2. *Matem. Sb.*, 1957, **85**, 149–168 (Russian).

[Po] L. S. Pontryagin. On dynamical systems that are close to Hamiltonian. *Zh. Eksp. Teoret. Fiz.*, 1934, **4**(8), 234–238 (Russian).

[Push] I. A. Pushkar'. Multidimensional generalization of the Il'yashenko theorem on Abelian integrals. *Funct. Anal. Appl.*, 1997, **31**(2), 100–108.

[Pya72] A. S. Pyartli. Birth of complex invariant manifolds close to a singular point of a parametrically dependent vector field. *Funct. Anal. Appl.*, 1972, **6**(4), 339–340.

[Pya78] A. S. Pyartli. Cycles of a system of two complex differential equations in a neighborhood of a fixed point. *Trudy Mosk. Matem. Obshch.*, 1978, **37**, 95–106 (Russian).

[R] R. Roussarie, On the number of limit cycles which appear by perturbation of separatrix loop of planar vector fields. *Bol. Soc. Brasil Mat.*, 1986, **17**(2), 67–101.

[RS] J.-P. Ramis, Y. Sibuya. Hukuhara domains and fundamental existence and uniqueness theorems for asymptotic solutions of G every type. *Asymptotic Anal.*, 1989, **2**(1), 39–94.

[S] S. Smale. Mathematical problems for the next century. *Math. Intelligencer*, 1998, **20**(2), 7–15.

[Se] A. Seidenberg. Reduction of singularities of the differential equation $A\,dx = B\,dy$. *Amer. J. Math.*, 1968, **90**, 248–269.

[Shch] A. A. Shcherbakov. Topological and analytic conjugacy of noncommutative groups of germs of conformal mappings. *Trudy Semin. im. I. G. Petrovskogo*, 1984, **10**, 170–196 (Russian).

[Si] C. L. Siegel. Iteration of analytic functions. *Ann. Math.*, 1942, **43**(2), 607–612.

[SRO] A. A. Shcherbakov, E. Rosales-González, L. Ortiz-Bobadilla. Countable set of limit cycles for the equation $dw/dz = P_n(z,w)/Q_n(z,w)$. *J. Dynam. Control Systems*, 1998, **4**(4), 539–581.

[V] A. N. Varchenko. Estimate of the number of zeros of an Abelian integral depending on a parameter and limit cycles. *Funct. Anal. Appl.*, 1984, **18**(2), 98–108.

[Vo] S. M. Voronin. Analytic classification of germs of conformal mappings $(\mathbb{C},0) \to (\mathbb{C},0)$. *Funct. Anal. Appl.*, 1981, **15**(1), 1–13.

[Y] J.-C. Yoccoz. Théorème de Siegel, nombres de Bruno et polynômes quadratiques. Petits diviseurs en dimension 1. *Astérisque*, 1995, **231**, 3–88.

[Z83] H. Żołądek. Versality of a family of symmetric vector fields on the plane. *Matem. Sb.*, 1983, **120**(4), 473–499 (Russian).

[Z87] H. Żołądek. Bifurcations of certain family of planar vector fields tangent to axes. *Diff. Equations*, 1987, **67**(1), 1–55.

P. S. Krasnoshchekov

Computerization... Let's Be Careful

Translated by R. Cooke

I believe that computing is changing the world more than any other factor today ...
The impact will be so profound that no industry will stay unchanged.
Bill Gates
"On the present and future of computer technology"
Moscow, the Kremlin, 10 October 1997

One has only to open one's eyes to see that the triumphs of industry,
which have enriched so many practical men, would never have seen the light
if only these practical men had existed, and if they had not been preceded
by disinterested fools who died poor, who never thought of the useful,
and yet had a guide that was not their own caprice.
Henri Poincaré
Science and Method, 1908

And you shall know the truth, and the truth shall set you free.
Jesus Christ
The Gospel According to John 8 : 32

Bill Gates is not the first person to rhapsodize over the triumphs of technological progress, the blessings it brings to the world, and the radical changes to which it subjects this world. It is indeed difficult to deny all these triumphs, blessings, and changes. Even so, let us hearken to those who see behind the glittering façade the other, negative side of this process.

In Russia, one of the first to openly cast doubt on blind faith in technological progress was Lev Nikolaevich Tolstoy: "I implore the reader… to remember these simple facts: that an army once increased can never be reduced; that ancient forests once destroyed cannot be renewed; that a population once corrupted by comfort and convenience can never be brought back to its primitive simplicity and moderation." He says further: "Those who believe in progress are sincere because their faith is profitable for them, and therefore they preach their faith with ferocity and cruelty. I cannot help remembering the Chinese war, in which three great powers, in complete sincerity, naively introduced the faith in progress into China by means of gunpowder and cannonballs" ("Progress and the definition of education," a response to Mr. Markov, *Russkii Vestnik*, 1862, No. 5).

I am not an opponent of technological progress. My life has turned out in such a way that it was necessary to apply my knowledge and talents in various large technological projects, and even in military affairs. I gave many years to the creation of automated design systems, which, one can say without exaggeration, was the model for the new computer information technology. In the creation of these systems it was necessary to solve problems whose existence Bill Gates may not even suspect. Solving these problems gave me a balanced and sober view of hasty computerization, and brought me to an understanding of an eternal truth: *In order to go further, one must make the necessary stops.*

The computer found its first effective application, as one would expect, in science. It gave impetus to the rapid development of many areas of applied science (such as, for example, automata theory), and it breathed life into the development of computational methods (computational mathematics is now recognized as an independent area of research in mathematics). Cybernetics, which is the study of control, has displayed its own indisputable achievements. A new profession has arisen — that of programmer; a new branch of mathematics — programming; and a new area of research — programming languages.

However, the first wave of euphoria has passed. It was suddenly noticed that the fascination with computation had begun to retard purely theoretical research. In scientific seminars computational results were being discussed more and more, and their interpretation was made difficult by the absence of a critical analysis of the theoretical models on which the computations were based.

A dangerous tendency has been noted in the classical "model–algorithm–program" triad. The effort to introduce computer technology into all spheres of life as quickly as possible has led to a decline in the quality of the theoretical models being used.

Many scholars, who believe in information as an absolute good, have invaded different areas of human activity with computers in order to process information and assist in reaching rational decisions. But mathematical methods and theoretical models have followed the computer at a much slower pace. And inadequate software generates an illusory knowledge in the user, based on an entrenched error, as a rule. What is taken for truth is actually a transformation of it, a plausible illusion. It is particularly dangerous when this happens in making crucial decisions in the socio-economic sphere.

However, the danger is as yet not very great. There is no euphoria among the devotees. The computer has taken its proper place, the place of an intelligent, disciplined assistant and even, in a certain sense, a colleague. The computer has remained a great help in the intellectual activity of humankind, but has not become an artificial intelligence, although it sometimes happens that chess programs win matches against world champions. Fortunately, the laws of intellectual activity are hidden from us under seven seals. This applies especially to the highest form of intellectual activity — creativity. This property of Man is a gift of God (in his image and likeness) or, if you prefer, the result of millions of years of evolution; in the final analysis, the two are the same. However, the human capacity for creative thought has been given to us in potential only — it can manifest itself and develop successfully only in an intellectual environment, in living contact with our own kind, that is, in human society. It is known that the so-called *Mowglis* raised by animals nearly always lose their intelligence. But the computer will never acquire such an environment, whatever may be said about self-teaching systems and the like. That is why the principles on which computer intelligence (or rather, pseudo-intelligence) is organized are completely different.

The opinion is widespread at present that the main reason the Russian Federation is behind the USA in many areas was the undervaluation of computer technology. That is not exactly the case. Let us recall that the Soviet Union rapidly overtook the USA in the creation of nuclear weapons, surpassed it in delivery systems, and was first into space. No one would dare claim that Soviet computers were better than American computers. The race was won by living human intelligence, which succeeded in effectively organizing problems for solution.

For that reason I would like to warn against computer fundamentalism. Every powerful new technology, whether automobilization or computerization, brings along with its obvious blessings a highly nonobvious threat to the harmonious and stable existence of the noosphere. Computerization is dangerous primarily because it acts on the most fragile and vulnerable component of the noosphere — the living intellectual environment. Bill Gates has spoken professionally and with inspiration of the usefulness of computerization. But it is worthwhile to think of the possible irreversible negative consequences that it may introduce into life.

Let me begin, if I may, with what is in my opinion the most important problem: the role of education, which, in the words of Gates "will sound entirely different." Education is a capacious and multilevel concept. To give an exhaustive definition of this concept is most likely impossible. Therefore, when Gates asserts that "in the future the most interesting question to ask of somebody, in order to understand their job opportunities, will be: 'What is your education?'," it is far from clear how to frame a response. Indeed, what could one do for the present author, whose specialty is described on his undergraduate diploma as mechanical engineering; on his docent diploma as mathematics; on his doctoral degree as cybernetics; on his professorial license as operations research, and on his certificate of election to the Academy as information and automata theory? How is he to answer the question posed above? After all, each of these qualifications of the author can be regarded as an education, but at the same time they are also education when taken all together. Moreover, this education is not merely a collection of professions having little connection with one another. They all complement each other harmoniously. Acting in concert, they form something qualitatively new and give the author immeasurably greater capability for scientific research than each would do if taken alone. And in Russia this is more commonplace than exceptional.

Unfortunately, computer technology is now being tried on the sacred millennium-old "teacher–student" interaction. In many technological institutes the computer provides examinations; more precisely, it tests. Such an examination is devoid of the principal and most important element — the dialog between teacher and student. An "averaging" is conducted over all the students who take the test. The computer takes no account of their irreproducible individuality; it cannot follow the logical reasoning of the person taking the test, and cannot afford him a chance to defend his point of view in a dialog. But testing is only a half-victory. It is being proposed that computer technology can be used to unify the lecturing process. But it is no good at all. A lecturer does not merely enunciate theoretical material; he interprets it. There are as many interpretations as there are lecturers. Even the same lecturer giving the same lecture does not repeat it word for word from one time to the next. Lecturing is a creative process. Lecturers are like orchestra conductors, each of whom gives his own interpretation of the same musical composition. Although it may seem that the orchestra has the score and that is all that is needed, there is a reason why great conductors and favorite lecturers exist. It is in the living interaction of students with the teacher, or of scholars in seminars and conferences that the aura, the environment, arises without which the living intellect cannot exist and develop.

Computer technology, by its very nature, introduces into the educational sphere those attributes of uniformity and standardization so loved by Americans. Although useful to a degree, unless it is stopped in time, it will lead to the bureaucratization of education — and education, as the ritual of making a person acquainted with knowledge and truth, will cease to exist. The system will begin to place its stamp of approval on those *educatees* (as Solzhenitsyn has accurately remarked) who cope with routine work but are utterly devoid of the culture of creative thought. The decline of scientific schools will begin — a phenomenon whose first signs are already visible. And this is dangerous, since there are, in any event, not very many people capable of creative thought in human society. "... the majority of people do not like to think, and that may be for the best, for they are guided by instinct... But instinct is a routine, and if not fructified by thought it would not progress any further even in man than in the bee or the ant. Consequently, it is necessary that someone think on behalf of those who don't like to think..." (H. Poincaré, *Science and Method*; Book 1, *The Scholar and Science*, 1908).

It is important that as many people as possible learn to think. Let us heed the words of Pascal: "... all our dignity consists of thought. We should exalt ourselves in that respect, not in relation to space and time, which we could never fill. Let us try instead to learn to think well: that is the principle of morality." In this connection we cannot avoid mentioning the Internet — another computer technology that is now much in fashion. It is possible that the Internet may enable us to fill time and space (there is a reason why it is also called the Web), but it is very doubtful that it can help us learn to think well.

Thus we must constantly take care to assure that computer technology brings us not only comfort and entertainment, but that it liberates the potential of our intelligence for creative thought. This is very important, since in our world technological progress is being implemented and directed by the efforts of, and in the interest of, business.

And modern business, despite all its indisputable good features, has one inherent defect: its main concern is to make a profit, preferably as quickly as possible. For that reason, to put the matter mildly, it is neutral with respect to morality. Business is in a hurry, it has no time to make the necessary stops — it simply doesn't notice the necessity. Thinking well is not a principle of business; its principle is thinking effectively. For that reason, in the larger scheme, business does not care if certain computer technologies that it advertises produce computer addicts who retreat from real life into the illusory world of virtual reality. The situation is arising in which life will have to adapt to the laws of business, rather than the reverse. The world is being turned upside down: business is becoming the only objective reality, which humankind will have to deal with and accept. Anything that does not yield a profit will lose the right to exist. The voluntary study of business is turning into its opposite — a compulsory study. That is why the computer technologies that are being developed most successfully are those in demand by businessmen and in the sphere of entertainment.

For people, “whose dignity consists of thought” and whose freedom resides in the knowledge of the truth, such an order of things is unacceptable. The disappearance of the “disinterested fools” from the arena of life will cause the instinctive striving of “practical men” for profit to cease being “fructified by thought,” and practice will become ineffective. There will simply be nothing to sell.

I am deliberately painting a lurid picture. Of course, the situation is not so tragic as all that. But you must agree that the tendency is noticeable. Nevertheless, the way out of this situation is known: computer technologies need to be perfected. They must become more knowledgeable, thereby expanding their sphere of application. One must not forget what the computer was originally invented for. Its inventors intended that it should promote not so much technical progress as the progress of humanity in its eternal quest to know the truth. On that path, as history shows, everything will turn out well even with technical progress. It is not a coincidence that Pascal, who called us first of all to learn “to think well,” was simultaneously the inventor of an adding machine — the first prototype, one may say, of the modern computer.

So then, what must be done in order to make computer technologies more and more knowledgeable? I shall try to state here a number of problems which, it seems to me, must be solved. I came to understand them during the twenty years I spent creating automated design systems, but the problems themselves are sufficiently general in nature that one can speak of computer technologies in general.

In the earliest automated design systems the creators attempted to make maximal use of the possibilities presented by the computer: rapid execution of a large number of computations; storage and retrieval of a large volume of information; visualization of results by means of computer graphics; human interaction with the computer in dialog mode. Over time, this approach spilled over into the computer technology that is now called an expert system. Such a system is based on the picture of the user as a specialist in a particular area of knowledge (physician, economist, engineer, executive, and so on) who is to be assisted in applying his knowledge to a specific job. These systems are used today in various spheres of activity. For example, they turned out to be very successful in coping with specific diagnostic problems, in which knowledge can be presented in the form of rather rigid instructions acting in the strict framework of formal logic. However, those who attempted to use the ideas of expert systems for automated design of such complex objects as an airplane, a ship, a factory, or a system for controlling these things, were unable to overcome the theoretical problems. In exactly the same way, the automobile did not take the problem of transportation off the agenda. The ubiquitous automobilization generated new problems: accidents and traffic jams, environmental pollution, parking and storage difficulties. And when the modern design offices were equipped with so-called personal work stations (certain analogs of expert systems) and linked in a network intended to carry out the functions of an automated design system, problems arose that were no less acute. One of them is that enormous volumes of information began to be processed in the system. And it is known from theory that this leads to an increase in entropy of the system, that is, information on the current state of the project is distorted, and the more executing elements it has, whether a program or a live designer at his personal work station, the greater the distortion. The increase of entropy can be combatted in complex systems only by using aggregation (enlargement, generalization) of information and distributing it over levels of detail; that is, by constructing a hierarchical system of information processing. Such, in essence, is the situation in any large design office, only such a hierarchy arises spontaneously under the pressure of circumstances and depends largely on the design practice that has been set up in the given office. That is why it will not be possible to get by with a simple "integration of these programs with all the other elements," as Bill Gates claims. It will be necessary to delve into the "specialized" problems of the areas in question. It is also obvious that not all hierarchical design systems in various design offices are equivalent. One office may be more productive, another less. Thus the hierarchy must not be arbitrary, but one that flows naturally from the functional nature of the object being designed and its degree of structural complexity. Designing a screw requires no hierarchy.

Contrary to expectation, the problem of making engineering decisions has grown more complicated, not simpler. Complaints have arisen that one cannot make a unique choice, that it simply doesn't exist. Attempts to improve some features of a structure have led to a worsening of others and conversely. The hopelessness of the situation has forced us to resort to an arbitrary device:

trying not to notice “superfluous” information. The traditional method of designing “from a prototype” has not undergone any essential changes. However, practically the first result of the introduction of the computer into the sphere of design was to pose to the engineer the problem of choice in all its unpleasant ambiguity. It was time to make one of the necessary stops.

But now let us forget about design. The point is that the problems just discussed are inherent not only in design. They are encountered wherever decisions (or choices) have to be made in a complicated informational environment. Interpreting design in its broadest sense, we may define any purposeful activity as design; but the concept of *synthesis* characterizes more accurately the problem of decision making. Science has traditionally studied reality as a given. Its method was analysis. But the time came when people began to transform the world (this is what Bill Gates is talking about). These transformations of the world are the result of purposeful activity of people in various manifestations of their lives. That is why the problem of the purposeful synthesis of systems and processes of diverse nature and complexity is on the agenda. Computer technologies must enable us to solve this problem in the way that is least upsetting to us and the environment, and that is possible only if the technologies themselves help us to “think well.” But to think well, one must first learn to think correctly. For that we need to apply the achievements of basic science.

Let us return to the problems of synthesis. Among them the central problem is unquestionably the problem of choice. But choice becomes possible only when there is something to choose between, that is, a set of alternatives of the system, object, or process being synthesized. In this situation, it is known from theory that the larger the set of alternatives, the better the choice will be. And here we encounter a situation that seems paradoxical at first sight: in order to construct the optimal synthesis one must be able to synthesize the entire set of possible alternatives. This problem is very complicated, but fundamental achievements in optimization theory, game theory, and mathematical modeling (these are components of the general theory of decision making), and the perfection of modern computers, strengthen our faith that it will be solved successfully. However, there are more than enough problems to be solved on this path. And the next one is to set up a system of tests (estimates) on the basis of which an optimal synthesis can be constructed. This system of tests is none other than the formalization of our desires and requirements for the qualitites of the object or process being synthesized. An optimal synthesis cannot be constructed without it. But the trouble is that, as a rule, our desires and requirements are in conflict with one another and cannot be formalized as a single test.

Well then, what is the way out of this situation? The theory of multi-test optimization answers that question: One must abandon categorical forms of requirements, and then a solution exists. But it turns out that the solution is not at all what we expected. It is not unique. An optimal synthesis is a set of alternatives that cannot be improved on in terms of our requirements; and, what is very important, in general it contains fewer elements than the original set.

So, there is not a unique answer. However, if you think about it, this is not so bad, since it leaves us with the possibility of further choice. This choice may be determined by considerations on a higher level that cannot be discussed here, or the choice may be made on the basis of a compromise, that is, by reconciling requirements. And it may happen in such a way that necessity forces us to realize all the results of a nonunique synthesis. For example, in the production of automobiles, it is necessary to manufacture both cars and trucks. The optimal synthesis in this case seems to suggest that we cannot satisfy our requirements with the production of cars alone or trucks alone. Both are needed.

However, let us return to the problem of synthesis of the original set of alternatives. This is usually done with a structural-parametric model of the object or process being synthesized. A structural-parametric model is a description that makes it possible to obtain the complete set of alternatives by varying the structure and parameters within prescribed limits. Moreover, each particular variation distinguishes a particular alternative of the set. To a more complicated object or process there corresponds a more complicated structural-parametric model. Unfortunately, science does not yet know how to solve optimization problems in which the space of variables is of very large dimension. For that reason the model must be simplified in order to decrease the number of variables to the minimum possible. Thus it is necessary to decompose the problem, that is, to break it into steps — to introduce a hierarchy of the structural-parametric description over the levels of detail (top-down planning). We recall that we have already spoken of such a hierarchy when we were discussing the problem of the increase in entropy in an information environment. Now this is the same hierarchy, and it is implemented by using the procedure of sequential aggregation of the structural-parametric model. Finally, we need a model of the functioning object, since without it we cannot compute the values of the test criteria and reject some versions (that is, construct an optimal synthesis). Such models should include all conceivable modes of functioning, and in technical areas it is known how to construct them in practice. The situation is not so good with the functioning of socio-economic systems. But let us assume that we do have a model of the functioning object. Obviously, it is not less complex than the structural-parametric model and consequently it is also subject to decomposition. Moreover, the decomposition of one model or another should be coordinated so that all the parts can be fitted into the same hierarchy.

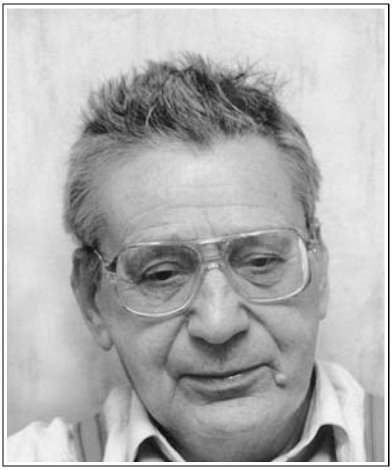
It now remains only to describe the general features of the process of optimal synthesis itself, from top to bottom along the hierarchy. It is assumed that the topmost level is the most aggregated, that is, the most simplified in the informational and descriptive sense. On this level one is dealing with macroparameters, and accordingly it is called the macrolevel. It is here that the problem of optimal synthesis is solved in essence; that is, demonstrably ineffective alternatives are rejected. This is a very important level. The possible external features (macrocharacteristics) of the future object or process are determined on this level. They have the property that it is now impossible to improve them using the set of original requirements. We cannot give preference to any of the alternatives, since they are not comparable with one another in terms of the chosen system of tests. The results of the preliminary synthesis then pass to the second, more detailed level. On this level one first solves the problem of disaggregation of the information received. This is a very difficult problem: It is necessary to find all the preimages of the object or process being synthesized by the given detailing that correspond to the macroimage arriving from the first level. Then, on the set of alternatives obtained in this way the problem of optimal synthesis is solved again, using the system of tests for the second level. Solving it is now easier than it would have been if we had begun the synthesizing process at the second level, since many alternatives were rejected in advance at the first level. And so we go, down the levels, until the complete optimal synthesis is constructed — a description of the object or process at all levels of the hierarchy is thereby obtained.

At this point, by the traditional methods of analysis — using checking calculations on exact models or in an experiment if experimental models have been prepared — we must ascertain whether the synthesis constructed is satisfactory. If it is, the problem can be considered solved; if not, the synthesizing process starts over, taking account of all corrections. And so it goes until the iteration process reaches the desired result.

This is by no means all, but I should probably stop at this point. After all, I promised to speak only about "what Bill Gates didn't say," and nothing more. There are many things that neither Bill Gates nor I have spoken of; but what we did say suffices for understanding the complexity of the problems that must be solved using computer technology, and the heavy demands they will make on that technology. For the time being I shall simply remark that the implementation of portions of this scheme in one of the design offices of the Russian Air Force contributed to the creation of one of the most manoeuvrable airplanes in the world.

I am not a prophet. That is why it is difficult for me to judge whether computer technology is developing along the lines I have indicated here, whether it is to be computer technology as Bill Gates understands it or turn into something for which there is not yet a name, and whether it will liberate human intelligence from the concerns of everyday routine and open the path to truth. In any case, as long as there exist "disinterested fools," hope is not lost. Everyone will gain, including business, whose profits will only increase, and business itself will become incomparably more moral... Only one thought troubles me: that my children, grandchildren, and great-grandchildren may lose that intellectual-emotional tension whose successful resolution provides a truly incomparable pleasure.

The present article is a version of the paper "What Bill Gates didn't talk about" published in *Vestnik Rossiiskoi Akademii Nauk*, 1998, **68**(11), 980–985.



V. A. Marchenko

The Generalized Shift, Transformation Operators, and Inverse Problems

Translated by R. Cooke

One might title this article "The way it was," using the language of television. In it I shall tell how the main inverse problems for one-dimensional second-order differential operators were solved in the period 1950–1955. More precisely: how the necessary machinery arose and where the main ideas came from.

Shift operators U_x^y, which are defined on functions $f(x)$ of a real variable x by the equality

$$U_x^y[f] = f(x+y) \qquad (x, y \in \mathbb{R}^1),$$

play an important role in classical harmonic analysis. For example, the following definitions are connected with them.

1) *The definition of almost-periodic functions.* A function $f(x)$ is *almost periodic* if the family $U_x^y[f]$ (where y is a parameter) is compact with respect to uniform convergence on the entire real line $-\infty < x < \infty$.

2) *The definition of positive definite functions.* A function $f(x)$ is *positive definite* if the inequality

$$\int_{-\infty}^{\infty}\int_{-\infty}^{\infty} U_x^{-y}[f]\,\varphi(x)\overline{\varphi(y)}\,\mathrm{d}x\,\mathrm{d}y \geqslant 0$$

holds for all functions $\varphi(x)$ of compact support.

3) *The definition of convolution.* We define the convolution $f * g$ by the equality

$$f * g(x) = \int_{-\infty}^{\infty} U_x^{-y}[f] g(y)\, \mathrm{d}y,$$

which transforms the space $L^1(-\infty, \infty)$ into a Banach algebra.

Between 1938 and 1940 Jean Delsarte and B. M. Levitan developed the theory of generalized shift operators $T_x^y[f]$, which map functions $f(x)$ into functions of two variables $T_x^y[f]$ and satisfy four axioms that generalized the properties of ordinary shift. Among these axioms the associative law is nontrivial:

$$T_r^s T_x^r[f] = T_x^r T_x^s[f].$$

(Here, and below, the subscript on the operator T_α^β means that it acts on a function of the variable α; if the function also depends on other variables, they are held fixed.)

The formal replacement of the ordinary shift by the generalized shift in classical harmonic analysis leads to theories of generalized almost-periodic functions, generalized positive definite functions, and a generalized convolution. The rigorous justification and development of these generalizations demanded the development of adequate analytic machinery. Intensive work in this area was conducted throughout World War II by Levitan and A. Ya. Povzner.

The operators that transform functions $f(x)$ into solutions $u(x,y)$ of the Cauchy problem

$$\frac{\partial^2 u}{\partial x^2} - r(x)u = \frac{\partial^2 u}{\partial y^2} - q(y)u, \tag{1}$$

$$u(x,0) = f(x), \qquad u_y'(x,0) = hf(x) \tag{2}$$

(where the functions $r(x)$, $q(y)$, and the number h are fixed) satisfy all the axioms for generalized shift operators except the associative law, which also holds if $r(t) \equiv q(t)$.

The operators defined by the relations

$$T_x^y[f] = u(x,y) \quad (-\infty < x, y < \infty),$$

$$R_x^y[f] = \begin{cases} u(x,y) & (0 \leqslant y \leqslant x < \infty) \\ u(y,x) & (0 \leqslant x \leqslant y < \infty), \end{cases}$$

were the first nontrivial examples of generalized shift operators on the entire line (T_x^y) and the half-line (R_x^y).

By applying Riemann's method to solve the Cauchy problem (1)–(2), Povzner obtained the following representation of the solution:

$$u(x,y) = \frac{1}{2}\left\{ f(x+y) + f(x-y) + \int_{x-y}^{x+y} R(x,y,t) f(t)\,\mathrm{d}t \right\}, \tag{3}$$

where $R(x,y,t)$ is the function obtained from the Riemann function by passing from the characteristics of Eq. (1) to the old coordinates. It is uniquely determined by the functions $r(x)$, $q(y)$, and the number h.

The solution of Eq. (1) obtained by separation of variables has the form

$$u(x,y) = \varphi(\mu,x)\psi(\mu,y),$$

where $\varphi(\mu,x)$ and $\psi(\mu,y)$ are arbitrary solutions of the equations

$$\frac{\mathrm{d}^2}{\mathrm{d}x^2}\varphi - r(x)\varphi = \mu\varphi, \quad \frac{\mathrm{d}^2}{\mathrm{d}y^2}\psi - q(y)\psi = \mu\psi,$$

and μ is an arbitrary constant. If $\psi(\mu,0) = 1$ and $\psi'(\mu,0) = h$, the function $u(x,y)$ obviously satisfies the initial conditions

$$u(x,0,\mu) = \varphi(\mu,x), \quad u'_y(x,0,\mu) = h\varphi(\mu,x);$$

that is, it is a solution of the Cauchy problem (1)–(2) in which $f(x) = \varphi(\mu,x)$. Hence, by (3) and the uniqueness of the solution of the Cauchy problem, it follows that

$$\varphi(\mu,x)\psi(\mu,y) = \frac{1}{2}\left\{ \varphi(\mu,x+y) + \varphi(\mu,x-y) + \int_{x-y}^{x+y} R(x,y,t)\varphi(\mu,t)\,\mathrm{d}t \right\}.$$

In particular, if $r(x) \equiv 0$ and $\varphi(\mu,x) = \cos\sqrt{\mu}x$, then

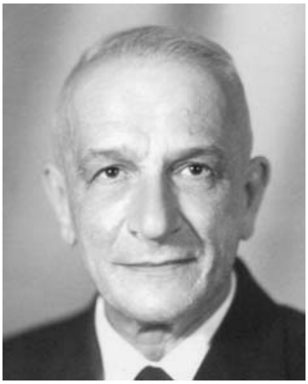
$$\begin{aligned} &\cos\sqrt{\mu}x\ \psi(\mu,y) \\ &\quad = \frac{1}{2}\left\{ \cos\sqrt{\mu}(x+y) + \cos\sqrt{\mu}(x-y) + \int_{x-y}^{x+y} R(x,y,t)\cos\sqrt{\mu}t\,\mathrm{d}t \right\}, \end{aligned}$$

from which, for $x = 0$, it follows that

$$\psi(\mu,y) = \cos\sqrt{\mu}y + \int_0^y K(y,t)\cos\sqrt{\mu}t\,\mathrm{d}t. \tag{4}$$

Here the continuous kernel

$$K(y,t) = \frac{1}{2}\{R(0,y,t) + R(0,y,-t)\}$$



A. Ya. Povzner

is independent of μ and is uniquely determined by the function $q(y)$ and the number h. Delsarte derived special cases of (4), and the general case was obtained by Povzner. Formula (4) shows that, for any function $q(x)$ and any number h, there exists an operator with an integrable kernel $K(x,t)$

$$V[f] = f(x) + \int_0^x K(x,t) f(t)\,\mathrm{d}t \quad (0 \leqslant x < \infty) \quad (5)$$

defined on all locally integrable functions $f(x)$ $(0 \leqslant x < \infty)$ and mapping the functions $\cos\sqrt{\mu}x$ into solutions of the equations

$$-\psi_{xx} + q(x)\psi = \mu\psi,$$

satisfying the initial conditions $\psi(\mu,0) = 1$, $\psi_x'(\mu,0) = h$.

Operators of the form (5) are called *Volterra operators*, and they form a group under composition. In particular, they are invertible, and the inverses have the same form

$$V^{-1}[g] = g(x) + \int_0^x L(x,t) g(t)\,\mathrm{d}t. \quad (6)$$

According to (4), there exist Volterra operators V_i $(i = 1,2)$ mapping the functions $\cos\sqrt{\mu}x$ into solutions of the equations

$$-\psi_{xx}^{(i)} + q^{(i)}(x)\psi^{(i)} = \mu\psi^{(i)}, \quad \psi^{(i)}(\mu,0) = 1, \quad \psi_x^{(i)}(\mu,0) = h_i \qquad (i = 1,2).$$

Hence for any two functions $q^{(1)}(x)$ and $q^{(2)}(x)$, $0 \leqslant x < \infty$, (now called *potentials*) and any two numbers h_1 and h_2, there exists a Volterra operator $V = V_2(V_1)^{-1}$ that maps solutions $\psi^{(1)}(\mu,x)$ of one equation into solutions $\psi^{(2)}(\mu,x)$ of the other. These Volterra operators are called *transformation operators*.

The Riemann method applied to the Cauchy problem

$$\frac{\partial^2 u}{\partial x^2} = \frac{\partial^2 u}{\partial y^2} + q(y)u$$

with initial conditions

$$u(0,x) = f(x), \quad u_y'(0,x) = f'(x),$$

leads in the same way to transformation operators of the form

$$W[f] = f(z) + \int_{-x}^x A(x,t) f(t)\,\mathrm{d}t, \quad (7)$$

which transform *any solutions* of one equation into solutions of the other equation while *conserving the initial conditions at zero*.

Of course, it was not accidental that transformation operators arose in the theory of generalized shift. They served as an important auxiliary device in the study of the Banach algebras and generalized almost-periodic and positive definite functions generated by generalized shift operators. The role of transformation operators is illustrated by the example of the generalized shift operator R_x^y, which, as one can easily see, can be expressed in terms of the transformation operator (4) by the formula

$$R_x^y[f] = V_x V_y S_x^y V_x^{-1}[f] \quad (x, y \geqslant 0),$$

where S_x^y denotes the usual symmetric shift operator on a half-line:

$$S_x^y[g] = \frac{1}{2}\big[g(x+y) + g\big(|x-y|\big)\big] \quad (x, y \geqslant 0).$$

Theoretically, this formula makes it possible to reduce the solution of problems connected with the generalized shift operator R_x^y to problems solved long ago in connection with the ordinary shift operator. But to do so it is necessary to estimate the norms of the transformation operator (4) and the operator inverse to it in the space $L^\infty(0, \infty)$ (or in a subspace of it). This is quite a difficult technical problem, which was solved in the early post-war years for real coefficients $q(x)$ satisfying the condition

$$\int_0^\infty (1+x^2)|q(x)|\,\mathrm{d}x < \infty, \tag{8}$$

making it possible in this case to give a complete description of the structure of the Banach algebras and almost-periodic functions generated by the generalized shift operators R_x^y. But the most important thing in these papers was probably *the use of transformation operators*, which became a familiar working device.

Shortly after the liberation of Khar'kov on the 23 August 1943, the faculty of the university returned from the army and evacuation and resumed their research. In those years the Department of Physics and Mathematics was not divided into physics and mathematics sections, and constant contact with physicists generated an interest in physical problems among the mathematicians. Among these problems, the *inverse problems of spectral analysis* were particularly attractive. In these problems one is required to recover a differential operator from some of its spectral characteristics. Problems of this type had first been stated and solved by V. A. Ambartsumyan in 1929. Unfortunately, we learned about this pioneering work only much later, from a 1946 article of Göran Borg. Despite the hardships of the postwar period, the university managed to subscribe to a few foreign journals, and in 1948 we received a journal (*Acta Math.*, **78**(2), 1–96) with a long article by Borg. Povzner assigned me to report on this article in the seminar. Its main result was a uniqueness theorem, according to which the real potential $q(x)$ in the equation

$$-y'' + q(x)y = \mu y \qquad (0 \leqslant x \leqslant \pi) \tag{9}$$

is uniquely determined by the spectra of two boundary-value problems, with the same boundary conditions at zero (and different conditions at π). But if even one eigenvalue is removed from the spectra, the remainder of the set does not determine the potential uniquely.

The proof of this uniqueness theorem can be easily reduced to proving that products of the eigenfunctions of the boundary-value problems are complete, a proof that plays a key role in Borg's paper. I noticed immediately that the mere fact that Volterra transformation operators exist automatically implies that these products are complete. Indeed, the existence of transformation operators implies the formula

$$\begin{aligned}
&\psi^{(1)}(\mu,x)\psi^{(2)}(\mu,x) \\
&\quad = \left(\cos\sqrt{\mu}x + \int_0^x K_1(x,t)\cos\sqrt{\mu}t\,\mathrm{d}t\right)\left(\cos\sqrt{\mu}x + \int_0^x K_2(x,t)\cos\sqrt{\mu}t\,\mathrm{d}t\right) \\
&\quad = \frac{1}{2} + \frac{1}{2}\left(\cos 2\sqrt{\mu}x + \int_0^x Q(x,t)\cos 2\sqrt{\mu}t\,\mathrm{d}t\right)
\end{aligned}$$

for products of solutions of Eqs. (9), and the question of the completeness of products of the eigenfunctions reduces to the completeness of the sequence of functions $\cos 2\sqrt{\mu_k}x$ $(0 \leqslant x \leqslant \pi)$, where μ_k ranges over the spectra of the two boundary-value problems. But the completeness of such a sequence follows immediately from long-known asymptotic formulas for the eigenvalues. The simplicity of the proof was an explicit testimony, showing that transformation operators are a natural and powerful tool for studying the spectral theory of differential operators. This motivated me to give them a special name (transformation operators), thereby greatly amusing N. I. Akhiezer, who remarked that the phrase sounded to him like "salted salt."

It would have been natural to try to generalize Borg's uniqueness theorem for Eqs. (9) to an infinite interval. Obviously, in this case one could not restrict attention to eigenvalues, since, for example, the spectrum of the problem may be purely continuous. For that reason it was necessary to seek other spectral characteristics that determine the potential uniquely.

The spectral theory of the self-adjoint operators generated by differential operators

$$L = -\frac{\mathrm{d}^2}{\mathrm{d}x^2} + q(x) \tag{10}$$

with real-valued potential $q(x)$ on finite and infinite intervals had been developed back in 1909–1910 by Hermann Weyl. In these papers, in particular, formulas are obtained for eigenfunction expansions, and Parseval's equality is proved, which for the intervals $0 \leqslant x \leqslant a$ and $a \leqslant \infty$ have the following form:

$$\begin{aligned} f(x) &= \int_{-\infty}^{\infty} \tilde{f}(\mu)\psi(\mu,x)\,\mathrm{d}\rho(\mu), \\ \tilde{f}(\mu) &= \int_0^a f(x)\psi(\mu,x)\,\mathrm{d}x, \\ \int_0^a |f(x)|^2\,\mathrm{d}x &= \int_{-\infty}^{\infty} \left|\tilde{f}(\mu)\right|^2 \mathrm{d}\rho(\mu). \end{aligned} \tag{11}$$

Here $f(x)$ is an arbitrary function in the Hilbert space $L^2(0,a)$; $\psi(\mu,x)$ is the solution of Eq. (9) with initial data $\psi(\mu,0) = 1$, $\psi'(\mu,0) = h$ (h is real!); $\rho(\mu)$ is a nondecreasing function called the *spectral function* of the operator; and the integrals converge in the metrics of $L^2(0,a)$ and $L^2(\mathrm{d}\rho)$ respectively.

It was discovered that the spectral functions determine the potentials uniquely. This maximally general uniqueness theorem also turned out to be a simple consequence of the existence of Volterra transformation operators. Indeed, suppose the spectral functions $\rho(\mu)$ and $\rho_1(\mu)$ of two operators L and L_1 on the interval $0 \leqslant x \leqslant a$ ($a \leqslant \infty$) coincide. Applying to Eq. (11) the transformation operator V that maps the eigenfunctions $\psi(\mu,x)$ of L into the eigenfunctions $\psi_1(\mu,x)$ of L_1, we obtain

$$\begin{aligned} V[f] &= V\left[\int_{-\infty}^{\infty} F(\mu)\psi(\mu,x)\,\mathrm{d}\rho(\mu)\right] \\ &= \int_{-\infty}^{\infty} F(\mu)\psi_1(\mu,x)\,\mathrm{d}\rho(\mu) = \int_{-\infty}^{\infty} F(\mu)\psi_1(\mu,x)\,\mathrm{d}\rho_1(\mu) \end{aligned}$$

and, by Parseval's equality

$$\|f\|^2 = \int_{-\infty}^{\infty} |F(\mu)|^2\,\mathrm{d}\rho(\mu) = \int_{-\infty}^{\infty} |F(\mu)|^2\,\mathrm{d}\rho_1(\mu) = \left\|V[f]\right\|^2,$$

from which it follows that the operator V is unitary: $V^* = V^{-1}$. But the identity operator is the only Volterra operator that is unitary. Indeed, it follows from (5) and (6) that

$$V^* = I + A, \qquad V^{-1} = I + B,$$

where the integral operators A and B have kernels

$$A(x,t) = \begin{cases} \overline{K(t,x)}, & t > x, \\ 0, & t < x, \end{cases} \qquad B(x,t) = \begin{cases} 0, & t > x, \\ L(x,t), & t < x, \end{cases}$$

and if $V^* = V^{-1}$, then $A(x,t) = B(x,t) = 0$; that is, $V = I$. Thus equality of the spectral functions implies that the eigenfunctions $\psi(\mu,x)$ and $\psi_1(\mu,x)$ are equal, and hence so are the potentials: $q(x) = q_1(x)$.

This uniqueness theorem made it possible to prove that in various particular cases the potential is also uniquely determined by other spectral data, in which the spectral function does not appear explicitly. For example, if the operators generated by (10) and the boundary conditions at zero have discrete spectra, then the potential is uniquely determined by the two spectra (in both the case of a finite interval and the case of an semi-infinite interval).

In the second half of the 1940s, the so-called inverse scattering problem was of great interest (especially for physicists). This problem involves reconstructing a potential from quantities obtained in experiments on the scattering of particles by the required potential. Interest arose in this problem not only because of a desire to find the potentials in various specific cases, but also to find the answer to the fundamental question whether the scattering data contain all the physical information. In the case of a spherically symmetric potential, the phase analysis of experimental data makes it possible theoretically to find the limiting phases that describe the asymptotic behavior of radial wave functions as $r \to \infty$. At the moment equal to zero the radial wave functions can be represented in the form $R(k,r) = r^{-1}\psi(k,r)$, where $\psi(k,r)$ are solutions of the equation

$$-\psi_{rr} + q(r) = k^2\psi \qquad (0 \leqslant r < \infty) \tag{12}$$

that are bounded at infinity, with initial data $\psi(k,0) = 0$, $\psi'(k,0) = 1$. If the potential $q(r)$ satisfies condition (8), the function $\psi(k,r)$ has the following asymptotics as $r \to \infty$:

$$\psi(k,r) = k^{-1}|M(k)|\big[\sin(kr + \eta(k)) + o(1)\big] \qquad (0 < k < \infty), \tag{13}$$

where the function

$$M(z) = 1 + \int_0^\infty \mathrm{e}^{-\mathrm{i}zt} q(t)\,\psi(z,t)\,\mathrm{d}t,$$

which is holomorphic in the lower half-plane, tends to 1 as $|z| \to \infty$ ($\Im z \leqslant 0$), and the limiting phase $\eta(k)$ equals the argument of this function on the real line:

$$\eta(k) = -\eta(-k) = \arg M(k) \qquad (0 < k < \infty).$$

In this case, the inverse scattering problem is to recover the potential $q(r)$ from the limiting phase $\eta(k)$. In 1949, N. Levinson proved that in the absence of a discrete spectrum the limiting phase does indeed determine the potential uniquely. But somewhat earlier V. Bargmann had shown that when there is a discrete spectrum the whole set of eigenvalues together with the limiting phase still does not determine the potential uniquely.

The general uniqueness theorem clarified this situation. The point is that the spectral function of an operator with potential satisfying condition (8) is absolutely continuous on the positive half-line and connected with the function $M(z)$ by the relation

$$\mathrm{d}\rho(\mu) = \frac{1}{\pi}\frac{\sqrt{\mu}}{|M(\sqrt{\mu})|^2}\,\mathrm{d}\mu \qquad (\mu > 0). \tag{14}$$

The function $M(z)$ has only a finite number of zeros in the lower half-plane, and all of them lie on the imaginary half-line, so that their squares are equal to the negative eigenvalues $\mu_l < 0$. This makes it possible to recover the function $M(z)$ uniquely using the Poisson–Schwarz formula

$$M(z) = \prod_l \left(\frac{z - \mathrm{i}k_l}{z + \mathrm{i}k_l}\right) \exp\left\{\frac{1}{\pi}\int_{-\infty}^{\infty} \frac{\tilde{\eta}(k)}{k - z}\,\mathrm{d}k\right\},$$

where $\mathrm{i}k_l = \sqrt{\mu_l}$ and

$$\tilde{\eta}(k) = \eta(k) - \sum_l \arg\left(\frac{k - \mathrm{i}k_l}{k + \mathrm{i}k_l}\right).$$

Consequently, the function $M(z)$ can be recovered on the entire lower half-plane from the limiting phase $\eta(k) = \arg M(k)$ and the discrete spectrum. But, according to (14), the spectral function can be recovered from these data only on the positive half-line. On the negative half-line the spectral function is piecewise constant and has jumps at the points of the discrete spectrum. For that reason, to recover the spectral function on the entire real line one needs to know, in addition to the limiting phase and the discrete spectrum, the size of its jumps

$$M_l^2 = \left(\int_0^{\infty} |\psi(\mathrm{i}k_l, x)|^2\,\mathrm{d}x\right)^{-1}$$

at the points of the discrete spectrum.

Thus the potential is uniquely determined by the set of quantities $\{\eta(k) : \mu_l, M_l \ (l = 1, 2, \ldots, n)\}$, which determine the asymptotic behavior (as $r \to +\infty$) of the normalized eigenfunctions of the continuous spectrum and the values of the derivatives at zero of the normalized eigenvalues of the discrete spectrum.

These uniqueness theorems were obtained by Borg and myself, independently of each other; in particular, Borg did not use transformation operators. I remark that transformation operators also made it possible to answer a question of Akhiezer: *Can the spectral function be bounded*? It turned out that as $\mu \to -\infty$ the spectral functions tend to finite limits with superexponential rapidity, while as $\mu \to +\infty$ they satisfy the asymptotic equality

$$\rho(\mu) \sim \frac{1}{\pi}\sqrt{\mu} \qquad \left(\rho(\mu) \sim \frac{2}{3\pi}\sqrt{\mu^3}\right),$$

if the expansion (11) is over solutions normalized by the condition $\psi(k,0) = 1$, $\psi'(k,0) = h$ ($\psi(k,0) = 0$, $\psi'(k,0) = 1$).

The main problems in the actual recovery of a potential from some spectral data, and the search for characteristic properties of spectral data, remained unsolved. However, soon after the publication of the uniqueness theorem, M. G. Krein solved the problem of recovering a potential from two spectra. He did not use transformation operators and instead started from the analogy of this inverse problem with the power moment problem. He described this fruitful idea as follows:

M. G. Krein

> Just as some power moment problem corresponds to every Jacobi matrix J, and the matrix J itself is completely determined by any solution of that problem (a mass distribution function), some generalized moment problem corresponds to a second-order differential operator L with a boundary condition at one endpoint, and the operator itself (if presented in some "canonical" form) can be determined from any distribution function of the moment problem, together with the boundary condition. For operators of "sufficiently regular type" this generalized moment problem is the problem of continuation of Hermitian-positive functions developed by the author.

Starting from this analogy, and given two alternating sequences of positive numbers (the spectra of two boundary-value problems), Krein found the spectral function $\rho(\mu)$ of the corresponding operator and introduced the positive definite function

$$F(t)=\int_0^\infty \frac{\cos\sqrt{\mu}t}{\mu}\,\mathrm{d}\rho(\mu),$$

which is a continual analog of the positive definite sequence in the moment problem. He then defined the *central mass* $M(x)$ of the function $F(t)$ as the largest number r for which the function $F(t)-r$ remains positive definite on the interval $(0,x)$. To compute the central mass one may use the equality

$$\big(M(x)\big)^{-1}=\sum_{j=1}^{\infty}\nu_j\left(\int_0^x \chi_j(t)\,\mathrm{d}t\right)^2,$$

where ν_j and $\chi_j(t)$ are the eigenvalues and orthonormalized eigenfunctions of the operator

$$\widehat{F}[f]=\int_0^x F(y-t)f(t)\,\mathrm{d}t \qquad (0\leqslant y\leqslant x).$$

The potential is expressed in terms of the central mass by the formula

$$q(x)=\frac{\varphi''(x)}{\varphi(x)};\qquad \varphi(x)=\left(-\frac{\mathrm{d}M(2x)}{\mathrm{d}x}\right)^{-\frac{1}{2}}.$$

All these operations can be carried out starting from arbitrary alternating sequences if the resulting function $M(x)$ happens to be absolutely continuous along with its first two derivatives, and the first derivative is nonzero. The question whether these conditions are sufficient, in order for the resulting equation to generate boundary-value problems whose spectra coincide with the given sequences, remained open.

Thus, the proposed method made it possible to recover a potential from two sequences, provided it is known *a priori* that they are the spectra of two boundary-value problems generated by the same equation on a finite interval.

Krein's paper appeared in 1951, and in the same year I. M. Gel'fand and B. M. Levitan obtained a complete solution of the inverse problem of recovering a potential from the spectral function $\rho(\mu)$. They also started from the analogy with the theory of orthogonal polynomials and made extensive use of transformation operators, interpreting their action as the continual analog of the Gram–Schmidt orthogonalization procedure; that is, assuming that the solution

$$\psi(\mu,x)=\cos\sqrt{\mu}x+\int_0^x K(x,t)\cos\sqrt{\mu}t\,\mathrm{d}t=V[\cos\sqrt{\mu}t] \tag{15}$$

is obtained by orthogonalizing the system of functions $\cos\sqrt{\mu}t$ $(0\leqslant t\leqslant x)$ with respect to the measure $\mathrm{d}\rho(\mu)$. The function $\psi(\mu,x)$ is indeed orthogonal (with

I. M. Gel'fand

B. M. Levitan

respect to the measure $\mathrm{d}\rho(\mu)$) to the functions $\cos\sqrt{\mu}y$ for all positive $y < x$, as follows from the fact that *the inverse operator* V^{-1} *is a Volterra operator*; that is, it is a consequence of the equality

$$\cos\sqrt{\mu}y = \psi(\mu,y) + \int_0^\infty L(y,t)\,\psi(\mu,t)\,\mathrm{d}t,$$

where $L(y,t) = 0$ for $t > y$. It follows from this equality and the expansion formula (11) that

$$\begin{aligned}&\int_{-\infty}^{\infty} \psi(\mu,x)\cos\sqrt{\mu}y\,\mathrm{d}\rho(\mu)\\ &= \int_{-\infty}^{\infty} \psi(\mu,x)\left\{\psi(\mu,y) + \int_0^\infty L(y,t)\,\psi(\mu,t)\,\mathrm{d}t\right\}\mathrm{d}\rho(\mu) = \delta(x-y) + L(y,x).\end{aligned}$$

Also, since $L(y,x) = 0$ for $y < x$, it follows that

$$\int_{-\infty}^{\infty} \psi(\mu,x)\cos\sqrt{\mu}y\,\mathrm{d}\rho(\mu) = 0 \qquad (0 \leqslant y < x). \tag{16}$$

This orthogonality relation plays a key role in the Gel'fand–Levitan method. As is known, the function

$$\rho_0(\mu) = \begin{cases} \frac{2}{\pi}\sqrt{\mu}, & \mu \geqslant 0,\\ 0, & \mu < 0 \end{cases}$$

is the spectral function of the operator (10), for which $h = q(x) \equiv 0$ and the eigenfunctions are $\cos\sqrt{\mu}x$. Substituting the right-hand side of (15) in place of

$\psi(\mu,x)$ in the orthogonality relation leads to the equality

$$\begin{aligned}0 &= \int_{-\infty}^{\infty}\left(\cos\sqrt{\mu}x+\int_0^x K(x,t)\cos\sqrt{\mu}t\,\mathrm{d}t\right)\cos\sqrt{\mu}y\,\mathrm{d}\rho_0(\mu)\\ &\quad+\int_{-\infty}^{\infty}\left(\cos\sqrt{\mu}x+\int_0^x K(x,t)\cos\sqrt{\mu}t\,\mathrm{d}t\right)\cos\sqrt{\mu}y\,\mathrm{d}\big(\rho(\mu)-\rho_0(\mu)\big)\\ &= \delta(x-y)+K(x,y)+f(x,y)+\int_0^x K(x,t)f(t,y)\,\mathrm{d}t,\end{aligned}$$

where

$$f(x,y)=\int_{-\infty}^{\infty}\cos\sqrt{\mu}x\,\cos\sqrt{\mu}y\,\mathrm{d}\big(\rho(\mu)-\rho_0(\mu)\big).$$

Consequently, for each fixed value of x the kernel $K(x,y)$ of the transformation operator satisfies the integral equation

$$K(x,y)+\int_0^x f(t,y)K(x,t)\,\mathrm{d}t=-f(x,t)\qquad(0\leqslant y\leqslant x)\tag{17}$$

with a real symmetric kernel $f(t,y)$ and right-hand side $-f(x,y)$, which can be expressed explicitly in terms of the spectral function $\rho(\lambda)$.

Equation (17) is the famous Gel'fand–Levitan equation which enabled them to solve the inverse problem of recovering a potential from the spectral function, and also to find necessary and sufficient conditions (with a small gap) for a nondecreasing function $\rho(\mu)$ in order to be the spectral function of some operator (10) defined on an infinite or finite half-open interval.

The kernel $f(x,y)$ of the integral operator in the Gel'fand–Levitan equation can also be expressed in terms of the function

$$F(t)=\int_{-\infty}^{\infty}\frac{1-\cos\sqrt{\mu}x}{\mu}\,\mathrm{d}\rho(\mu)\tag{18}$$

by means of the formula

$$f(x,y)=\frac{1}{2}\big\{F''(x+y)+F''(|x-y|)\big\}.$$

The following theorem is the main result of the paper: *A necessary and sufficient condition for a nondecreasing function $\rho(\mu)$ to be the spectral function of some operator, generated on the half-interval $[0,a)$ $(a\leqslant\infty)$ by the differential operation* (10) *and the boundary condition $y'(0)=hy(0)$, is that the function* (18) *is differentiable three times on the interval* $(0,2a)$ *and that $F(+0)=0$, $F'(+0)=1$, and $F''(+0)=-h$. When these conditions hold, Eqs.* (17) *have a* *unique solution for every $x \in [0,a)$, and the potential is given in terms of the solution $K(x,y)$ by the formula*

$$q(x) = 2\frac{\mathrm{d}}{\mathrm{d}x}K(x,x).$$

Here the potential $q(x)$ has as many derivatives as $F'''(x)$ has.

The original, rather lengthy proof of sufficiency was greatly simplified by Levinson, and the one-derivative gap between the necessary and sufficient conditions was removed by Krein, who had already introduced the function (15) — which differs only trivially from (18) — in his first paper. The Gel'fand–Levitan method became a model for solving other inverse problems, and the results that they obtained contained the answers to a number of fundamental problems. For example, it follows immediately from their resuts that a Sturm–Liouville operator defined on a half-line may have a spectrum of any type, and the spectrum of the classical Sturm–Liouville boundary-value problem with separated boundary conditions can be any sequence satisfying the well-known asymptotic formulas. Borg's problem of recovering a potential from two spectra can be easily reduced to recovering a potential from the spectral function, and this fact enabled Levitan and Gasymov to solve this problem, and find necessary and sufficient conditions which two sequences must satisfy in order to be the spectra of two boundary-value problems generated by the same Sturm–Liouville operator.

The Krein and Gel'fand–Levitan methods closely resemble each other, but they are not identical. In developing his method, Krein constructed a theory of direct and inverse problems of spectral analysis for a very large class of operators, which are far-ranging generalizations of the classical Sturm–Liouville operators. For example, this class contains operators of the form $\frac{\mathrm{d}}{\mathrm{d}M(x)} \cdot \frac{\mathrm{d}}{\mathrm{d}x}$, where $M(x)$ ($0 \leqslant x < a$, $a \leqslant \infty$) is an arbitrary nondecreasing function, which may, in particular, have intervals of constancy. The problem of transverse oscillations of an arbitrary string, supported by an elastic base, reduces to it, and here $M(x)$ is the mass of the portion of the string in the half-open interval $[0,a)$. For this class of operators, Krein proved the following most general theorem:

A necessary and sufficient condition for a nondecreasing function $\rho(\mu)$ to be the spectral function of some operator is that it satisfies the inequality

$$\int_0^\infty \frac{\mathrm{d}\rho(\mu)}{1+\mu} < \infty,$$

and the operator (that is, the function $M(x)$) can be recovered uniquely from it.

The general, rather complicated algorithm for recovering the operator becomes quite effective in many cases.

Of course, the Gel'fand–Levitan equation also makes it possible to solve the inverse problem of scattering theory, since one can find the spectral function from a knowledge of the set $\{\eta(k) : \mu_l, M_l\}$. But in order to do this, one must use the Poisson–Schwarz formula, which is very sensitive to variations of the limiting phase. Moreover, the Gel'fand–Levitan equation is poorly adapted for analyzing the behavior of the resulting potential as $x \to \infty$. I came to a better realization of the difficulties that arise after becoming acquainted with the dissertation of Neigauz, when I was appointed to be an official opponent at her thesis defense. To simplify the problem, I assumed that the potential not only tends to zero as $x \to \infty$, but that it is actually equal to zero for $x > N$. In this case the asymptotic equality (13) becomes exact for $x \geqslant N$, and the functions on the two sides have the same initial data at the point N. Therefore, by mentally transferring the origin to N, one can use a transformation operator of the form (7), which preserves the initial data. As a result, instead of an asymptotic relation, one obtains the exact equality

$$k|M(k)|^{-1}\psi(k,x) = \sin(kx+\eta(k)) + \int_x^{2N-x} A(N,x,t)\sin(kt+\eta(k))\,\mathrm{d}t,$$

in which $A(N,x,t) = 0$ for $x > N$. Similarly, for the normalized eigenfunctions of the discrete spectrum, one obtains the equalities

$$M_l\psi(\mathrm{i}k_l,x) = m_l\left(\mathrm{e}^{-k_l x} + \int_x^{2N-x} A(N,xt)\mathrm{e}^{-k_l t}\,\mathrm{d}t\right).$$

Since the functions

$$u(k,x) = k|M(k)|^{-1}\psi(k,x), \qquad u(k_l,x) = M_l\psi(\mathrm{i}k_l,x)$$

form a complete set of orthonormalized eigenfunctions, according to the main idea of Gel'fand and Levitan, these equalities can be connected with the orthogonalization procedure for the functions

$$\varphi(k,x) = \sin(kx+\eta(k)), \qquad \varphi(k_l,x) = m_l\mathrm{e}^{-k_l x}, \tag{19}$$

and one can prove the analog of the key orthogonality relation (16), namely

$$\frac{1}{\pi}\int_0^\infty u(k,x)\varphi(k,y)\,\mathrm{d}k + \sum_l u(k_l,x)\varphi(k_l,y) = 0 \qquad (0 \leqslant x < y < \infty). \tag{20}$$

B. Ya. Levin

Using this orthogonality relation, one can obtain a linear equation for the kernel $A(N,x,t)$ analogous to the Gel'fand–Levitan equation, and then pass to the limit $N \to \infty$. However, it is not necessary to pass to this limit: B. Ya. Levin informed me that under the condition (8) there exist transformation operators of the form

$$V[f] = f(x) + \int_x^\infty A(x,t) f(t)\,\mathrm{d}t,$$

that transform solutions of the equation $-y'' = k^2 y$, which are bounded on the half-line $(0,\infty)$, into solutions of Eq. (12) while preserving their asymptotic behavior as $x \to +\infty$. By virtue of this theorem of Levin,

$$u(k,x) = \varphi(k,x) + \int_x^\infty A(x,t)\varphi(k,t)\,\mathrm{d}t \qquad (0 < k < \infty,\, k = k_l),$$

and for $y > x$, according to (20),

$$\begin{aligned} 0 &= \frac{2}{\pi}\int_0^\infty \left\{ \varphi(k,x) + \int_x^\infty A(x,t)\varphi(k,t)\,\mathrm{d}t \right\} \varphi(k,y)\,\mathrm{d}k \\ &\quad + \sum_l \left\{ \varphi(k_l,x) + \int_x^\infty A(x,t)\varphi(k_l,t)\,\mathrm{d}t \right\} \varphi(k_l,y) \\ &= f(x,y) + \int_x^\infty A(x,t) f(t,y)\,\mathrm{d}y, \end{aligned}$$

where

$$f(x,y) = \frac{2}{\pi}\int_0^\infty \{\varphi(k,x)\varphi(k,y) - \sin kx \sin ky\}\,\mathrm{d}k + \sum_l \varphi(k_l,x)\varphi(k_l,y),$$

or, according to (19),

$$f(x,y) = F(x+y) = \frac{1}{2\pi}\int_{-\infty}^\infty \left(1 - \mathrm{e}^{2\mathrm{i}\eta(k)}\right)\mathrm{e}^{\mathrm{i}k(x+y)}\,\mathrm{d}k + \sum_l m_l^2 \mathrm{e}^{k_l(x+y)}.$$

Thus, for each value of $x \geqslant 0$ the kernel $A(x,y)$ satisfies the integral equation

$$A(x,y) + \int_x^\infty F(t+y) A(x,t)\,\mathrm{d}t = -F(x+y), \tag{21}$$

whose right-hand side and kernel can be expressed in terms of the scattering data $\{\eta(k) = -\eta(-k) : k_l, m_l \;\; (l = 1,2,\ldots,n)\}$ that determine the asymptotics of the normalized eigenfunctions as $x \to +\infty$. Analysis of Eq. (21) made it possible to prove the following theorem.

The necessary and sufficient conditions for the set

$$\{\eta(k) = -\eta(-k) : k_l, m_l \ \ (l = 1, 2, \ldots, n)\}$$

to be the scattering data by a real potential $q(x)$ *which satisfies the inequality*

$$\int_0^\infty x|q(x)|\,\mathrm{d}x < \infty,$$

are the following:

1. *The function*

$$F_s(t) = \frac{1}{2\pi}\int_{-\infty}^\infty \big(1 - \mathrm{e}^{2\mathrm{i}\eta(k)}\big)\mathrm{e}^{\mathrm{i}kt}\,\mathrm{d}k$$

belongs to $L^1(-\infty,\infty)$*, is differentiable on the positive half-line, and*

$$\int_0^\infty t|F_s'(t)|\,\mathrm{d}t < \infty.$$

2. *Levinson's equality holds*:

$$n = \frac{\eta(+0) - \eta(+\infty)}{\pi} - \frac{\sin^2\big(\eta(+0) - \eta(+\infty)\big)}{2}.$$

When these conditions hold, Eqs. (21) *have a unique solution for all* $x \geqslant 0$*, and the potential can be expressed in terms of their solutions by means of the formula*

$$q(x) = -2\frac{\mathrm{d}}{\mathrm{d}x}A(x,x).$$

Equations (17) and (21) were soon generalized to operators with matrix-valued potentials.

At this point, the first stage in the study of inverse problems is finished. The subsequent development proceeded mainly along the following lines.

The first line involves inverse problems for partial differential operators and particularly the inverse scattering problem for the three-dimensional Schrödinger operator with an arbitrary potential which decreases sufficiently rapidly. This problem was solved by L. D. Faddeev and R. Newton, and the decisive role here was played by the multidimensional analog of Volterra transformation operators discovered by Faddeev.

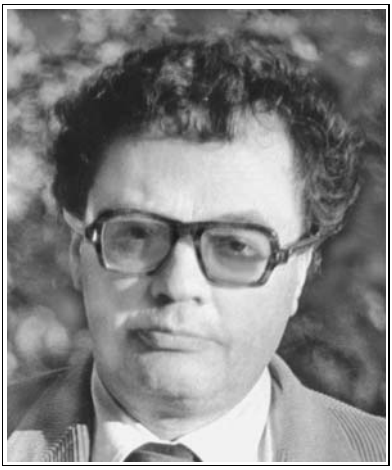
The second line involves the surprising discovery in 1967 by Gardner, Greene, Kruskal, and Miura of a connection between the inverse scattering problem and the Korteweg – de Vries equation, which enabled these authors to solve the Cauchy problem for this nonlinear equation. This paper laid the foundation for a new area of mathematical physics, making it possible to solve a number of important nonlinear equations, which are of importance for physics, by the method now known as the inverse problem method. The many-sided development of this area has been the subject of a huge number of papers, leading to significant progress on inverse problems.

However, certain problems still require solution. For example:

1. How can one tell from the spectral function whether or not a potential is bounded?

2. What restriction does the condition of orthogonality impose on the *local* structure of spectral functions of operators defined on a *finite* half-open interval $[0,a)$ $(a<\infty)$? (According to one of Krein's theorems, the spectral function $\rho(\mu)$ is orthogonal if and only if the linear span of the functions $(\sin\sqrt{\mu}t)/\sqrt{\mu}$ $(0<t<a)$ is dense in $L^2\big(\mathrm{d}\rho(\mu)\big)$.)

3. The spectral functions of operators defined on the entire real line are matrix valued. Which nondecreasing matrix-valued functions are spectral?



V. P. Maslov

Mathematics and the Trajectories of Typhoons

Translated by R. Cooke

1. Introduction

Strong discontinuities of solutions and their propagation have formed the basis of the classification of linear hyperbolic equations that is presented in textbooks of mathematical physics and partial differential equations, for example, in the classical texts of Courant and Friedrichs, Petrovskii, and others. After the appearance and development of the theory of distributions and generalized functions, it became possible to describe discontinuous solutions without using integral relations ("integral conservation laws"), as had been done earlier, using instead direct substitution of generalized functions into the original equation and constructing the "asymptotics with respect to smoothness." This procedure makes it possible to reduce the problem of finding a nonsmooth solution to an integral equation now with smooth corrections to its principal ("nonsmooth") part. Such a concept was developed successfully for elliptic and hypoelliptic equations by L. Hörmander and his school. For hyperbolic equations such an asymptotics was first proposed by P. Lax. Subsequently, the global asymptotics was obtained using a canonical operator in papers by the present author, Hörmander, and others.

It was natural to try to apply such methods to quasi-linear hyperbolic equations also, as the author did in [1], [2]. The well-known Hugoniot conditions in the theory of shock waves were obtained directly by assuming sufficient smoothness in the discrepancy that results from substituting a nonsmooth solution into the original equation, rather than from energy-integral considerations. Such an approach makes it relatively easy to write down an infinite chain of differential equations, the first of which coincides with the Hugoniot condition. The possibility of getting strongly discontinuous solutions — shock waves — for quasi-linear equations results from the fact that the "smoothness" series of distributions, in which the first term is the Heaviside theta-function, is an *algebra of generalized functions*. This means that the square, cube, and so on, of such functions can be represented by the same kinds of series. The question naturally arises as to what other algebras of generalized functions are generated by the "smoothness" asymptotics of solutions of quasi-linear hyperbolic equations and systems of equations in "general position." It was discovered [1] that there are *a total of three* such algebras when the discontinuity occurs on a hypersurface. One of them corresponds to shock waves, another to detonation waves, and the third to "limiting bell-shaped" waves or "narrow solitons," which result from passing to the limit in solutions of equations with vanishingly small dispersion. In parallel and independently, though slightly later, a group of mathematicians headed by J. Colombeau (see [3], and also [4] and [5]) began to study algebras of generalized functions from the point of view of applications to nonlinear equations.

In the case of point singularities, heuristic considerations showed that there can be only one algebra, corresponding to isolated vortices. Chains of ordinary differential equations resembling Hugoniot conditions and corrections to them can be associated with point singularities, just as with shock waves. The author delegated to his student V. Zhikharev the task of bringing some order into these heuristic considerations; but unfortunately only under certain additional hypotheses. A proof free of such hypotheses has recently been obtained by S. Dobrokhotov and his students [6]. In the course of the proof it turned out ([7], [8]) that, in a wonderful and completely surprising way, very complicated "vortex" chains have exceptionally simple and beautiful solutions.

S. Yu. Dobrokhotov

The author has ascribed, and continues to ascribe, great importance to point (vortex) singularities. I concluded my plenary survey address at the International Congress of Mathematicians in Warsaw in 1983 with the following conjecture: *It is precisely the point singularities generated by this algebra that correspond to such natural* *catastrophic phenomena as typhoons*; *and the trajectories of singularities defined by chains of equations of Hugoniot condition type and corrections to them may describe the trajectories of actual typhoons.* The fact that the trajectories of singularities and the trajectories of a number of actual typhoons are in rather good qualitative agreement [7] is very significant. During a visit to Moscow the great theoretical physicist Paul Dirac said that a theoretician obtains his highest pleasure when his results agree with experiment. In this sense Dobrokhotov's results exceeded my most optimistic predictions: The elegance and beauty that resulted in the solution of the chains mentioned above cannot help but be the result of physical phenomena. I am almost convinced that full confirmation of my conjecture is no longer beyond the horizon.

The present article is devoted to this conjecture and the remarkable facts discovered by Dobrokhotov.

2. Singular Solutions of Quasi-Linear Hyperbolic Systems

Infinite chains of differential equations are well known in continuum mechanics and mathematical physics: for example, the BBGKY (Bogolyubov–Born–Green–Kirkwood–Yvon) chains in statistical physics, moment chains in statistical hydromechanics, Toda chains in soliton theory, and so on. The diverse problems connected with them, such as their integration, closure, quantization, and so on, are also well known. For example, the procedure of forming the closure of BBGKY chains leads to the Boltzmann kinetic equation.

As noted, more than 20 years ago the author [1] discovered that certain previously unknown chains of ordinary differential equations arise of necessity in problems involving the description of singular solutions of quasi-linear hyperbolic systems. In accordance with the concept developed in [1], despite the different physical objects that these chains describe — we repeat, that those include shock waves, "infinitely narrow" solitons, and isolated vortices — such solutions and their description have much in common from the mathematical point of view, including the appearance of chains. Moreover, a large number of quasi-linear hyperbolic systems admit only singularities of the types listed above, provided we assume additionally that they preserve structure and are in "general position." Such a selection of singularities results from the presence of nonlinear terms: for linear hyperbolic systems, as is known, the solutions inherit every type of singularity from the original condition, at least locally. For shock waves, the first equation of the corresponding chains is the well known Hugoniot condition. The study of such chains in some detail for shock waves, the equations of gas dynamics, and solitons was carried out some time ago ([2], [9]). From the point of view of application to particular problems, however, such studies cannot be considered finished. For vortex (point) singularities the analogous chains had hardly been studied at all until recently.

In fluid dynamics, plasma physics, and atmospheric physics, in particular, in describing the motion (but not the formation and decay) of tropical cyclones (typhoons and hurricanes [1] — intermediate-scale planetary vortices) a system of "shallow water" equations is often used as a rather crude, but very important and universal two-dimensional dispersion-free nonviscous approximation, taking account of the rotation of the Earth and the dependence of the Coriolis force on latitude, in the so-called β-plane approximation (see [10] and [11], [12]):

$$\frac{\partial \eta}{\partial t} + \nabla \cdot (\eta \mathbf{u}) = 0, \quad \frac{\partial \mathbf{u}}{\partial t} + (\mathbf{u}, \nabla)\mathbf{u} - \omega \mathscr{T} \mathbf{u} + \nabla \eta = 0. \tag{1}$$

Here $x = (x_1, x_2) \in \mathbb{R}^2$, $\mathbf{u}$ is the two-dimensional velocity vector, $\eta > 0$ is the geopotential, $\mathscr{T} = \left(\begin{smallmatrix} 0 & 1 \\ -1 & 0 \end{smallmatrix}\right)$ is the 90°-rotation matrix, $\nabla = \big(\partial/\partial x_1, \partial/\partial x_2\big)$, $\omega/2$ is the Coriolis frequency on the β-plane: $\omega = \widetilde{\omega} + \beta x_2$, and $\widetilde{\omega}$ and β are parameters (physical constants), where β turns out to be very small in typhoon-trajectory problems.

The study of the chains that describe the propagation of (weak point) vortex singularities for this system, which was first carried out in papers of V. Zhikharev, V. Bulatov, Yu. Vladimirov, V. Danilov, and S. Dobrokhotov [13] and mainly in the subsequent cycle of papers of Dobrokhotov (see [7] and [8]), led Dobrokhotov to completely surprising, irregular, and exceptionally curious results of "integrability" type, which in the end yielded the description of sufficiently smooth trajectories of vortices using a family of (linear) Hill equations and square-integrable second-order systems that nearly coincided with equations of the physical pendulum type. These results, which were subsequently developed and made more precise in joint work with E. Semenov, K. Pankrashkin, and B. Tirozzi (see [6], [14], [15]), turn out to be very important from the practical as well as theoretical point of view. Indeed, if we assume (following specialists

[1] Tropical cyclones are meso-scale planetary vortices having a diameter of the order of 100 km and a conditional altitude of the order of 10 km. They differ from "ordinary" cyclones in that the velocity profile does not vary strongly with altitude over a large interval in their interior. This fact makes it possible to average over the altitude and describe their dynamics using a system of two-dimensional equations of the type used to describe shallow water. Near the center of such vortices their velocities are very small; the vicinity of the center is called the "eye." As a rule, tropical cyclones in the Pacific region are called *typhoons* and those in the Atlantic region *hurricanes*. We shall use on the term *typhoon* below, although everything that is said applies to hurricanes as well.

in atmospheric physics) that the solutions of (1) can describe the propagation of typhoons, and if "structurally self-similar and stable" singular solutions of this system that are nonsmooth at a point correspond to tropical cyclones, then by the uniqueness of such a singularity of general position, the trajectory of the typhoon must be close to the trajectory of the center of a point weak singularity. Thus the equations of "physical pendulum type" so obtained describe approximately the possible trajectories of typhoons. They can be used to recover and predict typhoon trajectories from known portions of them, for example, from satellite observations.

In my opinion, the fact that such familiar mathematical objects as the Hill equations, an equation of physical pendulum type, and so on, arise unexpectedly in the chains for isolated vortices is not accidental and goes beyond the "shallow water" model system. In my view, it raises chains for singular solutions to a new qualitative level and invites us to look at them more attentively from different points of view. On the other hand, even if one does not view the results obtained in [7] and [8] with an eye to generalizing them, they seem to me to be a combination of nontrivial mathematical constructions and observations having application to such a practically important problem as the prediction of typhoon trajectories, which is rather rare for basic research.

All the singular solutions noted above can be described by a formula that resembles the "nonlinear WKB" or "Witham" solutions or "distorted" simple Riemannian waves

$$\mathbf{w} = \mathbf{f}(x,t) + \mathbf{g}(x,t), \quad \mathbf{g}(x,t) = g(x,t)F\big(S(x,t)\big), \tag{2}$$

where $\mathbf{w}$ is a vector- or scalar-valued function, $x \in \mathbb{R}^n$, $F(\tau)$ is some scalar-valued function that is smooth outside the set $\tau = 0$ and has a singularity at $\tau = 0$, and the phase $S(x,t)$, the vector-valued (or scalar-valued) "background" $\mathbf{f}(x,t)$, and the "amplitude" $g(x,t)$ are smooth functions. A singularity may be determined, for example, by a discontinuity of first kind (in which case we have shock waves) or even be a continuous or once-differentiable function, in which case we have weak discontinuities. It is clear that the singularities of $\mathbf{w}(x,t)$ are determined by the zeros X of $S(x,t)$. For example, for shock waves in the one-dimensional scalar case ($n = 1$) we have $F = \Theta(\tau)$, where $\Theta(\tau)$ is the Heaviside[2] function: $\Theta = 0$ for $\tau < 0$ and $\Theta = 1$ for $\tau \geqslant 0$, and $S = x - X(t)$. But for an "infinitely narrow soliton" moving over the background $\mathbf{f}(x,t)$ we still have $S = x - X(t)$, but $F = \mathrm{Sol}(\tau)$, where $\mathrm{Sol} = 0$ for $\tau \neq 0$ and $\mathrm{Sol} = 1$ for $\tau = 0$. From the point of view of the space L_2, the generalized solutions of

[2] Sometimes called the *Dirichlet discontinuous factor* or the *unit step function*. — *Transl.*

second kind are generally equal to the background $\mathbf{f}(x,t)$; but they turn out to be completely reasonable if the scalar product and the space of basic functions are defined correctly; for example, they are the limits of soliton solutions of the Korteweg – de Vries equation with dispersion ε^2 as $\varepsilon \to 0$.

Finally, we present another example in the two-dimensional case ($x = (x_1,x_2) \in \mathbb{R}^2$) that is fundamental for this note and for [7] and [8]. As the function F we choose τ^α, where $\alpha > 0$ is not an integer, and we require that the function $S(x,t)$ be nonnegative, with the equality $S(x,t) = 0$ holding only at the point $x = X(t) \equiv (X_1(t), X_2(t))$. The set $\Gamma = \big(x = X(t),\ t \in [0,T]\big)$ forms the trajectory of the singular solution (2) on $[0,T]$. We also impose the requirement of "general position": the (nonnegative-definite) Hessian matrix $\left\| \frac{\partial^2 S}{\partial x_i \partial x_j} \right\| \Big|_\Gamma = \operatorname{Hess} S\big|_\Gamma$ is nondegenerate (positive-definite) on the trajectory Γ and has distinct eigenvalues. Moreover, we assume that the expansions of the components of the vector g in powers of $x - X(t)$ begin with the minimum-possible powers. For Eq. (1) it turns out that if there exist solutions of (2) and the assumptions imposed on $S(x,t)$ hold at time $t = t_0$, then they hold at later times also. Thus, with respect to x the function S is, up to higher-order terms, a positive-definite quadratic form with distinct eigenvalues and center at the points of the trajectory of the singularity. We have a weak "point" singularity: the function $\mathbf{w}$ itself is continuous; moreover, it equals zero at the singularity, and several of its derivatives are discontinuous. (It is such behavior of $\mathbf{w}$ that provides an argument for applying such singular solutions to model typhoons.)[3]

In the more general situation one may assume that $F(\tau)$ is continuous for all $\tau \geqslant 0$ and smooth for $\tau > 0$, and moreover that $F(0) = 0$ and $F_\tau \to \infty$ as $\tau \to +0$. It is clear that in this case $\mathbf{w}$ has a singularity at $X(t)$ on the trajectory Γ for every t. The solutions of the form (2) are distributions, and so it is clear that the methods of studying them belong to the circle of questions connected with the construction of algebras of generalized functions and their use in nonlinear equations (see [3]–[5]).

The important feature held in common by solutions of the form (2) is the following. First, they are "structurally self-similar." This means that if they had the form (2) with a given $F(\tau)$ at a certain time t_0, this dependence on τ will persist for times $t > t_0$, at least on intervals $t - t_0$ that are not too large. Second, they possess "structural stability." A small change in the initial conditions for the functions $S(x,t_0)$, $\mathbf{f}(x,t_0)$, $g(x,t_0)$ and the coefficients of the original equation does not lead to any change in the structure of the singularity of the solution $\mathbf{w}$, defined by the function $F(\tau)$.

---

[3] As we have noted, near the center of a typhoon the wind velocity is relatively small.

As already stated, for many physically reasonable quasi-linear hyperbolic equations, nearly all possible singularities having these properties belong to one of the structures listed above, and *in the last example*

$$F = \sqrt{\tau}.$$

This does not mean that the corresponding equation has no particular singular solutions different from those given above, for example, solutions that are radially symmetric in $x - X(t)$; but these seem to dissipate rapidly under small perturbations. [4]

Singularities of the latter type in system (1) are the object of our discussion. The corresponding solutions are defined by (2), in which $\vec{\mathbf{w}} = (\vec{\mathbf{u}}, \eta)$, $\mathbf{f} = (u, \rho)$; $\mathbf{g} = (\widetilde{u}, \widetilde{\rho})$, and $\vec{\mathbf{u}}$, u, and $\widetilde{u}$ are two-dimensional vector-valued functions, where in what follows it is convenient to denote the components of u by v and w, setting $u = (v, w)$. The solution (2) then assumes the form

$$\begin{pmatrix} \mathbf{u} \\ \eta \end{pmatrix} = \begin{pmatrix} \rho \\ u \end{pmatrix} + \begin{pmatrix} \tilde{\rho} \\ \tilde{u} \end{pmatrix}, \quad \begin{pmatrix} \tilde{\rho} \\ \tilde{u} \end{pmatrix} = \begin{pmatrix} \tilde{U} \\ \tilde{R} \end{pmatrix} \sqrt{S}. \tag{2'}$$

We immediately note two properties of singular solutions of the form (2′) that are inherent in them, and illustrate, on the one hand, that they are physically reasonable, and on the other hand, the rather surprising mathematical properties of the trajectories of singularities. It turns out that they have a vortex structure, and more: the trajectory $X(t)$ is "frozen into" the velocity field $\mathbf{u}$ (and u),

$$\dot{X}(t) = u\big(X(t), t\big) \equiv \mathbf{u}\big(X(t), t\big) \equiv V(t) \iff \dot{X}_1 = V_1, \ \dot{X}_2 = V_2; \tag{3}$$

and the Cauchy–Riemann equations hold for the complex velocities $\mathbf{v}(x,t) = v(x,t) + \mathrm{i}w(x,t)$ on the trajectory $X(t)$:

$$\frac{\partial v}{\partial x_1} = \frac{\partial w}{\partial x_2}, \quad \frac{\partial v}{\partial x_2} = -\frac{\partial w}{\partial x_1}. \tag{4}$$

This "freezing-in" of the singularity is not surprising — it is well known in fluid dynamics. The fact that it necessarily takes place for solutions of the form (2′) indicates that such a vorticial solution does not contradict the laws of fluid dynamics. (We remark, however, that the structure of the trajectory Γ can be quite complicated.) The requirement that the Cauchy–Riemann equations (4) should hold is curious: no analyticity conditions are assumed *a priori* (the

[4] In general, the invocation of "general position" considerations leads to a situation that is rather paradoxical from the point of view of the accepted approaches in mathematical physics: it would be natural to begin by studying solutions in a particular simple situation, which the radially symmetric case seems to be. However, using only crude considerations one can say considerably less about the trajectories of singularities in this case than in the case of "general position."

complex velocity $\mathbf{v}(x,t)$ is not even analytic in the variables x_1 and x_2), and Eq. (4), like (3), is a *corollary of the existence of the singular solution* (2). We remark that the conditions (4) are not invariant for (all) trajectories of the velocity field; in this elegant manner they describe the *effect of the existence of a vortex*[5] on the "smooth background" $u(x,t)$ and the trajectory.

From now on, it will be convenient to denote the derivatives $\partial v/\partial x_1$ and $\partial w/\partial x_2$ by $q(t)$ and $\partial v/\partial x_2$ and $-\partial w/\partial x_1$ by $p(t)$. We remark that $q(t)$ is half of the divergence of the field u and $p(t)$ is half of the third component of the curl of the field u on the trajectory $X(t)$: $q(t) = \frac{1}{2}\operatorname{div} u\big(x(t),t\big)$, $p(t) = -\frac{1}{2}\operatorname{curl}_3 u\big(x(t),t\big)$.

3. Chains for the Vortex Singularities of the Shallow-Water Equations, their Closure and Reduction to Hill's Equation

As already noted, among the basic observations of [1] was the point that it is possible to describe solutions of the form (2) using infinite-dimensional systems of ordinary differential equations for the coefficients of the Taylor-series expansions of a solution in the vicinity of a discontinuity (chains of conditions of Hugoniot type), which necessarily arise in the construction of the "smoothness" asymptotics of these solutions. This system is nonclosed, in the sense that the first n equations contain more than n variables, and hence solving it does not in general lead to an unambiguous determination of the location of the singularity. Without explaining the meaning of the unknown functions just now, we give the first equations of the "vortex" chain corresponding to a solution ($2'$) with $F = \sqrt{\tau}$ (to which Eq. (3) must be adjoined):

$$
\begin{aligned}
\dot{V}_1 - \omega_0 V_2 + \rho_{10} &= 0,\\
\dot{V}_2 + \omega_0 V_1 + \rho_{01} &= 0,\\
\dot{\rho}_0 + 2q\rho_0 &= 0,\\
\dot{\omega}_0 - \beta V_2 &= 0,\\
\dot{q} - p^2 + q^2 + \omega_0 p + 2r + \beta V_1/2 &= 0,\\
\dot{p} + 2pq - \omega_0 q - \beta V_2/2 &= 0,\\
\dot{\rho}_{10} + 3q\rho_{10} - p\rho_{01} + \rho_0(w_{11} + 2v_{20}) &= 0,\\
\dot{\rho}_{01} + 3q\rho_{01} + p\rho_{10} + \rho_0(v_{11} + 2w_{02}) &= 0,
\end{aligned}
\tag{5}
$$

[5] Together with the equality $2p - \omega\big(X(t)\big) = c\rho\big(X(t)\big)$, they follow from the conservation of the vector field $\mathbf{u}$ and the Rossby (or Ertel) invariant $(\mathbf{u}_{1x_2} - \mathbf{u}_{2x_1} + \omega)/\eta$ along the trajectories.

$$
\begin{gathered}
\dot{r}+4qr+\frac{1}{2}\rho_{10}(3v_{20}+w_{11}+v_{02})+\frac{1}{2}\rho_{01}(v_{11}+3w_{02}+w_{20}) \\
=-\rho_0(3v_{30}+3w_{03}+w_{21}+v_{12}), \\
\dot{v}_{20}+3qv_{20}-\omega_0 w_{20}-p(v_{11}-w_{20})=-3\rho_{30}, \\
\dot{v}_{11}+3qv_{11}-\omega_0 w_{11}-p(2v_{02}-2v_{20}-w_{11})+\beta p=-2\rho_{21}, \\
\dot{v}_{02}+3qv_{02}-\omega_0 w_{02}+p(v_{11}+w_{02})-\beta q=-\rho_{12}, \\
\dot{w}_{20}+3qw_{20}+\omega_0 v_{20}-p(w_{11}+v_{20})=-\rho_{21}, \\
\dot{w}_{11}+3qw_{11}+\omega_0 v_{11}-p(-2w_{20}+2w_{02}+v_{11})+\beta q=-2\rho_{12}, \\
\dot{w}_{02}+3qw_{02}+\omega_0 v_{02}+p(w_{11}-v_{02})+\beta p=-3\rho_{03}.
\end{gathered} \tag{6}
$$

In the system of 17 equations consisting of (3), (5) and (6), all 23 subscripted variables are unknown, so that the system of equations is nonclosed. Nevertheless, the idea of using some finite-dimensional closure of such chains to describe the dynamics of the singularities, on at least some time intervals, is very attractive. Closure is possible if certain global properties of the solution are known or some additional assumptions are imposed (for example, that the amplitude of the discontinuity be small).

For chains like the Hugoniot condition, which arise in describing shock waves of the very simple nonlinear Hopf equation $v_t+vv_x=0$, P. Prasad and R. Ravindran [16] used a method of closure ("truncation") based on setting the "extra" components of a solution with large indices equal to zero; this method yielded very good results. A similar closure procedure has been used in statistical physics and hydromechanics. The same approach was applied in the papers of Dobrokhotov and co-authors to close the system (3), (5), (6) and to describe the trajectories of vorticial singular solutions ($2'$) of Eq. (1).

Naturally, the systems of equations obtained by truncating chains of Hugoniot-type conditions cannot describe precisely the motion of singularities (or the front of singularities) on larger time intervals, since truncating the chains involves the forced localization of the problem and a description of the evolution of a singularity only over some neighborhood of it. However, if such a description is applied for times that are not too large and if the "truncated" chain has certain stability characteristics, it seems quite reasonable[6] to use such "truncated" chains. On the other hand, for example, in the problem of the trajectory of the "eye" of a typhoon it turns out to be rather problematic to obtain data that describe the velocity $\mathbf{u}$ and the geopotential η at the initial time and make

[6] An estimate for the difference between the solutions of truncated and nontruncated chains corresponding to shock waves of Hopf equation type has recently been obtained by V. Danilov and G. Omel'yanov.

possible a well-posed Cauchy problem for the system (1). The difficulty comes from the potentially sharp local variations in the velocity and the impossibility of arranging a dense enough spatial grid for measuring the wind velocity, pressure, and so on. But the trajectory of the "eye" of a typhoon can be rather reliably determined, for example, from satellites, and one may try to predict the future part of a typhoon trajectory from the part that is known by solving the extrapolation problem. To that end, in turn, one may try using the formulas for the trajectories of vortices obtained by integrating the (truncated) chains. Thus, by its physical formulation, localizing the problem is a perfectly reasonable thing to do.

Describing the closure in application to the system (3), (5), (6) means simply replacing the right-hand sides in (6) by zeros. But even after closure, there still remains a system of 17 nonlinear equations, and the analytic study of it appears rather problematic, even when the Coriolis force is constant (that is, $\beta = 0$). It is utterly surprising and remarkable, but clearly not accidental, that in the case $\beta = 0$ these 17 equations reduce (precisely) to a family of second-order linear equations with periodic coefficients — the system of Hill equations that is well known in the theory of nonlinear oscillations and celestial mechanics:

$$\frac{d^2\psi}{d\Phi^2} + \left(\lambda + \frac{1}{c^2}\Re\left((\alpha_1\alpha_2 - \alpha_0\bar{\alpha}_2)e^{i\Phi} + \frac{3}{2}\alpha_0\alpha_1 e^{2i\Phi}\right)\right)\psi = 0. \tag{7}$$

Here the parameters — the complex numbers α_0, α_1, and α_2 and the real numbers $c \neq 0$ and λ — are constants of integration of the "truncated" chain (3), (5), (6), and the bar denotes complex conjugation. We shall discuss the reduction of the "truncated" chain to Eq. (7) a little later, but right now we wish to point out one remarkable fact.

The derivation of (7) involved the introduction of new dependent variables in which the system (3), (5), (6) assumed a simpler form from the point of view of qualitative analysis of it. This made it possible to distinguish among the 17-parameter family of its solutions all the solutions that possess the following properties, which are natural from the point of view of application to the problem of typhoon trajectories. First, the geopotential η does not vary strongly over the lifetime of a typhoon; second, the trajectories of singularities (vortices) $X(t)$ are rather smooth on the same time scale — that is, they do not have large loops. Such a (mathematical) sorting has led to 6-parameter families of asymptotic solutions of the system (3), (5), (6), in which the solutions from this family can be found as the solutions of Hamiltonian systems with one degree of freedom that are solvable in quadratures (!) and are quantitatively and qualitatively close to the equation of a physical pendulum. Moreover, although the derivation of this system relied on purely mathematical considerations, it was obtained in such a way that the six parameters that characterize these solutions turned out to be perfectly reasonable physically.

Thus the study of a mathematical object — solutions of Eq. (1) of the form (2) with certain mathematical but physically reasonable properties — relying on absolutely nontrivial computations, unexpectedly led to a wonderfully simple result which has a clear physical meaning and important potential applications. We shall discuss all this in somewhat more detail below.

4. The Nonsmooth Part of the Solution

The following proposition describes the nonsmooth part of the solution (2′) of Eq. (1). (It was first stated by Maslov and Zhikharev in a slightly different form for the case $\omega = 0$). We use a dot to denote the derivative with respect to time and

$$G_{m_1 m_2} = \frac{1}{m_1!\,m_2!}\frac{\partial^{(m_1+m_2)}}{\partial x^{m_1}\partial t^{m_2}} G\big(X(t),t\big)$$

to denote the Taylor coefficients of smooth scalar and vector functions $G(x,t)$. We shall write $\rho_0(t)$, $\omega_0(t)$, and $V(t)$ instead of ρ_{00}, ω_{00}, and u_{00}.

Proposition 1. 1. *If the system* (1) *has a solution of the form* (2′) *satisfying the conditions stated above, then in addition to* (3) *and* (4) *we have*

$$\rho_{20} = \rho_{02} + \beta V_1/2, \quad \rho_{11} = \beta V_2/2.$$

The equality

$$\begin{pmatrix} \widetilde{u} \\ \widetilde{\rho} \end{pmatrix} = A\sqrt{S^{(2)} + O(|x-X(t)|^3)} \begin{pmatrix} \mathbf{T}\nabla S^{(2)} + O(|x-X(t)|^2) \\ 2c\rho_0 S^{(2)} + O(|x-X(t)|^3) \end{pmatrix}, \tag{8}$$

holds for the functions $\widetilde{\rho} = R\sqrt{S}$ *and* $\widetilde{u} = U\sqrt{S}$, *where*

$$S^{(2)} = \frac{1}{2}\rho_0(t)(x-X(t), \Pi(t)B\Pi^*(t)(x-X(t)), \quad B = \begin{pmatrix} b_1 & 0 \\ 0 & b_2 \end{pmatrix},$$

$$\Pi = \begin{pmatrix} \cos\theta & \sin\theta \\ -\sin\theta & \cos\theta \end{pmatrix}$$ *is the matrix of a rotation through angle*

$$\theta(t) = \theta_0 + \int_0^t p(t)\,\mathrm{d}t,$$

and $b_1 > 0$, $b_2 > 0$, $b_1 \neq b_2$, θ_0, $A \neq 0$, $c \neq 0$ *are real constants that characterize the initial structure of the vortex solution.*

For the derivatives ρ_{lj}*,* v_{lj}*,* w_{lj} *and the functions* $\omega_0(t)$*:* $V_1 = v\big(X(t)\big)$*,* $V_2 = w\big(X(t)\big)$*,* $r = \rho_{20} - \beta V_1/4 \equiv \rho_{02} + \beta V_1/4$*, conditions* (5) *and* (6) *hold* (*the initial relations from the chain, that is, the analog of the Hugoniot conditions and the corrections to them for isolated vortices of the "shallow water" system of equations*) *in addition to* (3) *and* (4).

2. *Conditions* (3)–(6) *and the representation* (8) *are necessary and sufficient conditions for the function* (2′) *to satisfy the original system of equations up to* $O(|x - X(t)|^3)$.

The analysis of the formula (8) presents no difficulty. The function $\widetilde{u}$ describes the motion of an isolated vortex along the trajectory Γ of the velocity field $\mathbf{u}$ (or $u(x,t)$). "In the main," the "section" of the vortex is an ellipse with semi-axes determined by the numbers b_1 and b_2 and initial angle θ_0. This "ellipse of asymmetry" rotates with the motion along the trajectory $X(t)$ (because of the Cauchy–Riemann conditions) with instantaneous angular velocity $\dot{\theta} = -\frac{1}{2}\operatorname{curl}_3 \mathbf{u}\big(X(t),t\big)$. The direction of rotation of the vortex itself is determined by the sign of the constant A: when $A > 0$, we have counterclockwise rotation, corresponding to the rotation of cyclones. Since for tropical cyclones the geopotential has a (local) minimum at the center, the constant c must be negative. The physical meaning of $-c$ is the value of the potential vortex on the trajectory. It thus follows from (8) that the value of the potential vortex on the trajectory of the center of the vortex itself must be positive. It is known from observations that such is indeed the case.

We remark also that the "vortex" (nonsmooth) part of the (tangential) velocity increases with sufficiently slow elongation from $X(t)$ — like $|x - X(t)|^2$. This means, in particular, that the vortex constructed does not behave like a rigid body. In a rigid body the velocity is proportional to $|x - X(t)|$. The function $\widetilde{\rho}$ increases even more slowly[7] — like $|x - X(t)|^3$. It is also easy to see that the curves $|\widetilde{u}| = \mathrm{const}$ (the level curves of the absolute value of the "vorticial" part of the velocity) get compressed as ρ_0 (the values of the geopotential on the trajectory) increases, at least in some neighborhood of the trajectory $X(t)$. We shall see below that on smooth trajectories $X(t)$ the function $\rho_0(t)$ increases under south-to-north motion (in the northern hemisphere); it thereby follows from (8) that vortices must get compressed under south-to-north motion. For typhoons this fact is also observed. Finally, we note that the dynamics of a typhoon just described retains the property of asymmetry ("general position"). In essence, a vortex in first approximation is transformed using a "locally"

[7] Such slow increase of the functions (8) is an argument in favor of applying them to typhoons — it corresponds to the presence of an "eye" in the typhoon.

conformal transformation. It is rotated, dilated, and compressed identically in all directions. The dilation factor is determined by the divergence of the velocity field $\mathbf{u}(x,t)$ on the trajectory $X(t)$.

In the system (1) the "compressibility" η plays an important role, both in the derivation of the chain and in the subsequent study of it. If we consider a two-dimensional Euler equation instead of the system (1) (for an incompressible fluid — formally this means that the equation of continuity in the system (1) is replaced by $\operatorname{div}\mathbf{u} = 0$), the vector-valued functions $X(t)$ and $V(t)$ will not link up with the other equations in u and ρ, and the trajectory Γ can be *given arbitrarily*. This fact, which follows from the invariance of the Euler equation in the whole space under transition to a noninertial coordinate system, seems to be explainable by the circumstance that the boundary effects in an incompressible medium affect the trajectory of the singularity even in the zeroth approximation, while in a compressible medium one can "localize," in some approximation, the problem of propagation of a singularity. The "compressibility" condition in writing down the chain for point singularities is consistent with the fact that for shock waves the Hugoniot conditions can be written for (compressible) equations of gas dynamics. Finally, we note that if we assume that the smooth background in ($2'$) is given, then the functions $(\widetilde{\rho},\widetilde{u})$, which define the singular part of the solution, will satisfy the system (1) linearized on the background (ρ,u). This linearized system has three types of characteristics or "modes," as it is customary to speak in hydrodynamics. One of them is the so-called "hydrodynamical" or "slow" mode, and the other two are the "acoustic" or "fast" modes. Here the "acoustic" or "fast" modes result from the presence of "compressibility" in the initial system (1): in the linearized Euler equation there are no such modes. The requirement of "structural self-similarity" of the solution ($2'$) leads to a motion of the singularity (vortex) only in a "slow" mode asymptotically, without transferring any energy to the "rapid modes," whose presence is due to compressibility. Thus, although the inclusion of the functions that define the trajectory in the chain is possible *due to the presence of fast modes*, the "main part" of the vortex is transported according to *"slow"* (*hydrodynamical*) modes, rather than *"fast"* ones.

5. Integrals of a Closed Chain and Reduction to Hill's Equation

As we have already said, the closure procedure described above involves setting the terms on the right-hand side of (6) equal to zero; we shall assume from now on that the right-hand sides are zero. This procedure is based on a proposition that follows from the analysis of the equations of the chain that follow (5) and (6) (and are given in [7] and [8]).

Proposition 2. *Suppose the coefficients* ρ_k*,* v_k*, and* w_k *satisfy Eqs.* (3)–(6) *for* $|k| \leqslant 2$*, and* S *and* g *are defined by* (8). *Then* u_k *and* v_k*,* $k=(k_1,k_2)$*,* $k_1+k_2=3$ *can be chosen so that the function* (2) *satisfies the original system* (1) *up to* $O(|x-X(t)|^3)$ *for all values of the Taylor coefficients for the functions* $u=(v,w)$ *with indices* $k=(k_1,k_2)$*,* $|k| \geqslant 4$*, and for the functions* g*,* S*, and* ρ *with indices* $|k| \geqslant 3$*. In particular, setting all these "leading" coefficients equal to zero, we obtain the approximate solutions* (2) $\operatorname{mod} O(|x-X(t)|^3)$ *with* $\mathbf{f}$*,* g*, and* S *depending polynomially on* $(x-X(t))$ (*with degree at most* 3).

Thus, after truncation of the chain we obtain in a certain sense an asymptotic solution (in powers of $x-X(t)$) of the original equation.

There is as yet no rigorous proof that the proposed closure of the chain leads to the determination of a singularity of the trajectory $X(t)$ that differs only slightly from the actual trajectory of the vortex over some time intervals. However, as will be seen below, the closed-chain solutions that interest us must be (and are chosen to be) stable; for that reason, if we assume that the right-hand sides in (6) are not zero but very small, one can probably prove (this is an open problem) that over certain time intervals they will make a very small contribution to the solution of the system. On the other hand, this proposition makes it possible to determine completely the "leading" part of the nonsmooth component of the solution (2′). Therefore, this truncation of the chain is quite reasonable: truncation at the preceding step (that is, setting the coefficients v_{20}, w_{02}, v_{02}, and so on equal to zero) does not enable us to describe the leading nonsmooth part accurately, and taking account of the following equations leads to a multiple complication of the equations. (Truncating at the third step adds another 12 equations, at the fourth step 27, and so forth.)

Let us explain how Hill's equation (7) arises. The reduction of the system (3)–(6) to this equation entailed [7] the introduction of new variables [15], [17], in which the "truncated" chain is greatly simplified; this, in turn, makes it possible both to simplify the reduction procedure itself and to advance a long way in the solution of the problem of typhoon trajectories, now taking account of a variable Coriolis force (the "β-effect"). To be specific, we introduce a complex coordinate $z=x_1+\mathrm{i}x_2$ on the (x_1,x_2)-plane. Together with the complex-conjugate coordinate $\bar{z}=x_1-\mathrm{i}x_2$, it generates the complex derivatives

$$\frac{\partial}{\partial z}=\frac{1}{2}\left(\frac{\partial}{\partial x_1}-\mathrm{i}\frac{\partial}{\partial x_2}\right),\quad \frac{\partial}{\partial \bar{z}}=\frac{1}{2}\left(\frac{\partial}{\partial x_1}+\mathrm{i}\frac{\partial}{\partial x_2}\right).$$

We then introduce the complex regular component of the velocity $\mathbf{v} = v(x,t) + \mathrm{i}w(x,t)$, the complex trajectory Γ of the singularity (vortex) $X = X_1(t) + \mathrm{i}X_2(t) \equiv z\big|_\Gamma$, the complex velocity of the singularity $V = V_1(t) + \mathrm{i}V_2(t) \equiv \mathbf{v}\big|_\Gamma$, and the following real and complex variables:

$$\nu = |c|\rho_0, \qquad Y = \frac{1}{\nu^{3/2}|c|}\left(2\frac{\partial^2\mathbf{v}}{\partial z\partial\bar{z}} - \frac{\beta}{3}\right)\bigg|_\Gamma, \qquad Z = \frac{1}{\nu^{3/2}|c|}\left(\frac{\partial^2\mathbf{v}}{\partial z^2} + \frac{\beta}{3}\right)\bigg|_\Gamma,$$

$$U = \frac{1}{\nu^{3/2}|c|}\frac{\partial^2\mathbf{v}}{\partial\bar{z}^2}\bigg|_\Gamma, \qquad W = \frac{1}{2\nu^{3/2}|c|}\left(\frac{c}{\mathrm{i}}\frac{\partial\rho}{\partial\bar{z}} - \frac{\partial^2\mathbf{v}}{\partial\bar{z}\partial z} + \frac{\partial^2\bar{\mathbf{v}}}{\partial\bar{z}^2} + \beta\right)\bigg|_\Gamma,$$

$$\lambda = \frac{1}{4} - \frac{2r}{\nu^2} + |c|\Re\left((Z - \bar{Y})W + \frac{3}{2}YZ\right).$$

In the variables just introduced, the "truncated" chain has the following form:

$$\dot{V} + \mathrm{i}\omega_0 V + \mathrm{i}\sigma(\nu)^{3/2}(Y + W - 2\bar{Z}) = 0, \quad \dot{Y} = \mathrm{i}(p - \omega_0)Y - \frac{\mathrm{i}\beta(2p + \omega_0)}{3|c|\nu^{3/2}},$$

$$\dot{Z} = \mathrm{i}(3p - \omega_0)Z + \frac{\mathrm{i}\omega_0\beta}{3|c|\nu^{3/2}}, \quad \dot{U} = -\mathrm{i}(p + \omega_0)U, \quad \dot{W} = -\mathrm{i}pW,$$

$$\dot{\lambda} = -\frac{2\beta}{\nu^{3/2}}\Im\left(\frac{2p}{3}W - \frac{\omega_0}{2}Y + \frac{2p + \omega_0}{2}Z\right), \quad \dot{\omega}_0 = \beta\Im V,$$

$$\frac{1}{2}\frac{\mathrm{d}}{\mathrm{d}t}\left(\frac{1}{\nu}\frac{\mathrm{d}\nu}{\mathrm{d}t}\right) - \frac{1}{4}\left(\frac{1}{\nu}\frac{\mathrm{d}\nu}{\mathrm{d}t}\right)^2 + c^2Q\nu^2 - b^2 = 0 \Longleftrightarrow \frac{\mathrm{d}^2\sqrt{\nu}}{\mathrm{d}\Phi^2} + Q\sqrt{\nu} = \frac{b^2}{(\sqrt{\nu})^3},$$

$$\Phi = \sigma\int_0^t \nu\,\mathrm{d}t.$$

Here

$$b^2 = \frac{\omega_0^2 + 2\beta\Re V}{4}, \quad \sigma = \operatorname{sign} c,$$

$$Q = \lambda + c\Re\left(ZW - \bar{Y}W + \frac{3}{2}YZ\right), \quad p = \frac{\omega_0 + \sigma\nu^3}{2}.$$

From such a representation with $\beta = 0$ it is easy to find the integrals of the "truncated" chain: they are $|Y|$, $|Z|$, $|U|$, $|W|$, and λ. It is also easy to discover the reduction to Hill's equation (7). Indeed, the potential in it coincides with the potential Q in the equation for $\sqrt{\nu}$, which is called Ermakov's equation and reduces exactly to that family of Hill's equations. This fact makes it possible to express all solutions of the "truncated" chain immediately in terms of solutions of Hill's equation, or at least roughly describe their properties. In particular, it makes it possible to connect the properties of the trajectory with the zonal theory. It is also clear that the solutions are oscillatory in nature.

In terms of its physical meaning the geopotential, and consequently also ρ_0, must be bounded above and below by positive constants on the time intervals under consideration; moreover in the typhoon problem these constants do not differ from each other greatly (they are within 10% of each other). Using the known stability properties of Hill's equation and explicit formulas that express the functions $\rho_0(t)$ and $X(t)$ in terms of solutions of Hill's equation, one arrives at the following important facts.

1. *The Coriolis frequency must be nonzero.*

2. *The parameters in Hill's equation* (*that is, the constants of integration of the "truncated" chain*) *must be such that the equation is stable, or at least correspond to a rather narrow zone of instability* (*with small increments and decrements of instability*).

A further consequence is that in the stable case the function $\rho_0(t)$ turns out to be a quasi-periodic function with two frequencies, one of which is equal to ω_0.

Another physically reasonable assumption — of sufficient smoothness of the trajectories Γ — and a completely elementary study of the equations for the velocity and the functions Y, Z, and W leads to the requirement that the oscillating part of the potential in Hill's equation be small, and that one of the frequencies $p-\omega_0$, $3p-\omega_0$, and p of this system be small. This requirement must be met both when $\beta=0$ and when β is small. Simple computations show that such a situation is possible when the parameter λ lies in a neighborhood of the numbers $1/4$ and $9/4$; in other words, *in a neighborhood of the first and third gaps in the spectrum of Hill's equation.*

When $\beta=0$, in particular cases, one can construct exact solutions with a constant value of ρ_0 by choosing some of the constants of integration of the truncated chain so that the potential in Hill's equation is equal to the constant[8] λ. Very simple formulas for the trajectories correspond to these solutions. For example, choosing $Y=W=0$, $p=\text{const}$, $\omega=\omega_0=\text{const}$, $c<0$, Dobrokhotov obtains an *exact formula*: $X=X^0+V^0\mathrm{e}^{-\mathrm{i}\omega_0 t}+A\mathrm{e}^{\mathrm{i}(\omega_0-3p)t}$, where X^0, V^0, and A are complex constants of integration. In the (x_1,x_2)-plane this trajectory is an epicycloidal curve, and if the frequency $3p-\omega_0$ is small, which corresponds to a parameter value λ near $9/4$ (the "trace" of the third instability zone of Hill's

[8] This is possible due to the "potential well" in Ermakov's equation for $\sqrt{v}$ *under the condition* $\omega\neq 0$.

equation), then the motion along this curve is compounded of a slow motion along a "great" circle (with this frequency) and a rapid motion with a "proper" Coriolis frequency ω_0. If $\lambda > 9/4$, then $3p < \omega_0$, and the motion is clockwise, while if $\lambda < 9/4$, then $3p > \omega_0$, and the motion is counterclockwise. Thus, in the absence of the "β-effect," the direction of motion is invariant and is *connected with the third and fourth stability zones of Hill's equation* (7). Similar solutions can be obtained for the situations when $Z = W = 0$, $p - \omega_0$ is small, and $Y = Z = 0$, p is small. Here, when $p - \omega_0$ is small, the constant c is positive, so that such a mode, in contrast to the other two, is of no interest from the point of view of the typhoon problem.

6. The "β-Effect," Averaging, and the Equation of Physical Pendulum Type for Typhoon Trajectories

Including the parameter β changes the situation rather strongly if the times are comparable with $1/\beta$, as is the case in the typhoon problem, and accords completely with the opinion of specialists as to the influence of the "β-effect" on trajectories. In the system being studied the parameter β is small, and the system is a typical adiabatically perturbed problem, in which averaging methods work: the integrals described above become slowly varying functions. In this process it turns out that the evolution of the two types of solutions described above for $\beta = 0$ — with a constant geopotential on Γ and either a small frequency $3p - \omega_0$ or a small frequency $3p$ — is of particular interest from the point of view of the typhoon problem. Since one of the frequencies of the system (of the "truncated" chain) is small, we are dealing with a "partial" averaging. We remark that λ and the other parameters in Hill's equation now become "slowly varying functions," and it is possible to pass from one stability zone to another, leading to a change in the direction of motion. (In solid-state physics, such an effect is called a Zener breakdown.)

We have already noted another truly remarkable and surprising fact: the averaged equations for these modes turn out to be reducible to quadratures. They can be reduced to a family of Hamiltonian systems consisting of two equations for the frequency ω_0, the angle ψ, the direction of the velocity V, a certain parameter R, and Ω depending on c (the constants of integration of the "truncated" chain). They have the same structure for all modes:

$$\dot{\omega}_0 = f\frac{\partial H}{\partial \psi}, \quad \dot{\psi} = -f\frac{\partial H}{\partial \omega_0}, \quad \omega_0|_{t=0} = \omega_0(0), \quad \psi|_{t=0} = \psi(0),$$
$$H = \cos\psi M + N,$$

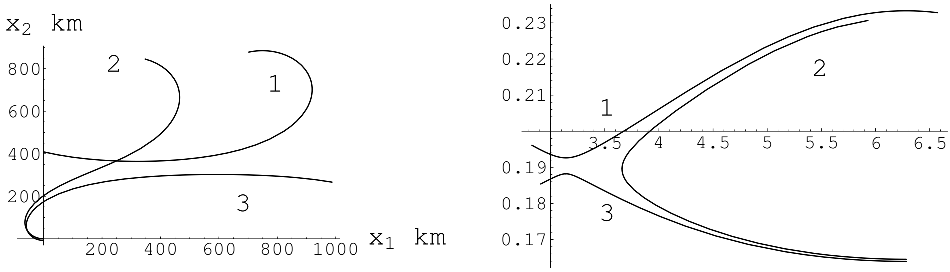
but with different functions M, N, and f. For example, for the case when the frequency $3p-\omega_0$ is "small,"

$$M = R\sqrt{1+\frac{7}{12|c|R^2}\left(\frac{11}{4}-\frac{7}{4(2\eta-1)^2}-\frac{1}{2\eta-1}+\frac{1}{14}\log(2\eta-1)\right)},$$

$$N = -\frac{7(\omega_2)^{3/2}}{4}\int_1^{\eta^7}\frac{y^9(5y^7-6)}{\sqrt{(y^7+3)(2y^7-1)^3}}\,dy,$$

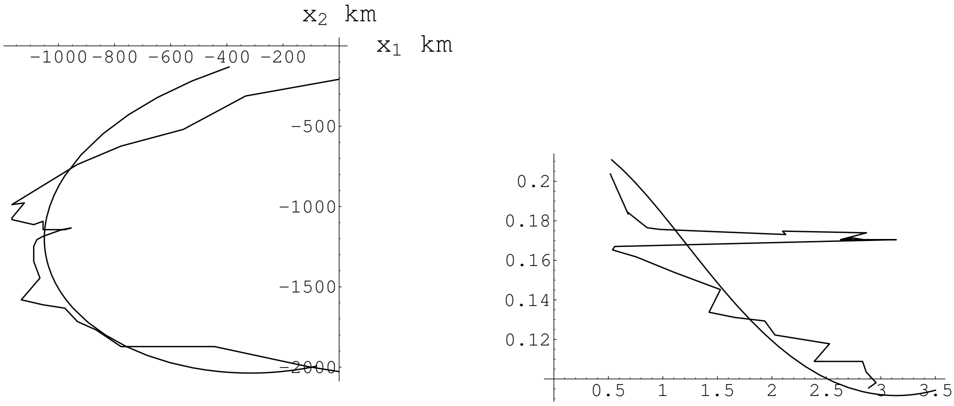
$$f = -\frac{4\sqrt{\omega_0(2\eta-1)^3}}{7\eta\sqrt{\eta+3}}, \quad \eta = \frac{\omega_0}{\Omega}.$$

Its phase portraits on the (ω_0,ψ)-plane are determined by the integral H of the system: $H=\text{const}$, and for particular regions nearly coincide with the well known phase portraits of the trajectories of a physical pendulum. Moreover, the averaging procedure itself, though carried out on the physical level of rigor, but confirmed by a computer, makes it possible to distinguish the "essential" constants among the 17 constants of integration of the "truncated" chain. These are the ones that have the strongest effect on the behavior of the trajectories and which are completely determined using the initial characteristics of the typhoon: the position of its center (eye), the speed of advance of the center, the third component of the curl of the velocity, and the potential vortex (the Rossby invariant) c on the trajectory Γ. Reduction to what is "almost the equation" of a physical pendulum (for different modes) makes it possible to classify the possible trajectories of typhoons and connect them with the trajectories of a physical pendulum over a time interval that does not greatly exceed half a period. For example, to infinite motions below and above the separatrix correspond $\supset$- and $\subset$-shaped trajectories, while to finite motions correspond S- and $\int$-shaped trajectories. Such a correspondence, in turn, enables us to establish a number of useful properties of trajectories. For example, if the initial position and velocity of a pendulum are known, one can determine its maximal displacement, velocity, and so on. The analog of this property is the possibility of determining from the initial data the leftmost and rightmost longitudes and the lowest and highest latitudes a trajectory will reach and so on. Of course, the solutions of the complete "truncated chain" may differ somewhat from those of the averaged chain. In particular, rapid oscillations may arise in them, which lead to vibration in the trajectory Γ. (Continuing with the pendulum analogy, one can say that the pendulum changes its length elastically, the point of suspension is attached to a spring, and the pendulum is a rotating rigid body, not a point mass.) Such "theoretical" typhoon trajectories of a complete "truncated" chain and the phase portraits corresponding to them are shown in Fig. 1a and 1b for one mode. It is



a *b*

Fig. 1



a *b*

Fig. 2

also clear that both the model itself (the system (1)) and the approximation used describe the dynamics of typhoons rather roughly; here, clearly, the greatest defect is the absence of the effect of an energy "drive" from the ocean (the system (1) is *conservative*). Nevertheless, even this model and the "truncated" chain have given a rather good qualitative agreement for a number of actual typhoons. In particular, the "theoretical" typhoons with realistic parameters duplicated the *S*-shaped zigzag trajectories of actual typhoons. One example of the trajectories of a "theoretical" (smooth line), and a real typhoon ("Deanna," June 1997, broken line) and the phase portraits corresponding to them are shown in Fig. 2a and 2b. The quantitative difference between the "theoretical" typhoon and the actual one can be roughly explained by the fact that *the model is conservative*. At large times the energy exchange between the ocean and the atmosphere may have a significant effect on the trajectory.

Naturally, much more research is needed if we are to use the results obtained by Dobrokhotov in problems involving the actual prediction of typhoon trajectories. In particular, we must include the energy exchange, the sphericity of the Earth, and so on. In addition, a number of curious mathematical questions arise here; for example, whether the chains are Hamiltonian, how to exhibit the mechanism (probably a group mechanism) that induces these reductions, and so on. But the facts that have been discovered already show with a great degree of plausibility that they are indeed connected with real typhoons: such surprising and fantastic mathematical beauty in the exact solution of a very complicated system of nonlinear differential equations in 17 unknowns could not occur unless it corresponded to the physical reality.

Bibliography

[1] V. P. Maslov. Three algebras corresponding to nonsmooth solutions of systems of quasi-linear hyperbolic equations. *Uspekhi Mat. Nauk*, 1980, **35**(2), 252–253 (Russian).

[2] V. P. Maslov. On the propagation of a shock wave in an isoentropic nonviscous gas. In: *Itogi Nauki i Tekhniki VINITI*, T. 8. Moscow: VINITI, 1977, 199–271 (Russian).

[3] J. F. Colombeau. *Elementary Introduction to New Generalized Functions*. Amsterdam: North-Holland, 1985.

[4] Yu. V. Egorov. A contribution to the theory of generalized functions. *Russ. Math. Surveys*, 1990, **45**(5), 1–49.

[5] V. G. Danilov, V. P. Maslov, V. M. Shelkovich. Algebras of the singularities of singular solutions to first-order quasi-linear strictly hyperbolic systems. *Theor. Math. Phys.*, 1998, **114**(1), 1–42.

[6] S. Yu. Dobrokhotov, K. V. Pankrashkin, E. S. Semenov. Proof of Maslov's conjecture about the structure of weak point singular solution of the shallow water equations. *Russ. J. Math. Phys.*, 2001, **8**(1), 25–52.

[7] S. Yu. Dobrokhotov. Hugoniot–Maslov chains for solitary vortices of the shallow water equations, I; II. *Russ. J. Math. Phys.*, 1999, **6**(2), 137–173; **6**(3), 282–313.

[8] S. Yu. Dobrokhotov. Integrability of truncated Hugoniot-Maslov chains for trajectories of mesoscale vortices on shallow water. *Theor. Math. Phys.*, 2000, **125**(3), 1721–1741.

[9] V. P. Maslov, G. A. Omel'yanov. Hugoniot-type conditions for infinitely narrow solutions of the equations for simple waves. *Sib. Math. J.*, 1983, **24**(5), 787–795.

[10] A. M. Obukhov. On the problem of geostrophic wind. *Izv. Akad. Nauk SSSR. Ser. Geogr.*, 1949, **13**(4), 281–306 (Russian).

[11] F. V. Dolzhanskii, V. A. Krymov, D. Yu. Manin. Stability and vortex structures of quasi-two-dimensional shear flows. *Physics–Uspekhi*, 1990, **33**(7), 495–520.

[12] J. Pedlosky. *Geophysical Fluid Dynamics*, 2nd edition. New York: Springer, 1987.

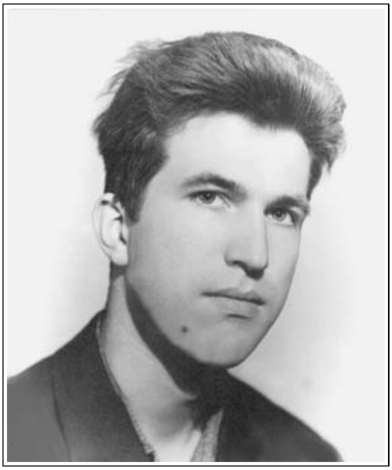
[13] V. V. Bulatov, Yu. V. Vladimirov, V. G. Danilov, S. Yu. Dobrokhotov. An example of computation of the "eye" of a typhoon on the basis of Maslov's conjecture. *Dokl. Ross. Akad. Nauk*, 1994, **338**(1), 102–105 (Russian).

[14] S. Yu. Dobrokhotov, B. Tirozzi. On the Hamiltonian property of the truncated Hugoniot–Maslov chain for trajectories of mesoscale vortices. *Russ. Acad. Sci. Dokl. Math.*, 2002, **65**(3), 453–458.

[15] S. Yu. Dobrokhotov, E. S. Semenov, B. Tirozzi. Hugoniot–Maslov chains for singular vorticial solutions to quasilinear hyperbolic systems and typhoon trajectory. *J. Math. Sci.*, 2004, **124**(5), 5209–5249.

[16] R. Ravindran, P. Prasad. A new theory of shock dynamics, I; II. *Appl. Math. Letters*, 1990, **3**(2), 77–81; **3**(3), 107–109.

[17] S. Yu. Dobrokhotov, E. S. Semenov, B. Tirozzi. Calculation of the integrals of the Hugoniot–Maslov chains for singular vortex solutions of shallow water equation. *Theor. Math. Phys.*, 2004, **139**(1), 500–512.



Yu. V. Matiyasevich

Hilbert's Tenth Problem: Diophantine Equations in the Twentieth Century

Translated by R. Cooke

In August 1900, Paris hosted the second International Congress of Mathematicians. (In those distant days, mathematicians were still able to number their meetings.) One of the invited speakers was David Hilbert. In what became a famous address called "Mathematische Probleme" Hilbert decided to indicate what were, in his opinion, the most important problems bequeathed by the departing nineteenth century to the arriving twentieth century. (We now know that much of it was to be inherited also by the twenty-first century.)

Traditionally, one speaks about 23 Hilbert's problems and numbers them as in the printed text of his address [14]. Actually, many of the Hilbert problems consist of several interrelated questions. For example, the 8th problem includes, in particular,

- Goldbach's Conjecture that every even number from 4 on is the sum of two prime numbers;
- the Riemann Hypothesis on the location of the complex zeros of the Riemann zeta-function, which is connected with the distribution of primes among the natural numbers;
- the conjecture about the existence of infinitely many twin-primes.

(It is of interest to note that neither "Fermat's Last Theorem" nor the Four Color Problem were included in the list by Hilbert.)

So little space was devoted to Problem 10 that the corresponding part of Hilbert's address can be reproduced in full here:

10. Entscheidung der Lösbarkeit einer diophantischen Gleichung. Eine diophantische Gleichung mit irgendwelchen Unbekannten und mit ganzen rationalen Zahlkoefficienten sei vorgelegt: *man soll ein Verfahren angeben, nach welchem sich mittels einer endlichen Anzahl von Operationen entscheiden läßt, ob die Gleichung in ganzen rationalen Zahlen lösbar ist.*

10. Determination of the solvability of a Diophantine equation. Given a Diophantine equation with any number of unknown quantities and with rational integral numerical coefficients: *To devise a process according to which it can be determined by a finite number of operations whether the equation is solvable in rational integers.* (Cited from the English translation of [14].)

The equations referred to in the 10th problem are named in honor of the Greek mathematician Diophantus who, most likely, lived in the third century A. D. *Diophantine equations* have the form

$$D(x_1,\dots,x_m) = 0, \tag{1}$$

where D is a polynomial with integer coefficients.

Hilbert asked about the solvability of Diophantine equations in *rational integers*, by which he meant the numbers $0, \pm 1, \pm 2, \dots$, which form a subset of the set of *algebraic integers*; when I speak of integers below, I mean precisely these numbers $0, \pm 1, \pm 2, \dots$, unless the opposite is stated explicitly.

The domain of allowable values of the unknowns is sometimes regarded as the defining characteristic, and Diophantine equations are taken to mean any equations that are to be solved in integers or natural numbers. On the other hand, the form (1) is also sometimes taken as the defining characteristic, and all polynomial equations are called Diophantine, regardless of the ring in which solutions are sought. Below, Diophantine equations will be understood exactly in the sense of Hilbert, that is, as equations of the form (1) with integer unknowns.

In the time since Diophantus, specialists in number theory have found solutions of many Diophantine equations and have shown that many others have no solutions. Why then did Hilbert regard this problem as still open in 1900?

The fact is that, for various classes of Diophantine equations, and sometimes even for individual specific Diophantine equations, mathematicians were forced to keep inventing new methods every time. (An illuminating example of this is the famous Fermat equation, whose unsolvability took more than three centuries to prove.) In the tenth problem, Hilbert was proposing that a *universal* method be found that would enable mathematicians, at least in principle, to establish the existence or nonexistence of solutions for an arbitrary Diophantine equation.

In modern terminology, Hilbert's tenth problem is a *decision problem*, that is, a problem consisting of a countable number of individual subproblems, each of which requires an answer of YES or NO. (In the present case, the individual subproblems correspond to particular Diophantine equations, about which the question is posed whether they have solutions.) The solution of a decision problem must be given in the form of an algorithm that yields the required answer in finite time for any initial data specifying the particular individual problem.

The word *algorithm*, however, does not appear in the statement of the tenth problem — instead, Hilbert uses a rather vague terminology, proposing that one should find "a process according to which it can be determined by a finite number of operations ... ". However, even if Hilbert had used the word *algorithm* in the statement of the tenth problem, that would not have improved the situation by much. The fact is that in 1900 the rigorous concept of an algorithm had not yet been developed. Examples of specific algorithms were known (the most "venerable" was surely the Euclidean algorithm for finding the greatest common divisor of two integers), there was an intuitive conception of an "algorithm in general," but there was no definition.

Does this mean that the tenth problem was ill-posed by Hilbert? By no means. Observe that he did not ask *whether there exists a process* according to which it can be determined... (as a modern mathematician would most likely do), but stated the problem positively:

man soll ein Verfahren angeben, ... || *To devise a process...*

Hilbert was an optimist in mathematics (the revolutionary results of Gödel appeared more than 30 years after the Hilbert problems) and it appears that he believed that the tenth problem had a positive solution. But if someone had found the "process" it required, it would most likely have become clear that the procedure really does give the required answer after a "finite number of operations."

A completely different situation arises if a decision problem has no solution, as happened in the case of Hilbert's tenth problem. To prove this, and even just to state the unsolvability of such a decision problem, one must have a rigorous definition of an algorithm (or at least a list of essential requirements that the algorithm must satisfy). The failure of all attempts to find a universal method of solving Diophantine equations might have been an impetus to develop a rigorous notion of an algorithm, but I have been unable to discover any *direct* effect of Hilbert's tenth problem on the development of the general definition of an algorithm.

That occurred more than thirty years after Hilbert stated his problems, as a result of investigations of Kurt Gödel, Alan Turing, Emil Post, Alonzo Church, and other mathematical logicians. Several techniques of various types were proposed for representing algorithms (Turing machines, Post machines, λ-calculus, partial recursive functions, and others). All these different techniques turned out to be equivalent to one another. On that basis, Church was the first to assert that one particular technique could be taken as a formal definition of the general notion of an algorithm, and it would adequately reflect our intuitive conception of what an algorithm is. This unprovable assertion is known as *Church's Thesis*.

Soon after Church's Thesis was formulated, algorithmically unsolvable decision problems were found. The first examples of such problems belonged to mathematical logic, and therefore did not discourage "real" mathematicians much.

The momentous year was 1947, in which two mathematicians, Andrei Andreevich Markov [19] in the USSR and Emil Leon Post [30] in the USA, independently of each other, published a proof of the algorithmic unsolvability of the so-called *word problem for finitely presented semigroups*. This problem is known also as *Thue's problem*, after the Norwegian mathematician Axel Thue, who had stated it in 1914 [39]. This was the first case in which it was possible to prove the impossibility of an algorithm for solving a decision problem which arose in mathematics.

After this success with Thue's problem on the one hand, and the failure to find a general method for Diophantine equations on the other, it was natural to attempt to prove the unsolvability of Hilbert's tenth problem. Already in 1944, Post [29] wrote that Hilbert's tenth problem "begs for an unsolvability proof."

These words inspired his student Martin Davis to attempt a negative solution of Hilbert's tenth problem. In the early 1950s he put forward a conjecture from which (together with the known fact that there exist algorithmically unsolvable problems) the impossibility of an algorithm that would determine whether an arbitrary Diophantine equation has a solution followed immediately.

To state Martin Davis' conjecture we need to introduce some relevant terminology; before doing so, we will examine an equivalent reformulation of Hilbert's tenth problem.

To be specific, Hilbert was asking for the solution of Diophantine equations in integers. One can consider the analogous decision problem of solvability of Diophantine equations in the natural numbers. (Following the tradition of mathematical logic, we take the natural numbers to consist of all nonnegative integers; that is, zero counts as a natural number.)

These two decision problems are equivalent to each other in the sense that each can be *reduced* to the other. Indeed, Eq. (1) has a solution in integers if and only if the equation

$$D(p_1 - q_1, \dots, p_m - q_m) = 0 \tag{2}$$

has a solution in the natural numbers. The reduction in the other direction is less obvious: Eq. (1) has a solution in the natural numbers if and only if the equation

$$D(w_1^2 + x_1^2 + y_1^2 + z_1^2, \dots, w_m^2 + x_m^2 + y_m^2 + z_m^2) = 0 \tag{3}$$

has a solution in the integers; for by a theorem of Lagrange every natural number is the sum of four squares.

For technical reasons, it is more convenient to work with natural numbers and throughout the following lower-case italic letters will denote such numbers.

Along with the consideration of equations with numerical coefficients (as in the statement of Hilbert's tenth problem), one can also study Diophantine equations with symbolic coefficients. A *family* of Diophantine equations has the form

$$D(a_1, \dots, a_n, x_1, \dots, x_m) = 0, \tag{4}$$

where D is a polynomial with integer coefficients whose variables are separated into two sets:

- *the parameters* $a_1, \dots, a_n$;
- *the unknowns* $x_1, \dots, x_m$.

Under one choice of values of the parameters $a_1, \dots, a_n$ Eq. (4) may have a solution in the unknowns $x_1, \dots, x_m$, while there is no solution under a different choice of parameters. We can consider the set $\mathfrak{M}$ consisting of all n-tuples $\langle a_1, \dots, a_n \rangle$ for which our parametric equation has a solution:

$$\langle a_1, \dots, a_n \rangle \in \mathfrak{M} \iff \exists x_1 \dots x_m \left\{ D(a_1, \dots, a_n, x_1, \dots, x_m) = 0 \right\}. \tag{5}$$

Sets that are representable in this form are called *Diophantine*, and an equivalence of the form (5) is called a *Diophantine representation* of the corresponding set; taking some liberties with language, one may say that the Diophantine equation itself is a Diophantine representation.

Martin Davis set himself the task of characterizing the entire class of Diophantine sets. The theory of computation imposes one obvious *necessary* condition for a set to be Diophantine, namely, if we are given a parametric Diophantine equation (4), then we can start listing all sets of $n+m$ numbers $a_1,\dots,a_n,x_1,\dots,x_m$ in some order, and verify Eq. (4) for each set. If it holds, we shall put the set $\langle a_1,\dots,a_n\rangle$ into a separate list. This list will contain only elements of the set $\mathfrak{M}$, and each element of this set sooner or later would appear in our list, possibly with repetitions.

The algorithm just exhibited for listing all the elements of a Diophantine set had a very special form. Allowing use of an arbitrary algorithm, we arrive at the following notion: a set $\mathfrak{M}$ consisting of n-tuples of natural numbers is called *recursively enumerable* if there exists an algorithm that, running for a potentially infinite time, will print out a list of all elements of the set $\mathfrak{M}$.

As we have seen, every Diophantine set is recursively enumerable. Martin Davis [3], [4] proposed that the converse is also true:

Martin Davis' conjecture. *The notions of Diophantine set and recursively enumerable set coincide; that is, a set is Diophantine if and only if it is recursively enumerable.*

The algorithmic unsolvability of Hilbert's tenth problem would follow immediately from Martin Davis' conjecture, since examples of recursively enumerable sets of natural numbers were known for which there is no algorithm that can determine whether an arbitrary integer belongs to them.

It is my opinion that Martin Davis' conjecture was quite daring. The informal arguments against it were very imposing. For example, it is easy to see that the set of all prime numbers is recursively enumerable. It followed from Martin Davis' conjecture that it is a Diophantine set, that is, there exists a specific polynomial P such that the equation

$$P(a,x_1,\dots,x_m)=0 \tag{6}$$

has a solution if and only if a is a prime. Hilary Putnam [32] remarked that this equation is equivalent to the following:

$$a=(x_0+1)\big(1-P^2(x_0,x_1,\dots,x_n)\big)-1. \tag{7}$$

Indeed, each solution of (6) can be expanded to a solution of (7) by the choice

$$x_0=a. \tag{8}$$

On the other hand, in each solution of (7) with nonnegative a the product on the right-hand side must be positive, which is possible only when

$$P(x_0,\dots,x_n)=0.$$

But then (8) also holds, and consequently also (6).

Thus it followed from Martin Davis' conjecture that there existed a remarkable polynomial, namely the right-hand side of (7), whose values at the natural numbers were precisely the set of prime numbers. This striking consequence was regarded by many as an informal argument against the conjecture.

Another implausible consequence was the following. For every fixed n we can list all Diophantine sets of n-tuples of natural numbers:

$$\mathfrak{M}_0, \mathfrak{M}_1, \ldots, \mathfrak{M}_k, \ldots \tag{9}$$

Speaking more formally, there exists a *universal recursively enumerable set* $\mathfrak{U}_n$ such that

$$\langle a_1, \ldots, a_n, k \rangle \in \mathfrak{U}_n \Longleftrightarrow \langle a_1, \ldots, a_n \rangle \in \mathfrak{M}_k. \tag{10}$$

According to Martin Davis' conjecture, the set $\mathfrak{U}_n$ has a Diophantine representation

$$\langle a_1, \ldots, a_n, a_{n+1} \rangle \in \mathfrak{U}_n \Longleftrightarrow$$
$$\exists y_1 \ldots y_M \ \big\{ U_n(a_1, \ldots, a_n, a_{n+1}, y_1, \ldots, y_M) = 0 \big\}. \tag{11}$$

For any Diophantine equation (4) we can effectively find k_D — the index of the set defined by this equation in the sequence (9); according to (10) and (11), the same set is also given by the equation

$$U_n(a_1, \ldots, a_n, k_D, y_1, \ldots, y_M) = 0. \tag{12}$$

Thus, Eqs. (4) and (12) have solutions for the same set of parameter values, but the new equation (12) has a fixed number of unknowns and a fixed degree, while the original equation (4) may have arbitrarily many unkowns and may be of arbitrarily high degree.

Martin Davis [4] took the first step toward the proof of his conjecture when he obtained an almost Diophantine representation:

Theorem (M. Davis)**.** *Every recursively enumerable set has a representation of the form*

$$\langle a_1, \ldots, a_n \rangle \in \mathfrak{M} \Longleftrightarrow \exists z \ \forall y \leqslant z \ \exists x_1 \ldots x_m \ \big\{ D(a_1, \ldots, a_n, x_1, \ldots, x_m, y, z) = 0 \big\},$$

where D is a polynomial with integer coefficients.

Representations of this type came to be called the *Davis normal form*. They became a quantitative improvement of the classical result of Kurt Gödel [12] on the existence of such arithmetic representations with an arbitrary number of universal quantifiers.

The single universal quantifier that remains in the Davis normal form was eliminated in a joint paper by Martin Davis, Hilary Putnam, and Julia Robinson [9], but a high price was paid for this elimination. To be specific, they were forced to consider the larger class of so-called *exponentially Diophantine equations*. These are equations of the form

$$E_{\mathrm{L}}(x_1,x_2,\dots,x_m) = E_{\mathrm{R}}(x_1,x_2,\dots,x_m),$$

where E_{L} and E_{R} are *exponential polynomials*, that is, expressions constructed by the traditional rules from specific natural numbers and variables using the operations of addition, multiplication, and exponentiation. The result was the *exponential Diophantine representation* (13) of recursively enumerable sets:

Theorem (M. Davis, H. Putnam, J. Robinson)**.** *For every recursively enumerable set $\mathfrak{M}$ consisting of n-tuples of natural numbers there exist exponential polynomials E_{L} and E_{R} such that*

$$\langle a_1,\dots,a_n\rangle \in \mathfrak{M} \Longleftrightarrow \exists x_1\dots x_m \\ \big\{E_{\mathrm{L}}(a_1,\dots,a_n,x_1,x_2,\dots,x_m) = E_{\mathrm{R}}(a_1,\dots,a_n,x_1,x_2,\dots,x_m)\big\}. \quad (13)$$

The algorithmic unsolvability of the analog of Hilbert's tenth problem for exponentially Diophantine equations followed immediately from this theorem, and this result in the end turned out to be an important link in the proof of the unsolvability of the tenth problem in its original formulation. This role of the Davis–Putnam–Robinson theorem was not understood immediately, however. Here, for example, is what Georg Kreisel wrote in *Mathematical Reviews* [17] about the paper [9] of Davis, Putnam, and Robinson:

> These results are superficially related to Hilbert's tenth Problem on (ordinary, i. e., non-exponential) Diophantine equations. The proof of the authors' results, though very elegant, does not use recondite facts in the theory of numbers nor in the theory of r. e. [recursively enumerable] sets, and so it is likely that the present result is not closely connected with Hilbert's tenth Problem. Also it is not altogether plausible that all (ordinary) Diophantine problems are uniformly reducible to those in a fixed number of variables of fixed degree, which would be the case if all r. e. sets were Diophantine.

Kreisel was not alone in such an underestimation of the role of the paper of Davis, Putnam, and Robinson. When my scientific advisor Sergei Yur'evich

Maslov [10] suggested to me in late 1965 (when I was a second-year student in the Department of Mathematics and Mechanics at Leningrad Unversity) that I should tackle Hilbert's tenth problem, I asked him what I should read first. In response I heard that, "there have been several papers of American mathematicians on Hilbert's tenth problem, but there is no need to read them." — "Why not?" — "Their approach has not yet led to any success, and is most likely 'barren'," answered Maslov. He told me of another approach being promoted by A. A. Markov. This approach was much less ambitious than Martin Davis' conjecture. Namely, it was known that to prove the undecidability of Hilbert's tenth problem, it sufficed to prove the algorithmic unsolvability of the so-called *word equations* (or *equations in free semigroups*), since they could easily be reduced to Diophantine equations.

I spent some time trying unsuccessfully to prove the unsolvability of word equations. Much later it became known that the approach suggested to me by Maslov could never lead to success — in 1997 Gennadii Semenovich Makanin [18] found an algorithm for word equations.

Makanin's theorem explained why I had been unable to prove the undecidability of word equations. This, however, was not the goal, but only a means; the goal was Hilbert's tenth problem. In the attempt to attain this goal I established that the larger class of systems of equations consisting both of word equalities and word length equalities could be reduced to Diophantine equations. A completely different technique was required for such a reduction, based on a positional system of notations in which the weights of digits are Fibonacci numbers. I published this result and two other papers devoted to various approaches to Hilbert's tenth problem.

None of my efforts at the time led to success (and the question of solvability of word-and-length equations remains open to this day), and in the end I read those "several papers of American mathematicians." (Sergei Ivanovich Adyan initiated and edited translations into Russian of the main papers on Hilbert's tenth problem; they were published in an issue of the journal *Matematika*.)

To carry the Davis–Putnam–Robinson result over to the case of "genuine" Diophantine equations, it sufficed to establish that the set

$$\big\{\langle a,b,c\rangle : a^b = c\big\} \tag{14}$$

is Diophantine. Indeed, it is not difficult to see that, using a Diophantine representation of this set,

$$a^b = c \Longleftrightarrow \exists z_1 \dots z_m \ \big\{A(a,b,c,z_1,\dots,z_m) = 0\big\}, \tag{15}$$

we could transform an arbitrarily exponentially Diophantine representation into a Diophantine representation at the cost of introducing additional unknowns. In other words, to prove Martin Davis' conjecture *in full* it would suffice to prove it for the *special case* of the set (14).

Julia Robinson began to study the question whether the set (14) is Diophantine as early as 1948, that is, just when Martin Davis stated his conjecture. However, the original direction of her research was just the opposite: the prominent mathematical logician Alfred Tarski was wondering whether one could prove, for example by induction, what seemed to him to be the plausible proposition that even the simpler set of all powers of 2 is *not* Diophantine.

After spending some time trying unsucessfully to prove Tarski's conjecture, Julia Robinson switched to searching for a Diophantine representation (15) for exponentiation. She did not succeed in finding it, but she established sufficient conditions for exponentiation to be Diophantine. To be specific, the representation (15) can be constructed on the basis of the Diophantine representation

$$\langle u,v\rangle \in \mathfrak{R} \Longleftrightarrow \exists y_1 \dots y_m \ \{J(u,v,y_1,\dots,y_m)\} = 0 \tag{16}$$

of any set $\mathfrak{R}$ having the following two properties:

- if $\langle u,v\rangle \in \mathfrak{R}$, then $u < v^v$;
- for every k there exist u and v such that $\langle u,v\rangle \in \mathfrak{R}$ and $u > v^k$.

Julia Robinson called the relation between u and v having these two properties a *relation of exponential growth*; in the literature they also began to be called the *Julia Robinson predicates*.

It might seem that to prove that exponentiation is Diophantine it remained to examine the numerous results obtained in number theory since Diophantus, and to find a two-parameter Diophantine equation that defined a relation of exponential growth. Amazing though it is, no such equation could be found.

Attempts to construct such an equation "artificially" also were going without success. When it became known that the existence of Julia Robinson's predicate defined by a Diophantine equation implied not only that exponentiation was Diophantine but also the full Martin Davis conjecture with all its incredible corollaries, the existence of such predicates began to seem even less plausible, and losing faith in it, Julia Robinson attempted to prove the opposite for a time.

My first impression of the definition of the exponential growth relation was "what an unnatural concept," but I soon began to recognize its naturalness and its importance.

I decided to organize a seminar on Hilbert's tenth problem. At the first session, where I gave a brief survey of the known results, the audience consisted of five logicians and five number theorists. The number theorists quickly stopped attending the seminar and the logicians all abandoned it soon after, so that I was left face to face with Hilbert's tenth problem. Much later, after it was solved, I asked one of the number theorists who had been at the first session of the seminar why the number theorists had abandoned it so quickly. The answer was, "We were striving to prove that the set of prime numbers is not Diophantine, convinced that we could do that in about two weeks." Although they didn't succeed at that, specialists in number theory would no longer come to a seminar whose purpose was to prove such an implausible assertion.

I was now devoting almost all my free time to the search for a Diophantine relation of exponential growth. There was nothing wrong with a second-year student attempting to solve a famous problem, but it looked silly when I continued my attempts unsuccessfully for several years. One professor began to laugh at me. Every time he met me he would ask, "Have you proved the unsolvability of Hilbert's tenth problem? Not yet? Then you won't be able to defend your senior thesis!"

Nevertheless, in 1969 I graduated and became a graduate student at the Leningrad Department of the Mathematical Institute of the USSR Academy of Sciences (LOMI). Naturally, Hilbert's tenth problem was an "undissertationable" topic and could not be the subject of my efforts, at least not for the three years while I was a graduate student.

Once in the autumn of 1969 one of my new colleagues said to me, "Drop into the library. In the latest issue of the *Proceedings of the American Mathematical Society* there is a new article of Julia Robinson on Hilbert's tenth problem." However, I was firm in my resolution to abandon that problem. I said to myself, "It's very nice that Julia Robinson is continuing to study Hilbert's tenth problem, but I myself can't afford to spend any more time on it." And I didn't go to the library.

Somewhere in Mathematical Heaven there must be a God or Goddess of mathematics who would not let me avoid becoming acquainted with Julia Robinson's new paper. Because of my undergraduate publications I was regarded as a specialist in Hilbert's tenth problem, and for that reason the journal *Referativnyi zhurnal "Matematika"* (the Russian counterpart of *Mathematical Reviews*) sent me a copy of Julia Robinson's article [36] to review.

Hilbert's tenth problem once again took hold of me. I saw that Julia Robinson had a new, very promising idea. It was connected with the special form of the Pell equation

$$x^2 - (a^2 - 1)y^2 = 1. \tag{17}$$

The solutions $\langle\chi_0,\psi_0\rangle$, $\langle\chi_1,\psi_1\rangle$, $\dots$, $\langle\chi_n,\psi_n\rangle$, $\dots$ of this equation, listed in increasing order, satisfy the recurrent relations

$$\chi_{n+1} = 2a\chi_n - \chi_{n-1}, \tag{18}$$

$$\psi_{n+1} = 2a\psi_n - \psi_{n-1}. \tag{19}$$

It is easy to see from this that for every m both sequences $\chi_0,\chi_1,\dots$ and $\psi_0,\psi_1,\dots$ are purely periodic modulo m, and consequently their linear combinations are also periodic. Then it is easy to verify by induction that the period of the sequence

$$\psi_0,\ \psi_1,\ \dots,\ \psi_n,\ \dots \quad (\mathrm{mod}\ (a-1)) \tag{20}$$

consists of the numbers

$$0, 1, 2, \dots, a-2, \tag{21}$$

and the period of the sequence

$$\chi_0-(a-2)\psi_0,\ \chi_1-(a-2)\psi_1,\ \dots,\ \chi_n-(a-2)\psi_n,\ \dots \quad (\mathrm{mod}\ (4a-5)) \tag{22}$$

begins with

$$2^0, 2^1, 2^2, \dots \tag{23}$$

The main new idea of Julia Robinson was to "synchronize" the sequences (20) and (22) by imposing some condition $G(a)$ to guarantee that

$$\begin{array}{c}\text{the length of the period of the sequence (22)}\\ \text{is divisible by the length of the period of the sequence (20).}\end{array} \tag{24}$$

Julia Robinson showed that if such a condition $G(a)$ is Diophantine and holds for an infinite number of values of a, then the set (14) is Diophantine. However, she did not succeed in finding a suitable Diophantine condition G, and even today we do not know any methods for giving a direct Diophantine definition of it.

I liked the idea of synchronization very much and tried to apply it in a slightly different situation. As mentioned above, I was using Fibonacci numbers to reduce word-and-length equations to Diophantine equations. In doing this I discovered (for myself) the equation

$$x^2 - xy - y^2 = \pm 1. \tag{25}$$

For the Fibonacci numbers this equation plays a role analogous to the role of the Pell equation, namely that the Fibonacci numbers ϕ_n, and only they, satisfy Eq. (25).

The fact that successive Fibonacci numbers give the solution of Eq. (25) was presented by Jean-Dominique Cassini to the Académie Royale des Sciences as long ago as 1680. It can be proved by a trivial induction. At the same time the stronger fact that Eq. (25) is characteristic of the Fibonacci numbers is somehow not given in standard textbooks. The induction required to prove the converse is less obvious, and that fact seems to be the reason for the inclusion of the problem of inverting Cassini's identity as Exercise 6.44 in *Concrete Mathematics* by Ronald Graham, Donald Knuth, and Oren Patashnik [13]. As the original source of this problem the authors cite my paper [21], but I have always suspected that such a simple and fundamental fact must have been discovered long before me. This suspicion turned out to be justified: I have recently found a paper of M. Wasteels [41] published in 1902 in the obscure journal *Mathesis*.

The arithmetical properties of the sequences ψ_n and ϕ_n are very similar. In particular, the sequence

$$0, 1, 3, 8, 21, \ldots \tag{26}$$

of even-indexed Fibonacci numbers satisfies the recurrence

$$\phi_{2(n+1)} = 3\phi_{2n} - \phi_{2(n-1)}, \tag{27}$$

which is analogous to (18) and (19). This sequence increases like $[(3+\sqrt{5})/2]^n$ and can be used in place of (23) to construct a relation of exponential growth. The role of (22) can be played by the sequence

$$\psi_0, \psi_1, \ldots, \psi_n, \ldots \quad (\text{mod } (a-3)), \tag{28}$$

since it begins like (26). Moreover, for particular values of a the period can be determined exactly. Namely, if

$$a = \phi_{2k} + \phi_{2k+2}, \tag{29}$$

then the period of the sequence (28) consists of the numbers

$$0, 1, 3, \ldots, \phi_{2k}, -\phi_{2k}, \ldots, -3, 1. \tag{30}$$

Such a simple form for the period looked very promising.

I thought intensively along these lines, even on New Year's Eve of 1970, and made my own contribution to the stories of absent-minded mathematicians, leaving my uncle's house in the morning wearing his overcoat. On the morning of 3 January it seemed to me that I had found the polynomial J required in (16); however, by evening I had found a mistake in my reasoning. But the next morning I was able to correct my construction.

What else needed to be done? As a student I had already had a sad experience, when I once announced that I had proved the unsolvability of Hilbert's tenth problem but found a mistake while presenting my proof. I would not have wanted to be once again in such a position disgraceful for a mathematician, and certain places in my new proof seemed rather suspicious to me. At first I supposed that all I had done was to apply Julia Robinson's idea for another recurrrent sequence. However, in her construction a vital role was played by a special equation that guaranteed that one variable was exponentially larger than another. In my supposed proof there was no need at all to use such an equation, and that was the strange part. Later on I realized that my construction was actually dual to Julia Robinson's construction. In fact, I had found a certain Diophantine condition $H(a)$ guaranteeing that

$$\begin{gathered}\text{the length of the period of the sequence (20)}\\ \text{is divisible by the length of the period of the sequence (28).}\end{gathered} \tag{31}$$

Such a condition H, however, could not play the role of Julia Robinson's G, and the construction as a whole was obtained in a completely different way. My transition from (24) to the dual (31) redistributed the difficulties. The route from a Diophantine representation of H to a Diophantine representation of relation of exponential growth is not as straightforward as the route proposed by Julia Robinson starting from a hypothetical Diophantine representation for G. On the other hand, constructing a Diophantine representation for my H turned out to be much simpler than for the condition G that Julia Robinson had been working with.

To do this, I used, in particular, the following lemma:

$$\phi_n^2 \,\big|\, \phi_m \Rightarrow \phi_n \,\big|\, m. \tag{32}$$

It is not difficult to prove this remarkable property of the Fibonacci numbers *once it has been stated*, but this beautiful fact seems not to have been known until 1969. My proof of the implication (32) was based on a theorem proved by the Soviet mathematician Nikolai Nikolaevich Vorob'ev in 1942, but published only in the third, augmented edition of his book [40]. (The translator of my article [21] misled the reader by changing the year of publication of [40] from 1969 to 1964, which was the year when the second edition appeared.)

Using (32) and other properties of the Fibonacci numbers, I proved the following theorem, on the basis of which it is easy to construct a Diophantine representation for the Fibonacci numbers with even indices:

A necessary and sufficient condition for the equality $v = \phi_{2u}$ is that there exist numbers g, h, l, m, x, y, and z such that

$$\begin{gathered} u \leqslant v < l, \\ l^2 - lz - z^2 = 1, \\ g^2 - 2gh - 4h^2 = 1, \\ l^2 \mid g, \\ m = 3 + (4h + g), \\ x^2 - myx + y^2 = 1, \\ u = \mathrm{rem}(x, l), \\ u = \mathrm{rem}(x, 4h + g). \end{gathered}$$

I wrote a detailed proof, not finding any errors while doing so, and asked Sergei Yur'evich Maslov and Vladimir Aleksandrovich Lifshits to check it, but not say anything to anyone for the time being. I had long planned to spend the winter holidays with my fiancée at a ski resort, and I left Leningrad without getting any response from Maslov or Lifshits. For two weeks I was skiing, simplifying the proof, and writing [21]. In that paper I attempted to express the influence of Julia Robinson's paper [36] on my work by the word *naveyano*, for which there seems to be no exact English equivalent,[1] and the translator later used the banal *suggested*.

After returning to Leningrad, I received confirmation from Maslov and Lifshits that my proof was correct, and it ceased to be a secret. Several other mathematicians also verified the proof, among them Dmitrii Konstantinovich Faddeev and Andrei Andreevich Markov, who were both known for their ability to find errors.

On 29 January 1970 my first public address on the solution of Hilbert's tenth problem took place at LOMI. Among the audience was Grigorii Samuilovich Tseitin, who soon afterward participated in a conference in Novosibirsk. He took a copy of my manuscript and asked my permission to give there a talk about my proof. (That may be why the Siberian rather than the Leningrad Department of the Mathematical Institute is erroneously indicated in the English translation of my article [21].) Among those who heard Tseitin in Novosibirsk was John McCarthy. When he returned to the USA, he sent his notes to Julia Robinson, and she passed them on to Martin Davis. Later, at my request, Julia sent a copy of these notes to me also. (The first page of them is reproduced in [34], p. 70.)

[1] The word means literally *wafted*; a better translation would be *inspired* or *evoked*. – *Transl.*

McCarthy's notes consisted of only a few basic equations and lemmas, and I imagine that only such specialists as Julia Robinson and Martin Davis, who had already spent a great deal of time intensively working in this area, were able to reproduce the proof from these notes. Martin Davis did not stop at that, and constructed a similar proof using the Pell equation instead of Eq. (25). This construction was communicated by him on 10 March 1970 at a seminar at Rockefeller University and published in [5]. Similar constructions based on the Pell equation were also published by N. K. Kosovskii [16] and G. V. Chudnovskii [2].

The example of a Diophantine relation with exponential growth, which I constructed, became the last link in the proof of Martin Davis' conjecture and thereby completed the "negative" solution of Hilbert's tenth problem — the algorithm required by the problem does not exist. A complete proof can now be found in many books, including my own [24]. The undecidability of Diophantine equations turned out to be a convenient tool for proving the undecidability of other decision problems. A revised bibliography (containing now over 300 references) of papers expounding the proof of the unsolvability of Hilbert's tenth problem or an application of that result can be found in [43].

One may pose the following question: *Would Hilbert have recognized the proof of the algorithmic unsolvability of Diophantine equations as a solution of his tenth problem*? I think he would. To justify this opinion, I quote from his address [14], in which the problems were posed:

Mitunter kommt es vor, daß wir die Beantwortung unter ungenügenden Voraussetzungen oder in unrichtigem Sinne erstreben und infolgedessen nicht zum Ziele gelangen. Es entsteht dann die Aufgabe, die Unmöglichkeit der Lösung des Problems unter den gegebenen Voraussetzungen und in dem verlangten Sinne nachzuweisen. Solche Unmöglichkeitsbeweise wurden schon von den Alten geführt, indem sie z. B. zeigten, daß die Hypotenuse eines gleichschenkligen rechtwinkligen Dreiecks zur Kathete in einem irrationalen Verhältnisse steht. In der neueren Mathematik spielt die Frage nach der Unmöglichkeit gewisser Lösungen eine hervorragende Rolle, und wir nehmen so gewahr, daß alte schwierige Probleme wie der Beweis des Parallelenaxioms,

At the same time it may be that we are trying to get an answer under unsatisfactory assumptions or in the wrong sense, and consequently have not reached the goal. The task then arises of proving the impossibility of the solution of the problem under the given assumptions and in the required sense. Such impossibility proofs were given even by the ancients when, for example, they showed that the hypotenuse of an isosceles right triangle is incommensurable with its leg. In modern mathematics the question of the impossibility of certain solutions plays a prominent role, and we take it for granted that such old difficult problems as the proof of the parallel postulate, die Quadratur des Kreises oder die Auflösung der Gleichungen 5. Grades durch Wurzelziehen, wenn auch in anderem als dem ursprünglich gemeinten Sinne, dennoch eine völlig befriedigende und strenge Lösung gefunden haben.

Diese merkwürdige Tatsache neben anderen philosophischen Gründen ist es wohl, welche in uns eine Überzeugung entstehen läßt, die jeder Mathematiker gewiß teilt, die aber bis jetzt wenigstens niemand durch Beweise gestützt hat — ich meine die Überzeugung, daß ein jedes bestimmte mathematische Problem einer strengen Erledigung notwendig fähig sein müsse, sei es, daß es gelingt, die Beantwortung der gestellten Frage zu geben, sei es, daß die Unmöglichkeit seiner Lösung und damit die Notwendigkeit des Mißlingens aller Versuche dargetan wird.

the quadrature of the circle, or the solution of equations of degree 5 through root extractions have been satisfactorily and rigorously solved, although in a sense that is not the one originally intended.

It is probably this remarkable fact, along with other philosophical grounds, that produces in us a conviction that every Mathematician certainly shares but no one to date has confirmed with a proof — I mean the conviction that every clearly stated mathematical problem must necessarily be capable of having a rigorous solution, whether one manages to find a solution to the question posed or to establish that the solution is impossible and thereby the inevitable failure of all attempts to solve it. (Cited from the English translation of [14].)

Having agreed that Hilbert would most likely be satisfied by the answer obtained, one may also pose a different question: *Would Hilbert have posed the problem as he did if he had foreseen its "negative" solution*? I think not.

As already mentioned at the beginning of this essay, less space was devoted to the tenth problem than to any other. This problem is stated in a very rigid way, while other Hilbert problems consist of a series of interrelated questions. We have seen that the solvability of Diophantine equations in integers that Hilbert was asking about is equivalent, as a decision problem, to the solvability of Diophantine equations in natural numbers. However, Diophantus himself sought solutions neither in integers nor in natural numbers, but in (positive) rational numbers. Why then did Hilbert not pose solvability in rational numbers as a separate question? The following explanation appears plausible.

One can show that the decision problem of solvability of polynomial equations in rational numbers is equivalent to the decision problem of solvability of *homogeneous* Diophantine equations in integers. Consequently, by proposing *explicitly* only to find "a process" for solving Diophantine equations in integers, Hilbert was *implicitly* proposing to find "a process" for solving equations in rational numbers as well.

Thus, the positive solution of the tenth problem that Hilbert expected would have given us also a procedure for determining the existence or nonexistence of solutions in rational numbers. What does the negative solution of this problem that we have obtained give us for the case of rational unknowns? Nothing. Homogeneous Diophantine equations are a very special subclass of Diophantine equations, and it is very possible that the "process," that Hilbert was asking to devise for all equations, does exist for this class.

By speaking of solutions in integers, Hilbert posed, among a number of related problems, the one most difficult case for the *positive* solution which he expected. If he had foreseen its negative solution, Hilbert would most likely have stated the problem for solvability in rational numbers as a separate question. Based on these considerations, we can now interpret the tenth problem in two senses:

- in the *narrow sense*, that is, word for word as Hilbert stated it;
- in the *broader sense*, including here all the problems whose solution would have followed easily from the positive solution of the tenth problem in the narrow sense that Hilbert expected.

In the narrow sense, Hilbert's tenth problem has been solved, but in the broader sense it remains a rich field for research. In particular, the solvability of polynomial equations in rational numbers can be regarded as Hilbert's tenth problem in the broader sense. Progress in this important area has not been great so far.

Another example of Hilbert's tenth problem in the broader sense is the solution of polynomial equations in the *Gaussian integers*, that is, numbers of the form $a+b\mathrm{i}$. It is easy to see that the equation

$$D(\chi_1,\dots,\chi_m)=0 \tag{33}$$

has a solution in Gaussian integers if and only if the equation

$$D(x_1+y_1\mathrm{i},\dots,x_m+y_m\mathrm{i})=0 \tag{34}$$

has a solution in integers. We can separate the real and imaginary parts:

$$D(x_1+y_1\mathrm{i},\dots,x_m+y_m\mathrm{i})=D_{\mathrm{R}}(x_1,\dots,x_m,y_1,\dots,y_m)+D_{\mathrm{I}}(x_1,\dots,x_m,y_1,\dots,y_m)\mathrm{i}$$

and transform (34) into a standard Diophantine equation

$$D_{\mathrm{R}}^2(x_1,\dots,x_m,y_1,\dots,y_m)+D_{\mathrm{I}}^2(x_1,\dots,x_m,y_1,\dots,y_m)=0. \tag{35}$$

Thus the decision problem of solving polynomial equations in Gaussian integers can easily be reduced to solving Diophantine equations in integers, and therefore can be regarded as a part of Hilbert's tenth problem in the broader sense.

The opposite reduction is less obvious. It was obtained by Jan Denef [11], who thereby proved the undecidability of the analog of Hilbert's tenth problem for the Gaussian integers.

The Gaussian integers are the algebraic integers of the field $Q(\sqrt{-1})$. It is not difficult to see that, in analogy with the transition from (33) to (35), solving equations in the ring of algebraic integers of any field which is a finite-degree extension of the rational numbers can also be reduced to solving a certain Diophantine equation in integers. The converse reductions are known at present only for particular classes of fields (see, for example, the survey [28]), and for the corresponding classes of rings of algebraic integers, the analogs of Hilbert's tenth problem are undecidable.

It is curious to reflect that if we do not restrict the degree of an extension, but are interested in solutions in arbitrary algebraic integers, then, as shown in [38], the corresponding decision problem turns out to be algorithmically solvable; however, we cannot regard this problem as the tenth problem in the broader sense, since there is no obvious reduction of solving an equation in arbitrary algebraic integers to solving a Diophantine equation in rational integers.

In the "definition" of the broader interpretation of Hilbert's tenth problem just given, we spoke of problems whose solution *would follow easily* from a positive solution of the tenth problem in the narrow sense. Thus, this interpretation depends on what we consider "following easily." In this situation it is natural to compare the difficulty of the solution of the given problem on the basis of a hypothetical positive solution of the tenth problem with its difficulty in the absense of such a solution. It turns out that under this view many well-known problems can be regarded as special cases of Hilbert's tenth problem in the broader sense.

The Fermat equation

$$x^n + y^n = z^n$$

does not fall formally under the individual subproblems of Hilbert's tenth problem in the narrow sense, since n occurs in an exponent. Having now at our disposal the Diophantine representation (15), we can restate "Fermat's Last Theorem" as the assertion that there are no solutions in natural numbers of the Diophantine equation

$$\begin{aligned} A^2(x+1, n+3, p, u_1, \ldots, u_m) + A^2(y+1, n+3, q, v_1, \ldots, v_m) \\ + A^2(z, n+3, p+q, w_1, \ldots, w_m) = 0, \quad (36) \end{aligned}$$

where A is the polynomial of (15). Thus a positive solution of the tenth problem in the narrow sense would have enabled us, at least theoretically, to prove or refute “Fermat’s Last Theorem.” In other words, although Hilbert did not include this “theorem” *explicitly* among his problems, it is present implicitly in the tenth problem.

Although such a reduction of “Fermat’s Last Theorem” to the unsolvability of the “genuine” Diophantine equation (36) was not known before 1970, it is not very surprising, since the original statement of this theorem deals with an exponentially Diophantine equation. As a less obvious example, let us consider Goldbach’s Conjecture, which, as mentioned above, was included by Hilbert in his eighth problem. It is easy to see that the set $\mathfrak{G}$ of counterexamples to this conjecture, that is, the set of even positive integers different from two that are not representable as the sum of two prime numbers, is recursively enumerable and therefore Diophantine. Accordingly, we can construct a Diophantine representation of it

$$a \in \mathfrak{G} \Longleftrightarrow \exists x_1 \dots x_m \left\{ G(a, x_1, \dots, x_m) = 0 \right\}.$$

In this notation, Goldbach’s Conjecture asserts that the set $\mathfrak{G}$ is empty, that is, that the Diophantine equation

$$G(x_0, x_1, \dots, x_m) = 0$$

has no solutions in natural numbers $x_0, x_1, \dots, x_m$. Thus a positive solution of the tenth problem would have enabled us to determine whether Goldbach’s Conjecture is true.

The reduction of Goldbach’s Conjecture to the unsolvability of a particular Diophantine equation, which was also unknown until 1970, is less obvious and technically more complicated than the reduction of “Fermat’s Last Theorem,” since we have to deal with prime numbers. The reduction of the Riemann Hypothesis, which also formed part of the eighth problem, is even more complicated. This conjecture tells about the location of the complex zeros of the *Riemann zeta-function*, which is defined for $\Re(z) > 1$ by the series

$$\zeta(z) = \sum_{n=1}^{\infty} \frac{1}{n^z}.$$

Using the fact that recursively enumerable sets are Diophantine, we can construct a specific Diophantine equation

$$R(x_1, \dots, x_m) = 0,$$

which has no solutions if and only if the Riemann Hypothesis is true. This reduction can be based either on analytic function theory or on the known reformulation of the Riemann Hypothesis in terms of the distribution of prime numbers among the natural numbers (for details see, for example, [8] or [24]).

Thus we see that another prominent mathematical problem can be restated as an individual subproblem of Hilbert's tenth problem in its original formulation.

The three famous problems considered above — "Fermat's Last Theorem," Goldbach's Conjecture, and the Riemann Hypothesis — involve numbers. The technique of *arithmetization*, which was developed in mathematical logic and goes back to the paper [12] of Kurt Gödel, makes it also possible to reduce to Diophantine equations problems connected with more complex structures. As yet another example, we consider the Four Color Conjecture, which has been a theorem of Kenneth Appel and Wolfgang Haken [1] since 1976. This is a theorem about the coloring of planar graphs, but without using it we can again construct a specific Diophantine equation

$$C(x_1,\dots,x_m) = 0,$$

which has no solutions if and only if the Four Color Conjecture is true. Thus yet another problem not included in the list by Hilbert, is contained in disguised form in Hilbert's tenth problem.

One should not think that every problem can easily be reduced to Diophantine equations. For example, this is not possible with the twin-primes conjecture, which also forms a part of Hilbert's eighth problem.

We have seen that four famous problems:

- "Fermat's Last Theorem,"
- Goldbach's Conjecture,
- the Riemann Hypothesis,
- the Four Color Problem,

can be restated as Diophantine equations. What good could these reductions do?

It does not make sense to hope to prove Goldbach's Conjecture or the Riemann Hypothesis or to give new proofs of "Fermat's Last Theorem" or the Four Color Theorem by studying the corresponding very complicated Diophantine equations. Rather, these reductions may serve as a "psychological explanation" of the unsolvability of Hilbert's tenth problem. It would be extremely amazing if a uniform "process" could be found that made it possible to obtain the solution of so large a number of problems of this difficulty coming from different areas of mathematics.

On the other hand, two of the four problems just discussed are still open, while the other two have been solved. This means that we can regard the methods developed to solve them as methods for analyzing particular complicated Diophantine equations and try to generalize these methods to other equations. The algorithmic unsolvability of Hilbert's tenth problem tells us that solving Diophantine equations is going to require more and more new methods.

Martin Davis' conjecture is nowadays often called the DPRM theorem, from the last names of the four researchers who made their contributions to its proof. However, the four of them never met together. Julia Robinson's work on the tenth problem is described in her "Autobiography" [33], written by her sister Constance Reid. Martin Davis has shared his reminiscences of his joint work with Hilary Putnam and Julia Robinson in [6] and [7]. I have told of my collaboration with Julia Robinson in the article [23]. I would like to take this opportunity to speak briefly about my meetings with Martin Davis, Hilary Putnam, and Julia Robinson.

I first met Martin in 1970 during the regular International Congress of Mathematicians taking place that year in Nice. On the first day of the Congress he walked up to me and said simply, "I am Martin Davis." This was a pleasant surprise for me, since Martin was not among the registered participants. In contrast, my first meeting with Julia Robinson during the Fourth International Congress on Logic, Methodology, and Philosophy of Science in Bucharest in 1971 was planned in advance, and Julia and her husband Raphael Robinson then came to my native Leningrad for several days.

In the list of participants of the Bucharest Congress I caught sight of the name of Hilary Putnam and asked Julia if she could introduce us. "I can if I recognize him," was her answer. Several days later she pointed him out: "Over there, in the red shirt, is Hilary Putnam." By that time I already knew that he was a proponent of Maoist ideas and it was "recommended" to me to have no contacts with him. Our brief encounter took place many years later, when I first came to the USA in 1989.

My collaboration with Julia had begun even before we met. The proof of Martin Davis' conjecture was constructive and made it possible, for example, to write out explicitly a polynomial representing prime numbers and only them (see, for example, [15] and [22]). Julia and Raphael Robinson were interested in another corollary (the one that the reviewer for *Mathematical Reviews* found improbable): the possibility of fixing M — the number of unknowns in the Diophantine representation (11) of a universal set and thereby also in the Diophantine representation of an arbitrary Diophantine set (12). (It can be shown that the value of M is independent of n.)

In my talk at Nice I reported that this M can be taken equal to 200. That estimate was very rough. Julia wrote to me that she and Raphael had obtained the value $M = 35$. This became the point of departure for our collaboration, which led to the value $M = 13$, published in a joint article [26] in *Acta Arithmetica*.

The choice of the place of publication was not accidental: that volume of the *Acta* was dedicated to the memory of the prominent Soviet mathematician Yurii Vladimirovich Linnik, whom Julia and I had known personally. I was presented to him shortly after I had proved the unsolvability of Hilbert's tenth problem. Someone had told Linnik this news, starting with one of its corollaries: "Matiyasevich can construct a polynomial with integer coefficients such that the set of positive values of the polynomial on the positive natural numbers is precisely the set of prime numbers." "That is wonderful," said Linnik. "It appears that we shall soon know many new things about the prime numbers." It was then explained to him that the main result was much more general: such a polynomial can be constructed for every recursively enumerable set. "That's too bad," said Linnik. "Now it seems that we won't learn anything new about the prime numbers from this."

Before our joint publication in the *Acta Arithmetica*, we had published a brief note [25], containing some auxiliary applications of the technique we had developed for reducing the number of unknowns in Diophantine equations. The choice of the place to publish that note was also not random. In 1973 A. A. Markov reached his 70th birthday. His colleagues at the Computing Center of the USSR Academy of Sciences decided to publish a collection of articles in his honor. I was invited to participate in this collection and proposed that an article should be written with Julia Robinson. She was enthusiastic about having her first publication in Russian, but she asked that her name be printed in full.

She had weighty reasons for wanting this. I was the translator of one of the fundamental papers [35] on automatic theorem proving that had been written by John A. Robinson. My translation appeared in 1970 in a collection of translations of major papers on this topic, in which the readers saw the following names: *Дж. Робинсон*, author of the article translated by *Ю. В. Матиясевич*; *М. Девис*, author of another fundamental paper on automatic theorem proving. In the minds of many these three names were associated with the recently solved tenth problem of Hilbert, and some of them thought that the author of the *resolution principle* — the primary tool in [35] — was Julia Robinson. To add to the confusion, in his article John Robinson thanked George Robinson, whose name, like those of Julia and John Robinson, was abbreviated in Russian as *Дж. Робинсон*.

For a long time I thought that only Soviet readers of this collection of translations fell into this error. To my amazement, the person who translated the monograph [27] *from Russian into English* made the same mistake, ascribing the authorship of the resolution principle to Julia Robinson.

When I was a student, I made an "error of the second kind": I did not identify J. Robinson, the author of a theorem in game theory, which we were being taught in the university, with J. Robinson who studied Hilbert's tenth problem.

My collaboration with Julia Robinson was conducted almost entirely by correspondence. It was the time when e-mail did not exist, and a letter took about three weeks to cross the ocean. One of my letters went astray, and I had to rewrite 11 pages — there were copying machines, but none accessible to me. To send every letter to the USA I had to obtain the permission, just as for a publication abroad. Nearly all of Julia's letters to me and my letters to her have been preserved and were given after her death to the University Archives at the Bancroft Library, University of California at Berkeley.

In 1974, the American Mathematical Society organized a symposium on the Hilbert problems. I was invited to give an address on the tenth problem, but the

Standing: Patrick Browne; *Seated, left to right*: Richard Guy, Martin Davis, Julia Robinson, Yuri V. Matiyasevich and Louise Guy (Calgary, 1982) participation of Soviet mathematicians in the meeting did not receive approval in my country. The speaker on the tenth problem was Julia Robinson, but she proposed that the publication [8] in the Proceedings of the conference be a joint publication with Martin Davis and me. We first discussed by telephone what each of us would write. The final paper — the union of our three parts into a coherent exposition — was done by Martin. I think this paper turned out as Julia had long expected: an exposition of numerous results obtained by logicians in connection with Hilbert's tenth problem, not overburdened with technical details.

My second meeting with Julia took place in 1975 in Canada during the regular International Congress on Logic, Methodology, and Philosophy of Science.

The above photograph was made in Calgary in late 1982, when I spent three months in Canada on an exchange program between the Steklov Mathematical Institute and Queen's University in Kingston, Ontario. At the time, Julia was the President of the American Mathematical Society. This work took a lot of her time, and she withdrew from research. She had traveled to Calgary on the way to a meeting of the American Mathematical Society in order to meet me again. Martin also came to Calgary for several days to meet me and Julia.

Julia and Martin had always expressed a willingness to host me in the USA. When *perestroika* came to the USSR, such a trip became possible, and in 1989

Yuri V. Matiyasevich and Martin Davis (Saint Petersburg, 1999)

I visited the Courant Institute at Martin's invitation. I reiterated the desire I had expressed at our very first meeting in Nice to see him in my native Leningrad. In the long run, Martin came in 1999 (but it was now Saint Petersburg) before our journey to the first international conference devoted to Hilbert's tenth problem, which was taking place in Belgium.

Bibliography

[1] K. Appel, W. Haken. Every planar map is four colorable. Part I. Discharging. *Illinois J. Math.*, 1977, **21**(3), 429–490.

[2] G. V. Chudnovskii. Diophantine predicates. *Uspekhi Mat. Nauk*, 1970, **25**(4), 185–186 (Russian).

[3] M. Davis. Arithmetical problems and recursively enumerable predicates (abstract). *J. Symbolic Logic*, 1950, **15**(1), 77–78.

[4] M. Davis. Arithmetical problems and recursively enumerable predicates. *J. Symbolic Logic*, 1953, **18**(1), 33–41.

[5] M. Davis. An explicit Diophantine definition of the exponential function. *Commun. Pure Appl. Math.*, 1971, **24**(2), 137–145.

[6] M. Davis. Foreword to the English translation of [24], p. xiii–xvii. Available via `http://logic.pdmi.ras.ru/~yumat/H10Pbook`. A version is included in [36], pp. 90–97.

[7] M. Davis. From logic to computer science and back. In: *People and Ideas in Theoretical Computer Science* (ed. C. S. Calude). New York: Springer, 1999, 53–85.

[8] M. Davis, Yu. Matijasevich, J. Robinson. Hilbert's tenth problem. Diophantine equations: positive aspects of a negative solution. *Proc. Sympos. Pure Math.*, 1976, **28**, 323–378. Reprinted in [37], pp. 269–324.

[9] M. Davis, H. Putnam, J. Robinson. The decision problem for exponential Diophantine equations. *Ann. Math.*, 1961, **74**(3), 425–436. Reprinted in [37], pp. 77–88.

[10] G. V. Davydov, Yu. V. Matiyasevich, G. E. Mints, V. P. Orevkov, A. O. Slisenko, A. V. Sochilina, N. A. Shanin. Sergei Yur'evich Maslov (obituary). *Russ. Math. Surveys*, 1984, **39**(2), 133–135.

[11] J. Denef. Hilbert's tenth problem for quadratic rings. *Proc. Amer. Math. Soc.*, 1975, **48**(1), 214–220.

[12] K. Gödel. Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme, I. *Monatsh. Math. Phys.*, 1931, **38**(1), 173–198. Reprinted with English translation in: *Kurt Gödel. Collected Works*, Vol. I (ed. S. Feferman et al.). Oxford University Press, 2003, 144–195. English translation can also be found in: *The Undecidable*. Basic papers on undecidable propositions, unsolvable problems and computable functions (ed. M. Davis). Hewlett, NY: Raven Press, 1965; corrected reprint: Mineola, NY: Dover, 2004, 4–38; *From Frege to Gödel: A Source Book in Mathematical Logic, 1879–1931* (ed. J. van Heijenoort). Cambridge, MA: Harvard University Press, 1967; reprinted 2002, 596–616.

[13] R. L. Graham, D. E. Knuth, O. Patashnik. *Concrete Mathematics*. Reading, MA: Addison-Wesley, 1994.

[14] D. Hilbert. Mathematische Probleme. Vortrag, gehalten auf dem internationalen Mathematiker Kongress zu Paris 1900. *Nachr. K. Ges. Wiss., Göttingen, Math.-Phys. Kl.*, 1900, 253–297. See also: *Arch. Math. Phys.*, 1901, 44–63, 213–237. Included in: D. Hilbert. *Gesammelte Abhandlungen*, Bd. 3. Berlin: Springer, 1935; reprinted: New York: Chelsea, 1965. English translation: *Bull. Amer. Math. Soc.*, 1902, **8**, 437–479; reprinted: *ibid.* (N. S.), 2000, **37**(4), 407–436. Reprinted in: *Mathematical Developments Arising from Hilbert Problems* (ed. F. E. Browder). Providence, RI: Amer. Math. Soc., 1976, 1–34 (Proc. Sympos. Pure Math., **28**).

[15] J. P. Jones, D. Sato, H. Wada, D. Wiens. Diophantine representation of the set of prime numbers. *Amer. Math. Monthly*, 1976, **83**(6), 449–464.

[16] N. K. Kosovskii. On Diophantine representations of sequences of solutions of the Pell equation. *Zap. Nauchn. Semin. LOMI*, 1971, **20**, 49–59 (Russian); English translation: *J. Sov. Math.*, 1973, **1**(1), 28–35.

[17] G. Kreisel. A3061: Davis, Martin; Putnam, Hilary; Robinson, Julia. The decision problem for exponential Diophantine equations. *Math. Reviews*, 1962, **24A** (6A), 573.

[18] G. S. Makanin. The problem of solvability of equations in a free semigroup. *Math. USSR, Sb.*, 1977, **32**(2), 129–198.

[19] A. A. Markov. The impossibility of certain algorithms in the theory of associative systems. *Dokl. Akad. Nauk SSSR*, 1947, **55**(7), 587–590 (Russian).

[20] Yu. V. Matiyasevich. The connection of systems of word-length equations with Hilbert's tenth problem. *Zap. Nauchn. Semin. LOMI*, 1968, **8**, 132–144 (Russian); English translation: *Semin. Math., V. A. Steklov Math. Inst.*, 1970, **8**, 61–67. Available via `http://logic.pdmi.ras.ru/~yumat/Journal`

[21] Yu. V. Matiyasevich. Enumerable sets are Diophantine. *Dokl. Akad. Nauk SSSR*, 1970, **191**(2), 279–282 (Russian); English translation: *Sov. Math. Dokl.*, 1970, **11**(2), 354–358.

[22] Yu. V. Matiyasevich. Primes are nonnegative values of a polynomial in 10 variables. *Zap. Nauchn. Semin. LOMI*, 1977, **68**, 62–82 (Russian); English translation: *J. Sov. Math.*, 1981, **15**(1), 33–44.

[23] J. Matijasevich. My collaboration with Julia Robinson. *Math. Intelligencer*, 1992, **14**(4), 38–45; corrections: 1993, **15**(1), 75. Reprinted in: *Mathematical Conversations* (compiled by R. Wilson and J. Gray). New York: Springer, 2001, 49–60. Available via `http://logic.pdmi.ras.ru/~yumat/Julia`

[24] Yu. V. Matiyasevich. *Hilbert's Tenth Problem*. Moscow: Nauka, 1993 (Russian); English translation (with a foreword by M. Davis): Cambridge, MA: MIT Press, 1993; French translation: *Le dixième problème de Hilbert*. Masson, 1995. Homepage of the book: `http://logic.pdmi.ras.ru/~yumat/H10Pbook`

[25] Yurii Matiyasevich, Julia Robinson. Two universal three-quantifier representations of recursively enumerable sets. In: *Theory of Algorithms and Mathematical Logic*. Moscow: Computing Center of the USSR Academy of Sciences, 1974, 112–123 (Russian). Reprinted in [37], pp. 223–234. English translation available via `http://logic.pdmi.ras.ru/~yumat/Journal`

[26] Yu. Matijasevich, J. Robinson. Reduction of an arbitrary Diophantine equation to one in 13 unknowns. *Acta Arithm.*, 1975, **27**, 521–553. Reprinted in [37], pp. 235–267.

[27] V. P. Orevkov. *Complexity of Proofs and their Transformations in Axiomatic Theories*. Providence, RI: Amer. Math. Soc., 1993. (Transl. Math. Monographs, **128**).

[28] T. Pheidas, K. Zahidi. Undecidability of existential theories of rings and fields: a survey. In: *Hilbert's Tenth Problem: Relations with Arithmetic and Algebraic Geometry* (Ghent, 1999). Providence, RI: Amer. Math. Soc., 2000, 49–105 (Contemp. Math., **270**).

[29] E. L. Post. Recursively enumerable sets of positive integers and their decision problems. *Bull. Amer. Math. Soc.*, 1944, **50**, 284–316. Reprinted in [31], pp. 461–494.

[30] E. L. Post. Recursive unsolvability of a problem of Thue. *J. Symbolic Logic*, 1947, **12**, 1–11. Reprinted in [31], pp. 503–513.

[31] E. L. Post. *Solvability, Provability, Definability: The Collected Works of E. L. Post* (ed. M. Davis). Boston, MA: Birkhäuser, 1994.

[32] H. Putnam. An unsolvable problem in number theory. *J. Symbolic Logic*, 1960, **25**(3), 220–232.

[33] C. Reid. The autobiography of Julia Robinson. *College Math. J.*, 1986, **17**(1), 3–21. Extended version in [34], pp. 0–83.

[34] C. Reid. *JULIA. A Life in Mathematics*. Washington: Math. Assoc. of America, 1996.

[35] J. A. Robinson. A machine-oriented logic based on the resolution principle. *J. Assoc. Comput. Mach.*, 1965, **12**, 23–41; Russian translation in: *Cybernetics Collection*. New Ser., No. 7 (eds. A. A. Lyapunov and O. B. Lupanov). Moscow: Mir, 1970, 194–218.

[36] J. Robinson. Unsolvable Diophantine problems. *Proc. Amer. Math. Soc.*, 1969, **22**(2), 534–538. Reprinted in [37], pp. 195–199.

[37] J. Robinson. *The Collected Works of Julia Robinson*. With an introduction of Constance Reid. Edited and with a foreword by Solomon Feferman. Providence, RI: Amer. Math. Soc., 1996 (Collected Works, **6**).

[38] R. S. Rumely. Arithmetic over the ring of all algebraic integers. *J. Reine Angew. Math.*, 1986, **386**(5), 127–133.

[39] A. Thue. Probleme über Veränderungen von Zeichenreihen nach gegebenen Regeln. *Vid. Skr. I. Mat.-naru. Kl.*, 1914, **10**. Reprinted in: A. Thue. *Selected Mathematical Papers*. Oslo, 1977, 493–524.

[40] N. N. Vorobiev. *Fibonacci Numbers*. Third augmented Russian edition: Moscow, Nauka, 1969. Translation of the 6th (1992) Russian edition: Basel: Birkhäuser, 2002.

[41] M. J. Wasteels. Quelques propriétés des nombres de Fibonacci. *Mathesis, troisième sér.*, 1902, **2**, 60–62.

[42] *The Hilbert Problems* (ed. P. S. Aleksandrov). Moscow: Nauka, 1969 (Russian); German translation: *Die Hilbertschen Probleme*, 4. Auflage. Frankfurt/Main: Verlag Harri Deutsch, 1998 (Ostwalds Klassiker der exakten Wiss., **252**).

[43] `http://logic.pdmi.ras.ru/Hilbert10`

V. D. Milman

Observations on the Movement of People and Ideas in Twentieth-Century Mathematics

Translated by R. Cooke

The title of this article reflects my interpretation of the purpose of this collection. As I understand it, the articles are to be partly historical and partly mathematical. As a result, this article consists of three parts.

The first part contains the words *mathematics* and *mathematicians*, but not mathematics itself; it describes the relocation of Russian (Soviet) mathematics to the West, more precisely, to Israel. I use the word *relocation* rather than *emigration* because of the size and scope of the process. As it happened, I found myself at the center of that event.

The second part is a short historical remark on the ancestry of Banach and the origin of the term *Banach spaces*, so that this part is closer to mathematics.

In the third part I present a little mathematics, including some of my recent observations on functional analysis, a field that played a large role in the mathematics of the mid-twentieth century and then blossomed into a large number of new areas. Here I shall sketch a picture of an area that is just now in the process of separating from functional analysis. Provisionally, we call it *asymptotic geometric analysis*, although I am not sure that this name will stick.

1. From Russian to Israeli Mathematics

The emigration of mathematicians from the Soviet Union to Israel began in the early 1970s, and as a result the so-called “Russian” mathematics began to relocate to the West. This emigration of mathematicians had a significant effect on both the West and Russia as early as the 1970s, and it became an avalanche in the 1990s. Every mathematical center in the West was touched and enriched by this movement. But only a few people understood that, while beneficial for these individual centers, it bore elements of tragedy for mathematics as a whole.

At one stage, the Russian mathematical school looked as if it might disappear altogether, but today it can be said that the reality turned out to be less dramatic. Despite a hundredfold difference in salary, many first-class mathematicians remained in Russia. It is particularly gratifying to observe a very young generation of outstanding students graduating from Russian universities (as well as Ukrainian and other universities). The best universities of the West (and Israel) are striving to get them as graduate students.

The concept of the “Russian mathematical school” is distinct from the concept of “the Luzin school,” “the Kolmogorov school,” or “the Gel’fand school,” although it includes these schools and many others. This concept, which is extremely difficult to explain to a Westerner, encompasses traditions that prescribe ways of studying mathematics and a code of behavior for mathematicians. It is more an intellectual necessity (and a game) than it is work. Scholars raised in the traditions of the Russian mathematical school do not study mathematics for the sake of a salary. That is why the “chats” in the corridors of mathematics departments go on for hours at a time; and that is the most effective forum for studying and exchanging the latest mathematical news. That is why seminars have a beginning but no definite end, and a seminar lasting less than two hours is inconceivable. A “Russian” mathematician wants to know everything. In the Russian school the need to know is a drug; it replaces vodka (and goes with vodka).

But, to return to the West, where Russian scholars streamed: there have been, and there are, Western mathematicians who understood the tragedy of the decline of the Russian school and what it meant for the development of worldwide mathematics and made titanic efforts in an attempt to halt the process — to help their colleagues in Russia.

I shall give just one example — Pierre Deligne. In his letter to the president of the American Mathematical Society, a copy of which he sent to me, he called attention to the decline of high schools with a mathematical bent (meaning schools that nurture children in intellectual, scientific, and especially mathematical traditions). He regarded the decline of such schools, and the movement *en masse* of their teachers to the West, as a tragedy for the future of mathematics.

I wrote back to him that in Israel we were making an effort to support specialists in mathematics education and to found, if not schools, at least classes for specialized education, in order to transfer the Russian mathematical school to Israel and preseve that tradition. I think some enthusiasts succeeded in getting such classes going and even possibly whole schools. Time will tell...

But my short narrative is about the relocation of large numbers of mature mathematicians to Israel. I happened to be at the center of all the events connected with their reception and absorption, from the very time I arrived in Israel in July of 1973 up to the late 1990s. I shall describe some events connected with this relocation of science, tell some interesting and even "unbelievable" stories, present some impressive numbers about the migration, and explain how we succeeded in finding places for so many researchers and how the face of mathematics in Israel changed after their arrival.

The emigration of the mid-1970s had already brought mathematicians of the highest caliber and of all ages to Israel: Mikhail Lifshits and David Milman, Israel Gohberg and Il'ya Pyatetskii-Shapiro, Shoshana Kamin, Boris Moishezon, Yurii Gurevich and I (I include myself in this group). It also brought some very young ones: Yosef Yomdin, Il'ya Rips, Yurii Kifer, Grigorii Sivashinskii, and others. The overwhelming majority of these were hired by Tel Aviv University, which was quite young at the time, but also the Hebrew University of Jerusalem and the University of Beersheva. Later on, the Technion and Haifa University, which had just been founded at that time, also began to notice the Russian emigrés. Still later, in the early 1980s, they were joined by the Weizmann Institute in Rehovot and Bar-Ilan University. By that time the interest of the Hebrew University of Jerusalem in the Russian mathematical emigration had grown noticeably cold. At the same time, the Weizmann Institute had been strongly interested; Bar-Ilan and the University of Beersheva and to some extent the Technion were developing on the basis of the Soviet mathematicians arriving in Israel. But that was in the 1990s.

Fortunately for the scholars arriving from Russia in the 1970s, Professor Yuval Ne'eman, who was the president of Tel Aviv University at the time, understood what a unique opportunity the Soviet emigration was opening for scientific development in Israel. Books have been written about Ne'eman, who was both a general and a famous theoretical physicist; what is important for our story is that in those days he was a councillor and the main strategist of the scientific development of the country for the political elite of Israel. After becoming the president of a peripheral university in Tel Aviv, which had just separated from the Hebrew University of Jerusalem and had been promoted from the status of a dependent college to that of a new university, Ne'eman succeeded over a period of several years in turning it into the largest institute in the country. To this day, the establishment at the Hebrew University of Jerusalem has not forgiven him for that. For example, I heard the following amusing statement, made by a prominent Israeli mathematician: “Well, of course he [Yuval] borrowed hundreds of millions of lire [about 20 million dollars] from the banks to develop the university, and then the government had to cover the debts!” Not a word, not a hint of gratitude for creating a powerful new scientific base in Israel!

Thus, Ne’eman had a fine understanding of the importance of the Russian emigration for the development of science and the opening of new scientific research areas in Israel. Science in Israel, and mathematics in particular, had been confined to a few areas. (As an example, logic was begun by Fraenkel in the 1930s.) With the arrival of Furstenberg and Weiss from the USA ergodic theory began to develop in Jerusalem. But until the arrival of Pyatetskii-Shapiro, Israel had no specialists in representation theory; until Moishezon came, there was no one in algebraic geometry. These are now highly developed fields in Israel.

With the wave of emigrés of the 1990s the number of research areas represented by Israeli mathematicians became so extensive that diversity of areas is no longer an issue. Freud used to say that when your head doesn’t ache, you don’t think about it. It suffices to say that at the International Congress of Mathematicians in Berlin in 1998 the Israeli invited speakers were represented in eight sections, that is, nearly half of all sections! Moreover, six of the nine invited speakers were Russian emigrés.

I arrived in Tel Aviv with my family on 25 July 1973. By the 28th of July the president of the university had already scheduled an appointment with me. Naturally, I remember this meeting. The sense of a substantive conversation has been preserved in my memory, even though I did not know a single word of Hebrew and hardly knew a word of English. On the other side only Ne’eman’s secretary understood a few Russian words, but very badly. But somehow I understood a great deal, and he understood a great deal, including the steps necessary to increase the emigration of Jews from Russia — Yuval understood what was happening in Russia as if he had lived there. And mainly, he understood what needed to be done for me personally. I was invited then to work as a professor at the University starting from the first of August.

Much of what I succeeded in doing subsequently for the reception and settling of mathematical imigration in Israel was due to the support of Yuval Ne’eman. And it was all set up during that meeting on the third day after I arrived in the country.

We later became very close, and I think I remained his main advisor on all questions involving the resettlement of mathematical emigrés. This became especially important during the 1990s, which were years when a huge human wave was breaking on Israel (some 200,000 emigrés in 1990, and almost as many in 1991, while the Jewish population of Israel at the end of the 1980s had been less than 4 million). By 1993 the number of emigrés with an advanced degree in mathematics exceeded one thousand! All these people had to be settled (more precisely, fitted in) according to specialty and knowledge so as to use their scientific potential.

I can hardly convey the intenseness of those days and explain how it was done. The plain fact is that there are now essentially no unsettled mathematicians in Israel. (Unfortunately, there are unsettled physicists.)

Yuval Ne'eman was the minister of science in those critical years, and later minister of energy. I had his permission to phone him at any time and to meet him in any emergency; this enabled me to deal with the problem of the hundreds of arriving scientists. However, while in the 1970s and again in the 1980s (which were virtually devoid of emigrés) Professor Ne'eman was almost the only representative of the establishment who understood the importance of the scientific emigration, by the beginning of the 1990s we were "standing" on several "pillars."

By the early 1990s a mathematician had entered the Israeli establishment, Professor Dan Amir, who became the assistant rector and then the rector of Tel Aviv University. We became friends during my first months in Israel. He worked in the same areas that I worked in, and we wrote some joint papers. Over the entire 27 years that I have lived in Israel, the mildness of his personality has not opposed, but rather complemented, a certain harshness of my own. We have been, and remain, a good team. His photographic memory retained the names and details of thousands of arriving scholars. With his help, hundreds of them found at least a temporary respite at Tel Aviv University. Moreover, he was very close (a friend from school years) to the then head of the Council on Higher Education, who is also a mathematician, Professor Ammon Pazy. Together they constitute the second "pillar." I must emphasize that the role of Dan Amir has been much more important than his mere connection with the Council: he had many other contacts in various spheres that were needed for success.

Finally, the president of the Israeli Academy of Sciences in those days, Professor Yehoshuah Jortner (an outstanding chemist who, in particular, received the Wolf Prize in chemistry), was interested in increasing the "weight" of Israeli science by using the Soviet emigration. He managed to bring significant resources to absorb the scientific elite among the emigrés. For example, the *Barecha* project was his program, aimed at the highest level. Through this program each participant received 80,000 dollars of support to purchase accomodation and 40,000 dollars for scholarly activity and setting up a laboratory.

The program arose, like many others, as a necessity to settle particular people. Namely, Grigorii Margulis, Vladimir Drinfeld, and Gennadii Henkin asked me to send them a written invitation. Something had to be done for them, and Jortner did it. And all three eventually went elsewhere. Unfortunately we, in Israel, were often — too often — merely a backstop. However, the program worked and 16 scholars were settled in Israel, perhaps not all of the level that we aimed at.

Let me give another example of Jortner's activity. On behalf of the Israel Academy of Sciences he sent me to Moscow and Leningrad in September of 1990. In order to do so I had to cut short my participation in the Mathematical Congress in Kyoto. My wife and I then returned to Israel for only 12 hours and immediately flew to Vienna, where our visas to Russia did not arrive until late at night. By early morning we were flying to Moscow, together with the family of Professor Shalom Abarbanel,an applied mathematician and (in the 1970s) the rector of our university. (One of the many piquant details of this trip: Shalom Abarbaneland his wife had dual citizenship and were traveling with us on American passports with a single purpose — to make sure that nothing unforeseen happened to my wife and myself in Russia. We weren't entirely joking when we called them our bodyguards.) The purpose of our trip was to estimate the size of the expected scientific emigration and its "swath" in levels and areas of research, so that the country could prepare itself to receive the scholars.

Thus, by the early 1990s the leaders of Israeli science were prepared to give active support to the stream of emigrés from Russia. Yuval Ne'eman was the minister of science; Dan Amir was the assistant rector and later the rector, who also represented the Council on Higher Education. Yehoshuah Jortner was the president of the Academy of Sciences. All three were professors at Tel Aviv University in the Faculty of Exact Sciences (physics, mathematics, chemistry). All of them regarded me as their advisor on questions of scientific emigration to Israel. (Perhaps I was the only one with this status.) Of course, many other influential people in the scientific and political establishments of Israel were willing to render some help on occasion. And many scholars from the stream of the 1970s donated their time to help the newly arriving people in the years from 1990 to 1994.

With such a distribution of forces, the financing of the short-term settlement of the scholars was the least of the problems: the necessary funds were obtained from the government (for example, Yuval Ne'eman created several hundred grants through the budget of his ministry) and by means of large donations (not very fast, and not without the heavy work of many people, but that is another story). However, the main problem — long-term settlement — remained. And it was, unfortunately, not the only problem.

In the university system the appointment of a scholar begins with the corresponding department announcing its desire to make the appointment. The question of financing is dealt with later. I shall give just one example from the past to show how nontrivial this problem is.

Issai Schur and Otto Toeplitz fled from Germany to Israel in 1939. They were not yet 70 years old, but they could not find work either at the Hebrew University of Jerusalem, which was the only one at the time, or anywhere else. (It is true that Israel did not have Tel Aviv University at the time.) They both died about a year and a half after they arrived, both from heart attacks, as I recall. I leave it to the reader to decide: Were there mathematicians in Israel of the stature of Schur?

As early as the late 1970s the mathematics department at Tel Aviv University was eager to appoint Russian mathematical emigrés, perhaps because it had been established on that basis. Between 1973 and 1978 alone, the following people were hired there (in chronological order): Boris Moishezon, Vitali and David Milman, Israel Gohberg, Boris Korenblyum, Il'ya Pyatetskii-Shapiro. The active search for appointments in the department had begun back in Russia.

Here is an interesting example. In 1981 the president of Tel Aviv University, Professor of Economics Ben-Shahar, negotiated with the multimillionaire Armand Hammer. (Background information: the duties of the president are different from those of the rector. The president is elected by the Board of Trustees and deals with the problems of financing the university, while the rector is elected by the Senate, that is, all the full professors of the University, and is responsible for the scholarly and pedagogical activity.) This was the same Hammer who in his youth had dealt with Lenin and then supported business contacts with all the governments of the Soviet Union. Hammer did not seem willing to make a donation to the University, but he was willing to do something. Then the idea arose that he might "ransom" some *refusee* scholars for us. And Hammer agreed.

As always, I had to prepare a list and documentation. Naturally, representatives of all university professions would have to be considered; but, also naturally, the majority were mathematicians. (And not only because I was the one who made up the list, but also because our department was prepared to hire them; although I had been unable to discuss the question at departmental meetings, no doubt of their willingness arose.) I recall three names of those who were chosen after discussion: Yakov Eliashberg, Abram Kagan, and Mark Freidlin.

I leave aside the piquant details of the negotiations. For example, we were supposed to be sure that they would all come to Israel, if Hammer "ransomed" them. At the time that was not clear in regard to Eliashberg. (I got Gromov involved in this matter, and he made a telephone call to Yasha to get his OK.)

Hammer seems to have agreed with Brezhnev on the deal, since he got ready to fly to Moscow – on his own plane, naturally – and bring them all to us. However... Brezhnev died, and the deal fell through. Hammer wrote to us that he needed some time to get in contact with the succeeding Soviet leaders, but, as we know, they changed too frequently at that period.

As I mentioned above, in the 1990s there was no lack of desire to hire mathematicians arriving from Moscow at the universities of Bar-Ilan and Beersheva, at the Technion, at Tel Aviv, and later at the Weizmann Institute (Rehovot). But large sums of money were needed. These were sought through various channels.

For example, the RAShI Foundation established the *Guastella* program, for about 25 positions per year all over Israel for scholars at most 48 years old who had reached at most the rank of *haver professor* – approximately that of an opper echelon of associate professor in American terms. Later we abolished this restriction. Then we raised the age of eligibility for participants till 58, first only for a year and then for another year. As always, this was done for a particular person, Genrikh Belitskii in this case. But many others were then appointed by means of this loophole. When the stream of emigrés abated, the Foundation reduced its participation to 3–5 positions per year and only continued it at all thanks to the personal influence of Dan Amir. Foundations are interested in only grandiose projects. And they are right: the universities should solve problems of a small number of people. But how can they be solved when all the funds are exhausted? Hence comes the paradoxical but understandable principle that it is easier to find work for many people arriving with a large stream of emigration, than for those who arrive with a small trickle.

And here is an example of a failure that is also instructive. In the attempt to procure funds for support of scholars of both pre-retirement and retirement age, I wrote a letter to the superbillionaire Leslie Wexner. By chance, while I was at a meeting in Columbus, Ohio, where he was speaking about Jewish emigration, I sensed a "kindred spirit" in his arguments and the form in which he presented them. Of course, the university bureaucracy participated in every step. The rector at the time, Professor of History Itamar Rabinovich (later Israel's ambassador to the United States), found a way to get my letter onto Wexner's desk.

It was a modest request, about two million dollars. Then came January 1991, and the war broke out in the Persian Gulf. In an interval between SCUD attacks, when it seemed that Saddam Hussein had run out of them and that there would be no more attacks, I flew to Columbus to meet with Wexner.

To the surprise of all, he gave me a 45-minute appointment. That is a lot of time for such a man. It was explained to me that if the interview ended ahead of time, that meant everything had fallen through, but if it went overtime, that meant he liked me and everything would be all right.

When the time of the appointment was set, I was already in Columbus, and there was no one to instruct me on the details of how to behave. I found a videotape of his speech in order to shorten the time required to get acquainted at the interview. After all, you have only a few minutes to get to know the man and get into the rhythm of the conversation.

It was explained to me that he gets involved only in large projects and is not interested in minor ones. So I changed plans on the fly – I prepared a proposal to establish an Institute for Advanced Studies with an investment of 25 million dollars. Was that a mistake? I really don't know. But I didn't have papers with details yet. (He was to ask me for them at the end of the conversation.)

It was the time for the appointment. I arrived at the headquarters of his company too early – a mistake, but I was afraid I'd have trouble finding it and be late. It was a vast territory with many buildings inside and security at the entrance. I was told that it was too early, but that I could go to the administration building. I then waited in the vestibule of the building for another ten minutes, getting nervous. I had arrived early on purpose, thinking that I would wait in his reception room and would be able to talk with his secretary so as to get a feeling for the atmosphere. At last I was invited to go up to the first floor. He was waiting for me on the stairs. It was a good thing that I knew him by sight from the videotape. Wexner ushered me into his office, through a room where two secretaries were sitting.

Here I must digress. Everything that we know about such people has been picked up from serials like *Dallas*. I had figured that I would be sitting in a huge reception room (it turned out to be a small, ordinary office with cabinets full of files, divided in two for the two secretaries), that I would walk about in a large office, trying to appear nonchalant while they looked me over.

At that point I entered . . . the office of my dreams. It was a room of average size (who needs anything bigger?), there was a continuous table along the walls with a computer, a telephone, a chair, and a small sheaf of papers every couple of meters. (I had always dreamed of having a separate desk for each problem and project I am working on.) In the center of the room was an oval table (for meetings?) – not small, but not for showing off. The chairs were comfortable but not luxurious. They were for sitting on, not to impress people.

We sat side by side and I had the feeling that I knew and understood this man: he is something of another "I," who had studied business rather than mathematics, not a *nouveau riche* from Dallas (or one of those *millionerchiks*[1] – as I call them since meeting Wexner – with whom I have to deal in my university). The conversation flowed easily and simply, but it lasted an hour! Fifteen minutes

[1] This is the word *millioner* with Russian diminutival *chik*. – *Eds.*

longer than planned. In parting he said, "I know everything about you, but you don't know anything about me." Turning to his secretary, he said, "Bring the materials on me." And he gave them to me.

However, just as we don't understand people from the world of big business, they don't understand us either. At the end of the conversation Wexner telephoned some physicist in New York — his assistant, as he explained it (science advisor, I thought). "He understands your language (meaning science) and you can discuss the details."

A day later I was to be in New York and fly out at night from there to Israel. More SCUDs had fallen, and I did not want to leave my family alone. To find out who I would be discussing the details with, I telephoned Dima Kazhdan at Harvard. He made an exhaustive search, but couldn't find such a physicist. Only through my own university did I learn that the person in question had only a bachelor's degree in physics from Stanford University. Actually, he had completed only three years of study — an incomplete higher education, as we would say in Russia — and then gone into business. He had many hundreds of millions of dollars and worked for Wexner.

Just as we cannot tell the difference between people who have tens of millions of dollars ("incomplete higher education") from those who have several billion, they cannot tell the difference between a professor and a person who does not even have a master's degree!

I think I didn't understand this "assistant." Everything was just like in *Dallas*: A long black limousine that picked me up and took me to Madison Avenue, a secretary who met the limousine and accompanied me to a large headquarters — a floor with a separate elevator, with Renoirs on the walls (which, however, turned out to be copies), and so forth.

After my meeting with Wexner I conducted myself with confidence and made myself right at home, whereas, it appears, I should have presented myself as weak and humble. Still, they spent some seven hours on me and asked me to send a detailed proposal (to which I never received either a "yes" or a "no").

This was the first very serious project that I participated in — there had been smaller successful ones earlier — and it was no wonder that it fell through, although it came very near to success. Raising donations is also a science, and one must be an expert to succeed in it. I later learned many important things needed for success in such matters, but even that might not have been enough for such a grandiose project as I was pursuing at that time.

As things turned out, we solved the problem of settling the older generation of arriving scholars with our own resources. The Council on Higher Education established a special program for famous scholars aged 59 and above, which dealt with the most acute problem, the issue of pensions. Once again, a program was established for the sake of a particular person (Yurii Lubich), but during two or three years we got 22 such positions and six more have been added recently (again because of the need to hire another mathematician, this time Viktor Palamodov).

Through this program, in addition to the mathematicians, some very well known physicists were accepted, such as Isaak Khalatnikov, the former director of the Landau Institute of Theoretical Physics and Yuzik Levinson, who was awarded the State prize. Other beneficiaries include the very well known biochemist and corresponding member of the USSR Academy of Sciences, Lev Bergel'son; the neurophysiologist Professor Mark Shik; and the specialist in art history Mikhail Libman.

Looking through these notes, I saw that I have devoted the largest amount of space to describing our failures (with Hammer and Wexner). Actually, these were the only failures that I can think of, not counting the fact that Margulis and Drinfeld did not come to Israel. (But I don't think there was anything we could have done to change that.) For that reason, I shall now balance my narration by telling two successful stories.

It was May 1991. The University decided to give me a special award during the week of meetings of the Board of Trustees for my efforts to absorb scholars. The award was only a pretext. The real purpose was to take advantage of the ceremony as a suitable occasion to make a speech with a call for support.

I spoke for about ten minutes in very solemn surroundings. On the presidium were the heads of the societies of Friends of Tel Aviv University from many different countries. My wife later told me, "They had tears in their eyes when you were speaking; then they took out their calculators and did some computations." But my own voice was also breaking. The president of the University (Professor of Medicine, Moshe Mani) came up to me during the reception and said in my ear: "We've already gotten a million dollars!" I was told that later the videotape of that affair went the rounds of various Jewish organizations and invariably brought in donations for the absorption of scientists.

Here is another prosaic story that enabled us to settle some 50 mathematicians for several years. In 1992 there was a change of administration in Israel. Rabin and his party, the *Avoda*, replaced Shamir and the *Likud*. Such situations are usually accompanied by large budgetary changes — a rearrangement of priorities takes place. The new authorities may slow or even halt expenditures; but in fact they did not even get time to plan what to spend the money on. As a result, on a certain day in late December a report of unexpended budgetary allocations lay on the desk of the minister of finance. The minister was free to dispose of these any way he chose. But the new minister still didn't know what he wanted, and for that he may be *assisted*.

In early December representatives of the majority of mathematics departments, charged with settling the new emigrés, assembled in my office in Tel Aviv. (There was no one from Jerusalem; no one there was responsible for the almost nonexistent emigrés.) We calculated that some 35 to 40 mathematicians had been appointed to universities throughout Israel for either tenured or tenure-track positions. Another 100 to 110 were connected to the universities or newly founded institutes of mathematics, such as the Institute of Industrial Mathematics in Beersheva, which continues to develop even now; the Institute of Mathematics in Afula, which was a branch of Haifa University, but no longer exists; and the mathematical centers in the colleges, in particular in Ariel under the auspices of Bar-Ilan University. However, the financial support of about 50 positions was coming to an end, and the situation of the scholars was tragic.

They were being supported by a special program, the so-called *maagarot* (reservoirs). We had established it back at the beginning of 1990 when Yuval Ne'eman was the minister of science, but with financial support from other ministries as well. A budgetary request for a "reservoir" was supposed to come from a department interested in it, and be awarded to this department. The request was supposed to describe the number of people, but not mention them by name, so that people could be put into the "reservoir" immediately, without unnecessary bureaucracy; and we could even negotiate with them in advance, while they were still in Russia. The reader will perhaps no longer be surprised when I say that nearly all of the *maagarot* were used for mathematicians. The "reservoirs" could theoretically have been 5 to 10 times more, so that we were not taking them away from other areas.

The condition of the newly arrived physicists was much worse: the physics department at our university showed no initiative or active interest. Many of newly arriving physicists worked in areas close to mathematical physics. For that reason the mathematics department was able to take them under its wing. A "reservoir" was not particularly needed by the mathematics department for hiring mathematicians — we knew how to do it without "temporary" solutions. But once, just a week before I left for the USA, I decided that we had to take the initiative and establish a "reservoir" to help physicists. Usually a request for a *maagara* was considered for several months and signed by the minister, the general director of the ministry, and so on. I realized that my request would get through faster, but I had no idea that we would get official confirmation of our *maagara* even before my departure. In the rush of paperwork, no indication had been made of the number of people in the "reservoir," that is, we had received carte blanche. However, we used it very sparingly.

But let us return to the meeting in my office. There was a problem with the budget for 1993: There were no funds for the *maagarot*, and the new ministers didn't know what they were. A catastrophe was looming.

Therefore at our meeting we drafted a "politically astute" letter to the minister of finance, signed formally in our name by the head of the Israeli Mathematical Union, the representative from Beersheva University, Professor Miriam Cohen. It was also vital to get the letter onto the desk of the minister on just the right day. (In political circles, that is called "influence".) This was done, and our *maagarot*–reservoirs were extended with full financing.

I have described only a few of the numerous programs and methods established since the early 1990s for the reception and "fitting-in" of Soviet/Russian scientists in Israel. In addition to the "reservoirs" there were also "hothouses" — called *hamamot* — for applied areas (and again applied mathematicians passed through them in large numbers). In addition to the elite *Guastella* and *Barecha* programs, programs for aged scholars, there were and are the so-called Shapiro fellowship, the *Giladi* and *Kamea* programs, which alternate with one another.

A "fellowship" is intended for the first appointment of all scholars in general; a selection of the better researchers among them is made for the *Giladi*, and an even higher level for the *Kamea*, participation in which essentially amounts to a tenured position in the universities, colleges and scientific centers. To date some 500 positions (!) have been planned and almost 300 already assigned. And in each of these programs a large portion of the positions have been occupied by mathematicians.

Today about 20 to 25 percent of the professors of mathematics in Israel came from Soviet schools. Some 40 percent of the invited talks by Israelis at International Congresses of Mathematicians were presented by Russian emigrés. All three of the Israelis who were awarded the European Prize for Young Mathematicians (Leonid Polterovich at the Budapest Congress in 1996, Semen Alesker and Denis Gaitsgori at the Barcelona Congress in 2000) were Russian emigrés. On the other hand, they all received the Ph. D. at Tel Aviv University, even though Polterovich arrived in Israel as a mature mathematician.

As a result of this explosion of talent, the worldwide status of Israeli mathematics has changed: Israel has passed from the next-to-last group in representation at the International Mathematical Union (IMU), which it belonged to until 1990, to the highest league. At the sessions of the General Assembly of the IMU in August 1998 Israel was represented by five votes, just like Russia, the USA, Britain, France, Germany, Italy, Canada, China, and Japan.

The rise was rapid. By 1990 the representation of Israel had already increased from two to three votes. Then at the first opportunity, at a session of the

General Assembly in 1994, it rose to four, and in 1998 to the maximum possible — five! Naturally, the increase in number of active mathematicians in Israel and the mathematical activity as a whole was the leading factor in this process.

The influence of Russian mathematical traditions is enormous. It shows up not only in the development of new areas of research in Israeli science, but also in the style of the seminars, in the conversations in the university corridors, in the number of students interested in mathematics and in their level.

I think we can now say confidently that the Russian mathematical school and its traditions will be preserved; they will take root in a new country and a new environment.

2. The Ancestry of Banach and the Origin of the Term *Banach Space*

Let me begin with a small remark on the origin of the term *Banach space*. I heard it from my father, David Milman, co-author of the Krein–Milman theorem and the founder of the geometric study of infinite-dimensional normed spaces.

In his book *A Course in Functional Analysis*[2] Banach denoted operators by the letter A. These were the initial objects of study, and the complete normed spaces on which they operated were denoted by the Latin letter B. That was natural, and there is no indication that he was "hinting" at his own name by using that letter.

Functional analysis had only just begun (this was in the mid-1930s), and two young scholars, Vitold Shmul'yan and David Milman, started writing "Banach space" in their papers instead of "B-space," as others did. Soon everyone switched to the new language.

Now let us speak of the origin of Stefan Banach. It is known that he grew up in an adopted family, but my story will be about his biological family. In Polish biographies of Banach it is written that the details of his childhood are not known, that he never knew his mother or father and therefore (?) he earned his living by tutoring from the age of 15. It was believed ("everyone believed," as Steinhaus wrote) that he somehow wound up in the family of a laundress named Banach shortly after he was born, and that she took care of him.

In contrast to such sparse information, it is pointed out that his father was a man named Greczek, who worked in the administration of the Krakow railroad. According to information from Steinhaus — one of the closest people to Banach

[2] This book was translated into Ukrainian in 1948 from the 1931 Polish edition of the book *Theory of Operators*; no Russian translation was ever published.

in the mathematical world, who knew him in the 1910s – his father had no contacts with Stefan at all. If so, the fact that his father's name is indicated, while nothing at all is known about the mother or childhood of Banach is puzzling. I note, however, that according to the book of the Polish reporter R. Kaluza (*Through a Reporter's Eyes: The Life of Stefan Banach*, English translation, Birkhäuser, 1996), which was written for the hundredth anniversary of his birth, Banach had a warm relationship with his father, who always took care of him (but for some reason didn't give him his name?).

Stefan Banach
(1892–1945)

Through a fortuitous concatenation of circumstances, I know a different story. The reader may regard it as an unfounded legend, although I personally have no doubt of its complete veracity. Judge for yourself.

The facts. For the entire 27 years that I have lived in Israel I have worked in the mathematics department of Tel Aviv University with Professor of Applied Mathematics Bernie Schiff. He died prematurely in December 1999 at the age of 68. All those years I knew that the maiden name of his wife Miriam was Banach, and that she had heard a legend about her grandmother's younger brother, who abandoned his Orthodox Jewish family at an early age and was baptized and renamed by the church as an adopted son in some family.

After Bernie's death several professors in our department decided to meet with Miriam Banach-Schiff in order to get the details. She knew very little: nearly the entire family perished in the Holocaust. Her father left for the Netherlands in 1930 and thereby survived. But I shall relate what she knew.

Her grandfather and grandmother – Moishe and Netl – were second cousins, as was often the case in traditional religious Jewish families in the late nineteenth and early twentieth centuries, and both bore the name Banach. Miriam did not know the exact year of her grandmother's birth, but Miriam's father was born in 1907, which may give some idea of her grandmother's age. I recall that the official date of birth of Stefan Banach was 1892.

Her grandmother's younger brother left the family at an early age and became a Catholic. One must realize that for a deeply religious family this was a great tragedy. (Miriam and her entire family belong to the extreme orthodox religious movement even today.) Therefore all relations with the younger brother were broken off, and it was considered bad taste even to show interest in news about him. Nevertheless, certain information penetrated to the family; possibly the older sister Netl wanted to know what her younger brother was doing.

Miriam's uncle — that is, Netl's son — told her that this brother, whose first name Miriam did not know, had studied at a polytechnic institute, and that one of the mathematics professors had recognized the capable student and helped him to develop his talent. Later on, as they heard, he became a professor.

When they were shown a picture of Stefan Banach, all the members of the family confirmed that he bore an uncanny resemblance to one of Netl's sons — Joseph Banach.

Miriam told me that many years ago her husband Bernie showed her an old photograph — a group picture of the participants in a conference where there were about 50 people — and asked her if she could find Banach in the picture. She did not know what the mathematician Stefan Banach looked like, since she had never seen his portrait, but she unerringly picked him out in the group photograph on the basis of his resemblance to her uncle.

Miriam's grandfather Moishe Banach was born in the hamlet of Tarnovskie Gory (Galicia), a large railroad hub near Krakow. Miriam was not sure that her grandmother and her younger brother (Stefan?) were born in that exact place, but it was probably not far from there.

I would like to end this story with a speculative remark of my own. It was the late 1930s. A vicious war was looming (or already going on), in which a person of Jewish descent could not survive. To have "holes" in your biography or your ancestry puts you in mortal danger. If one believes the story above (and I do), Banach's ethnicity could have been determined by pulling down his trousers. For that reason, in order to "patch" the holes a story was concocted of an unknown father and mother, that he was perhaps the illegitimate son of a white-collar worker. And the choice of the "father's" name might not have been random: his godfather (Greczek?) may have "turned into" his father. As it happens, Greczek's first name was also Stefan. It was necessary that everyone believed the story and that no questions arose. And everyone believed it.

The war was coming to an end. Exhausted by the war, Banach died soon after the liberation of Poland, but the legend lived on and has now been embellished with details.

3. From Functional Analysis to Asymptotic Geometric Analysis

3.1. Classical (Infinite-Dimensional) Functional Analysis

Functional analysis arose in the early twentieth century and assumed the form familiar to us due to the almost unmatchable strength and scope of the Polish mathematical school at that time. A dozen names of first-class mathematicians, in addition to Banach, are embedded in the names of the classical theorems and need not be recalled here. Undoubtedly, functional analysis was a major force in the development of analysis throughout most of the twentieth century. Many problems and areas of research in classical analysis were recast and got their second wind in the framework of functional analysis. The various kinds of convergence and classes of infinite-dimensional spaces that arise so naturally in the context of classical analysis led to the development of *topology* and the concepts of normed and topological spaces, along with the concepts of *completeness* and Banach spaces. Linear algebra and the Fredholm theory of integral operators led to the development of operator theory. Existence (and uniqueness) theorems for various kinds of equations, including integral and ordinary and partial differential equations, crystallized the concept of *compactness* and led to numerous fixed-point theorems, the Sobolev embedding theorems, and interpolation theory. The requirements of physics (and, once again, classical analysis and partial differential equations) led to the theory of (unbounded) self-adjoint operators and Schwartz distributions.

Next comes the development of *algebraic analysis*, which began with the papers of Gel'fand in the late 1930s and during the 1940s and 1950s on normed rings (Banach algebras, as we now call them), and was continued also by Gel'fand and his school in a panoply of specialties and problems that was awe-inspiring in its breadth and included, for example, *"infinite-dimensional" representation theory*. The theory of *von Neumann factors*, which developed in parallel with it, and the theory of C^*-algebras, which grew under the influence of A. Connes into the amazingly beautiful and profound *noncommutative geometry*, are still on the ascent, and, I think, far from the summit. Even in a very compressed list one must add to these the influence of the forces that grew up within functional analysis in the development of applied areas such as approximation and optimization theory, game theory and partial differential equations, and also the development of computer science.

However, all the areas I have named (and some I have not named) soon grew into independent areas of research. Possibly that was precisely because of their rapid success. Naturally, for a certain period of time after the actual separation of these areas, many specialists who worked in them continued to think that they were studying functional analysis. I think that, as they look back on it, they would not say this today.

As a result, by the mid-1960s functional analysis had been "stripped down" to the problems that we provisionally called (and still call) *geometric*. The so-called *geometric functional analysis* turned out to be functional analysis proper. Over an extended period of time, geometric functional analysis was reduced rather simplistically to two classes of problems.

On the one hand there is the study of the geometry of infinite-dimensional convex bodies, which seems to have been begun in the papers of D. P. Milman in the late 1930s. The first (1938) obviously geometric theorem asserts that *a uniformly convex space is reflexive*, that is, the local geometry of the unit sphere implies the global topological property of reflexivity. There followed a stream of results with various co-authors from the famous Odessa school of M. G. Krein. For example, the Krein–Milman theorem on extreme points (1940), which connects geometry and topology with the linear structure; or the concept of a normal structure and the associated fixed-point theorems introduced by M. S. Brodskii and Milman (1948), and so on. This line of research was continued from the 1950s on by R. James in the USA, M. I. Kadets in Ukraine, A. Dvoretzky and his school in Israel, and A. Pełczyński and his school in Poland. It carried on successfully into the 1960s and 1970s. For example, the amazingly beautiful and surprising papers of James on nonreflexivity, or the remarkable theorem of Dvoretzky (1960), which at the time was interpreted as an extension of this circle of ideas. (I do not think of it that way today.) The concepts of the spectrum of a function and spectrum distortion, which arose next, connected geometry of the sort found in Dvoretzky's theorem with the linear structure of an infinite-dimensional space. (The state of this field in the late 1960s can be sensed from the survey [16]; for a modern view and its subsequent development, see the survey [22].) Remarkable particular results appeared still later, such as Maurey's interpretation of the normal structure and fixed-point theorems, or the concept of stable norms close to that of a spectrum which was introduced by J.-L. Krivine and B. Maurey, and the related theorems about l_p-spaces. But on the whole our understanding of geometric problems has changed, and I shall discuss them below.

Another area of geometric functional analysis, which arose in the time of Banach and as a result of his initiative, studied the linear structure of an infinite-dimensional Banach space. But what do we mean by "linear structure"?

There is a classical interpretation that was developed as early as the 1930s: the search for subspaces with a large symmetry group (naturally, symmetry is understood in the sense of isomorphism up to constants, that is, as bounded operators) and subspaces with various "good" properties. The Polish school of functional analysis grouped around Pełczyński continued to play a significant role in this development. Its ideal purpose was to show how an arbitrary Banach space can be constructed from blocks that are maximally simple, that is, have a large symmetry group. For example, does every infinite-dimensional Banach space contain a subspace isomorphic to some l_p ($1 \leqslant p < \infty$) or to c_0 or to a subspace with an unconditional basis? And other similar questions. Many remarkable results in this area are expounded in the books [5] and [6], and in the surveys [15] and [16].

However, recent advances (Gowers and Maurey [26] and the subsequent series of papers of Gowers, for example, [25]) have shown how simplistic was such an understanding of the possibilities inherent in the concept of a norm and a Banach space. It is important to note that the first breakthrough in the direction of a completely new construction of a norm was the extremely original (and nontrivial) paper of Tsirelson [32]. He constructed, what is in my opinion, the first "nonclassical" normed space. The norm in this space is defined not by a formula, but by an "equation." (In the brief survey [19] I describe this development.) By now Tsirelson's construction has been studied "inside and out." Many surprises (and advances) turned out to be connected with it. The surveys [22] and [14] will introduce the interested reader to some of these advances. It should be noted that the breakthroughs of the 1990s – and that is not just a few remarkable counterexamples to open problems of the past, but also the construction of a new infinite-dimensional geometry of convex bodies – were directly inspired by Tsirelson's paper (1974) and the problems of spectrum distortion introduced in the 1960s. (Two papers are relevant here – [27] and [16].) Thus, in the activity connected with the theory of infinite-dimensional Banach spaces, the 20-year period from the early 1970s to the early 1990s turned out to be unnecessary for obtaining the results at which the main efforts of this theory had been directed. Still, during this period several remarkable results were obtained. For example, Enflo's solution of two problems that had been open since Banach's time: the example of a Banach space without a basis and the construction of an operator having no nontrivial invariant subspace. But two new areas deserve special mention. One of these – geometric operator theory – goes back to Grothendieck (in the late 1950s), but was "explained" to specialists in functional analysis in a 1968 paper of Lindenstrauss and Pełczyński.

Subsequently extended by A. Pietsch, this theory quickly turned into one of the major tools of geometric functional analysis (see, for example, [8] or [10]). The other area (type-cotype theory) was initiated and developed by Maurey and G. Pisier in the mid-1970s. It brought into abstract functional analysis the ideas and methods of probability theory and harmonic analysis, and had a dominant influence up to the mid-1980s (see [7]). However, these new areas were looking mainly "in a different direction" and turned out to be important in the asymptotic theory, to which we now turn.

3.2. Another View of the Concept of a "Linear Structure"

In this section we shall exhibit an interpretation of "linear structure" that is completely different from the classical structure discussed above. In fact, there now exist two different interpretations, two opposite (in a certain sense) routes to the study of structures that replace the classical interpretation of a linear structure. However, both routes study certain asymptotics of the behavior of finite-dimensional subspaces of a given normed space.

3.2.1. In the first of these approaches we investigate certain families of finite-dimensional spaces containing subspaces of arbitrarily large dimension. For example, we might study the family of all finite-dimensional spaces or all finite-dimensional subspaces of a given normed (infinite-dimensional) space. It turns out that when the dimension of the space increases to infinity, remarkable and unexpected regularities are revealed, which had been hidden behind the apparent and expected diversity that increases with the dimension. Indeed, I see in this approach another way of looking at the concept of an "infinite-dimensional" space. In this view, it is not a single space but a family of spaces, each of which is finite-dimensional. However, their dimensions are not collectively bounded, and their asymptotic behavior exhibits infinite-dimensional phenomena that are not characteristic of either the individual finite-dimensional spaces or infinite-dimensional spaces. It is this theory that I call *asymptotic geometric analysis*. In its early stages it was often called the *local theory*. Its rapid development from the mid-1970s to the mid-1980s led to the idea that we are dealing with an area different from the problems and aims of classical functional analysis, although closely connected with it (see the books [7], [9], and [10]). At first (in the mid-1980s) we called it *geometric analysis*, since it studied geometric objects using the concepts of analysis. (I emphasize: concepts, not techniques.) A particular geometric object (for example, a convex body in a fixed space) turns into a family of objects, for example, a family of convex bodies in different spaces of increasing dimension. The asymptotic properties of such families reflect isomorphic geometric properties of the family. This is the crux of the theoretical difference between our approach and the standard geometric vision, in which isometric (or "almost" isometric) properties are studied (see [18]). In the next subsection I shall give several precisely stated propositions, and we shall see examples of models of the behavior of such families.

However, the phrase *geometric analysis* turned out to be irresistibly attractive for a large number of scholars studying very different kinds of mathematics, and using it ceased to make sense. (This is one form of "pollution" in mathematics.) For that reason, we now use the term *asymptotic geometric analysis* but also *convex geometric analysis*, first of all because we are studying mainly the asymptotic behavior of convex bodies, and second because the name nicely emphasizes the merger of this area with classical convexity theory and the theory of geometric inequalities that is occurring.

3.2.2. I would now like to say a few words about a very recent approach to purely infinite-dimensional phenomena that have no finite-dimensional analogs but can be studied using families of finite-dimensional subspaces of a fixed space, specially chosen for the purpose. This is the so-called *asymptotic infinite-dimensional theory* (see [29] and [22]). In this approach we sweep away the information of finite-dimensional character and study the space "at infinity."

The fundamental concept in this theory is that of an *asymptotic* (*finite-dimensional*) *space* of a given infinite-dimensional Banach space X. The basic idea behind it is the stabilization at infinity of finite-dimensional subspaces of a fixed (but arbitrary) dimension, which occur in X "everywhere sufficiently far out". I realize how murky this sounds, and I shall attempt to make the construction more precise immediately. Fix an integer k. For a subspace $E \subset X$ of finite codimension (say $\operatorname{codim} E = n$) we denote by $T_k(E)$ the closure in the Banach–Mazur metric of the set of k-dimensional subspaces of E. We denote by $\{X\}_k$ the family of k-dimensional spaces (regarded as a subset of the compact Banach–Mazur set of all k-dimensional normed subspaces) that is the limit (or, what is the same, the intersection) of $T_k(E)$ over the filtration of subspaces of finite codimension as the codimension n tends to infinity. It follows from simple compactness considerations that $\{X\}_k$ is not empty. These are the asymptotic k-dimensional spaces of the original space X. A more "working" approach to describing this set uses the language of game theory, introduced for closely related purposes by Gowers (see [29]).

The set of all asymptotic spaces, $\{X\}_k$, $k = 2, 3, \ldots$, is the asymptotic linear structure of X. (It is constructed over the filtration of all subspaces of finite defect; however, the filtrations may be chosen in other ways.) I would rather refer the reader to the original papers and the only survey that has yet appeared, on a closely related topic [22].

3.3. Asymptotic Geometric Analysis: Some Examples

In this section I intend to show by several examples that the asymptotic point of view on spaces of high dimension opens a new intuition, and that the results obtained were not (and could not have been) predicted on the basis of experience and intuition in the study of infinite-dimensional spaces or spaces of fixed dimension. The examples have deliberately been chosen so that all the objects and concepts used are classical, even elementary. In that way, it is easier to compare a result with one's own intuition. For better acquaintance with this theory I have already recommended three monographs above. Let us add to that several surveys that emphasize different aspects of the theory and different ideas that arise in it: [13], [11], [18], [20], and [21].

3.3.1. Consider a set K in $\mathbb{R}^n$. How does the diameter of a set change under a "random" orthogonal projection of rank k (that is, under projection onto a "random" subspace of dimension k)? It turns out that the answer is almost independent of the set K. To be precise, I introduce some notation: $d(T)$ denotes the diameter of the set T in the standard Euclidean norm on $\mathbb{R}^n$; $P_E K$ is the orthogonal projection of K on the subspace E; and $D_k(K)$ denotes the average (mathematical expectation) $\mathbb{E}\big(d(P_E K)\,\big|\,\dim E = k\big)$ of the diameter of the set $P_E K$ over all k-dimensional subspaces E. The width $w(K;u)$ of the set K in the direction $u \in S^{n-1}$, where S^{n-1} is the unit Euclidean sphere, is defined as

$$w(K;u) = \sup\{(u,x) \mid x \in K\} - \inf\{(u,x) \mid x \in K\}.$$

Finally, the average width $w(K)$ of the set K is

$$w(K) = \int_{u\in S^{n-1}} w(k;u)\,\mathrm{d}\sigma(u),$$

where the integration is taken with respect to the normalized (that is, probabalistic) Lebesgue measure on the sphere S^{n-1}.

Proposition. *There exist constants $c > 0$ and C such that, for every n and every $K \subset \mathbb{R}^n$,*

$$c\sqrt{\frac{k}{n}}\,d(K) \leqslant D_k(K) \leqslant C\sqrt{\frac{k}{n}}\,d(K)$$

for $n \geqslant k \geqslant k^* = n\big(w(K)/d(K)\big)^2$. For k less than the critical value k^* the average diameter $D_k(K)$ stabilizes:

$$cw(K) \leqslant D_k(K) \leqslant Cw(K)$$

for $1 \leqslant k \leqslant k^*$.

Moreover, the only reason for the stabilization is that a random projection of K on a subspace of dimension $\approx [\theta k^*]$ approaches the Euclidean ball of radius $1/2w(K)$ up to $\sqrt{\theta}$. (This last statement needs some additional clarification, but we refer the reader to [21] for a precise discussion and references.)

Thus, up to stabilization, the rate of decrease of the diameter of a random projection is generally independent of the set K: the closed interval $[0,d]$ behaves just like an extremely complicated set! However, stabilization settles for them at different dimensions k^* and means that a random projection on a dimension proportional to k^* approximates a Euclidean ball in a certain sense. As it turns out, for the closed interval $k^* \sim 1$ (as one would expect).

On the other hand, such regularity of behavior is possible due to the "isomorphic" form of the answer. I am referring to the universal constants (c and C) that accompany the "formula" for the behavior. The answer actually depends on the set K, but on a more refined scale. The proposition describes a zone in which it is "fuzzy." This is a typical phenomenon of the isomorphic geometry to which the (dimensionally) asymptotic view of geometric problems leads. Of course, the isomorphic point of view in geometry makes no sense in a fixed dimension, but it arises naturally in asymptotic problems. We have accumulated a large number of surprising facts of such an isomorphic geometry (see the surveys [11], [18], [20]).

3.3.2. Let us denote by $N(K,T)$ the covering number of two convex sets K and T in $\mathbb{R}^n$:

$$N(K,T) = \min\left\{ N \,\middle|\, \exists x_i \in \mathbb{R}^n \,\middle|\, \bigcup_{i=1}^{N}(x_i + T) \supset K \right\}$$

(the number $\log N(K,T)$ is often called the *entropy* of the covering).

It is clear that the order of growth of $N(K,T)\cdot N(T,K)$ is exponential in the dimension of even similar sets K and T of the same volume. On the other hand, this quantity may be arbitrarily large, not because of different geometries of the sets themselves, but due to "incorrect positioning" of one set relative to the other. For that reason we introduce a second quantity

$$M(K,T) = \inf\left\{ N(K,uT)\cdot N(uT,K) \,\middle|\, u \in \mathrm{SL}_n \right\}.$$

However, the geometry of the sets K and T may be very different and the Banach–Mazur distance $d(K,T)$ may be of order n, even for centrally symmetric sets. (This is a nontrivial result of Gluskin, but order $\sqrt{n}$ is trivial.) Thus, the order of growth of $M(K,T)$ with respect to the dimension n might conceivably reach $\mathrm{e}^{cn\log n}$. But the result is much better: there exists a number C such that

$$M(K,T) \leqslant \mathrm{e}^{Cn}$$

for every dimension n and every two convex sets K and T of the same volume (see [21] for discussion and references). Thus, from the point of view of coverings, the geometry of two arbitrary convex sets is much the same. A large list of similar results and the reason behind them is described in the surveys [18] and [20].

This discovery of the "resemblance" of arbitrary convex bodies in spaces of very high dimension manifests itself in a great variety of situations. I think it indicates the existence of probabilistic structures accompanying spaces of high dimension. By that I mean something more than the mere fact that we use probabilistic methods in the proofs. A space of high dimension (or, more precisely, a family of spaces whose dimensions increase to infinity) in its very essence contains elements of randomness and is in some sense a random medium. Naturally, I should have to present dozens of well-known results to confirm that intuition, but also naturally, I am stopping here in this brief essay.

3.3.3. As a last example, let us discuss approximation problems. For sets K and T the Minkowski sum $K+T$ is the set $K+T=\{x+y \,|\, x\in K, y\in T\}$. Let $I=[-x,x]$, $x\in S^{n-1}$, that is, I is an interval of length $2x$.

Consider

$$K_N = K(N;u) = \frac{1}{N}\sum_{i=1}^{N} u_i I,$$

where $u_i \in \mathrm{O}(n)$ are orthogonal operators. Thus K_N is an average of N intervals. The question is: How many intervals suffice to obtain a good approximation of the Euclidean ball?

We remark that, for any distribution of points $\{x_i\}_1^N$ on the sphere S^{n-1} there exists a layer between two parallel hyperplanes that contains all these points and whose width is of the order $\sim \sqrt{\log N/N}$. It is possible that this circumstance is responsible for the feeling that N must be of order $\exp(cn)$. However, there actually exist $N \sim n/\varepsilon^2$ points $\{x_i\}_1^N$ on the sphere such that

$$K(\varepsilon) = \frac{1}{N}\sum_{i=1}^{N}[-x_i,x_i]$$

approximates the Euclidean ball D (of radius $r\sim 1/\sqrt{N}$) within ε; that is, $D/(1+\varepsilon)\subset K(\varepsilon)\subset D(1+\varepsilon)$. Moreover, for any $\lambda>1$ there exists a constant $c(\lambda)$ and intervals $I_i=[-x_i,x_i]$, which need not number more than $N=\lambda n$, such that for some Euclidean ball D

$$D \subset K_N = \frac{1}{N}\sum_{i=1}^{N} I_i \subset c(\lambda)D.$$

This last result follows from a result of Kashin (1976); however, the entire picture is described in more detail, for example, in the survey [18], which also contains references to the original papers.

As it happens, the closed interval can be replaced by any set and the answer is still valid. Many other problems of approximation and different symmetrizations have been studied and lead to similar answers. Usually, the final answer turns out to be a logarithmic function of the "likely answer" suggested to us by the intuition we were trained in. This discrepancy is amazing and needs to be clarified.

I think that our intuition on the level of the variety and forms of behavior of a multidimensional space is mainly connected with the exponential growth (with respect to dimension) of coverings (entropy) and with the calculations of volumes. Still, there is a compensating factor, the so-called *concentration of measure* that is always observed in uniform distributions on multidimensional manifolds (the concentration phenomenon). The first example of this phenomenon seems to have been described by Paul Lévy in his 1919 lectures. (The second edition [4] of his work is well known.) However, a realization of the extent to which this phenomenon is general and the fact that it compensates for the exponential enlargement of volumes was a consequence of applying this technique in problems of asymptotic geometry. (The first, and perhaps still the leading, example of such an application was published in the USSR [28].) Here I shall simply refer the reader to the numerous surveys that describe both the development of the technique and its applications: [17], [3], [18], [7], [23], [24]. Moreover, many other books and papers discuss the concentration phenomenon in discrete mathematics ([1]), geometry ([2], [12]), and ergodic theory ([30], [31]).

Bibliography

Books

[1] N. Alon, J. H. Spencer. *The Probabilistic Method*. New York: Wiley & Sons, 1992.

[2] M. Gromov. *Metric Structures for Riemannian and Non-Riemannian spaces*, based on *Structures metriques des variétés Riemanniennes*, with appendices by M. Katz, P. Pansu and S. Semmes. Boston, MA – Basel – Berlin: Birkhäuser, 1999.

[3] M. Ledoux, M. Talagrand. *Probability in Banach Spaces*. Berlin: Springer, 1991 (Ergeb. Math. Grenzgeb. 3. Folge, **23**).

[4] P. Lévy. *Problèmes concrets d'analyse fonctionelle*. Paris: Gauthier-Villars, 1951.

[5] J. Lindenstrauss, L. Tzafriri. *Classical Banach Spaces*, Vol. I: Sequence Spaces. Berlin: Springer, 1977 (Ergeb. Math. Grenzgeb., **92**).

[6] J. Lindenstrauss, L. Tzafriri. *Classical Banach Spaces*, Vol. II: Function Spaces. Berlin: Springer, 1979 (Ergeb. Math. Grenzgeb., **97**).

[7] V. Milman, G. Schechtman. *Asymptotic Theory of Finite-Dimensional Normed Spaces*. Berlin: Springer, 1986 (Lecture Notes in Math., **1200**).

[8] G. Pisier. *Factorization of Linear Operators and the Geometry of Banach Spaces*. Providence, RI: Amer. Math. Soc., 1986 (CBMS Regional Conf. Ser. in Math., **60**).

[9] G. Pisier. *The Volume of Convex Bodies and Banach Space Geometry*. Cambridge University Press, 1989 (Cambridge Tracts in Math., **94**).

[10] N. Tomczak-Jaegermann. *Banach–Mazur Distances and Finite-Dimensional Operator Ideal*. Harlow: Longman; New York: Wiley & Sons, 1989 (Pitman Monographs and Surveys in Pure and Appl. Math., **38**).

Surveys

[11] A. A. Giannopoulos, V. D. Milman. Euclidean structures in finite dimensional normed spaces. In: *Handbook of the Geometry of Banach Spaces* (eds. W. B. Johnson and J. Lindenstrauss). Amsterdam: North-Holland, 2001, 707–779.

[12] M. Gromov. Spaces and questions. *Geom. Funct. Anal.*, 2000, Special Volume, Part I, 118–161.

[13] J. Lindenstrauss, V. Milman. The local theory of normed spaces and its applications to convexity. In: *Handbook of Convex Geometry*, Vol. B (eds. P. M. Gruber and J. M. Wills). Amsterdam: North-Holland, 1993, 1149–1220.

[14] B. Maurey. Quelques progres dans la comprehension de la dimension infinie. In: *Espaces de Banach classiques et quantiques*. Paris: Soc. Math. France (Journée Annuelle), 1994, 1–29.

[15] V. D. Milman. The geometric theory of Banach spaces. I. The theory of basic and minimal systems. *Russ. Math. Surveys*, 1970, **25**(3), 111–170.

[16] V. D. Milman. The geometric theory of Banach spaces. II. Geometry of the unit sphere. *Russ. Math. Surveys*, 1971, **26**(6), 79–163.

[17] V. Milman. The heritage of P. Lévy in geometric functional analysis. *Astérisque*, 1988, **157–158**, 73–141.

[18] V. Milman. Surprising geometric phenomena in high-dimensional convexity theory. In: *European Congress of Mathematics* (Budapest, 1996), Vol. II. Basel: Birkhäuser, 1998, 73–91 (Progr. Math., **169**).

[19] V. Milman. Linear structure of Banach spaces, asymptotic view. In: *Banach Space Theory and its Applications*. Proceedings of Wuhan International Conference on Banach Spaces. Wuhan University Press, 1996, 1–12.

[20] V. Milman. Randomness and pattern in convex geometric analysis. In: *Proceedings of the International Congress of Mathematicians* (Berlin, 1998), Vol. II. *Documenta Math.*, 1998, Extra Vol. II, 665–677.

[21] V. Milman. Topics in asymptotic geometric analysis. *Geom. Funct. Anal.*, 2000, Special Volume, Part II, 792–815.

[22] E. Odell. On subspaces, asymptotic structure and distortion of Banach spaces; connections with logic. In: *Analysis and Logic* (Mons, 1997). Cambridge University Press, 2003, 189–267 (London Math. Soc. Lecture Note Ser., **262**).

[23] M. Talagrand. Concentration of measure and isoperimetric inequalities in product spaces. *Publ. Math. IHES*, 1995, **81**, 73–205.

[24] M. Talagrand. A new look at independence. *Ann. Probab.*, 1996, **24**(1), 1–34.

Articles

[25] W. T. Gowers. Recent results in the theory of infinite-dimensional Banach spaces. In: *Proceedings of the International Congress of Mathematicians* (Zürich, 1994), Vol. 1, 2. Basel: Birkhäuser, 1995, 933–942.

[26] W. T. Gowers, B. Maurey. The unconditional basic sequence problem. *J. Amer. Math. Soc.*, 1993, **6**(4), 851–874.

[27] V. D. Milman. Spectrum of bounded continuous functions on the unit sphere in a Banach space. *Funct. Anal. Appl.*, 1969, **3**(2), 137–146.

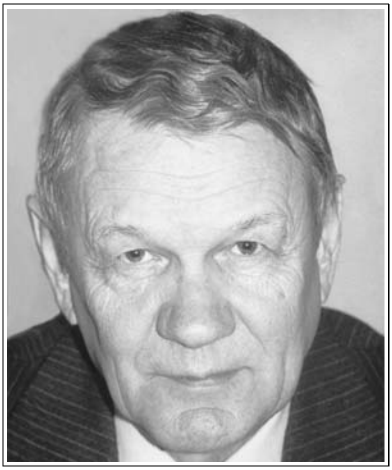
[28] V. D. Milman. New proof of the theorem of A. Dvoretzky on intersections of convex bodies. *Funct. Anal. Appl.*, 1971, **5**(4), 288–295.

[29] B. Maurey, V. D. Milman, N. Tomczak-Jaegermann. Asymptotic infinite-dimensional theory of Banach spaces. In: *Geometric Aspects of Functional Analysis* (Israel, 1992–1994). Basel: Birkhäuser, 1995, 149–175.

[30] V. G. Pestov. Amenable representations and dynamics of the unit sphere in an infinite-dimensional Hilbert space. *Geom. Funct. Anal.*, 2000, **10**(5), 1171–1201.

[31] V. G. Pestov. Ramsey–Milman phenomenon, Urysohn metric spaces and extremely amenable groups. *Israel J. Math.*, 2002, **127**, 317–357.

[32] B. S. Tsirelson. Not every Banach space contains an imbedding of l_p or c_0. *Funct. Anal. Appl.*, 1974, **8**(2), 138–141.



E. F. Mishchenko

About Aleksandrov, Pontryagin and Their Scientific Schools

Translated by R. Cooke

The topological schools of Aleksandrov and Pontryagin, and Pontryagin's school of differential equations and the mathematical theory of optimal processes, were undisputably significant phenomena in the mathematical life of the twentieth century.

I had the good fortune to be a close student of both Aleksandrov and Pontryagin at different times. I think that is why I received a proposal from the editors of this volume to write an article on these schools and their founders. Not without hesitation did I agree to this proposal – to make a survey as interesting as complete would be beyond my powers. For that reason I shall briefly talk about just a few of the main papers of Aleksandrov and Pontryagin, using as a source not just my own personal perception of these papers, but also several survey articles written by other authors (but some of my own also). In addition, I will share my recollections of my great teachers, and the atmosphere in and around their schools, not omitting some episodes that are perhaps recalled only by myself. These episodes often surface in my memory, and I hope they will be interesting to others – if only as a few fragments of the multifeatured history of the research schools of Aleksandrov and Pontryagin.

1.

PAVEL SERGEEVICH ALEKSANDROV (1896–1982) is well known to all the mathematicians of the world as the founder of the Moscow school of topology and as the permanent (1932–1964) president of the Moscow Mathematical Society. His personal contribution to general topology had a decisive effect on the entire development of this branch of mathematics.

After beginning his research activity brilliantly at the age of 18 with the proof of a fundamental theorem on the cardinality of *B*-sets (every uncountable Borel set contains a perfect subset) and the construction of the well-known *A*-operation, and then suffering a failure in solving the continuum problem, which he began at the suggestion of his university teacher Luzin, Aleksandrov left off studying mathematics for two years — fortunately, not forever — left Moscow and successfully tried several other professions (reader of a provincial committee on education, director of a dramatic theater, and others). However, in the end he returned to mathematics, taught at Smolensk University, and from 1921 on was a teacher and professor of Moscow University, for a long time serving simultaneously as head of the division of topology in the Steklov Mathematical Institute.

In 1921, Aleksandrov made the acquaintance of Pavel Samoilovich Uryson, and they soon began to carry out joint research in the field of topology — a subject that was completely unknown in Russia at the time. Their joint work did not last long — in the summer of 1924 Uryson died tragically at the age of 26. But during these three years they laid the foundations of what is now called *general topology* — the comprehensive study of topological spaces. Although the concept of a topological space had been introduced into mathematics by Fréchet (1906) and Hausdorff (1914), their versions were only a general abstract framework. To fill that framework with geometric content was the object of the early papers of Aleksandrov and Uryson on the dimension of sets, which goes back conceptually to the papers of H. Poincaré, L. E. J. Brouwer, and H. Lebesgue. Next followed the theorems of Aleksandrov and Uryson on metrizability of topological spaces. At the same time Aleksandrov began to construct the theory of compact spaces, introducing the term *bicompact* into mathematics. This concept, which has not been associated with its creator for a long time, became firmly fixed in all textbooks of topology and still today is used constantly in various fields of mathematics. (To be sure, I have heard that the Bourbaki group interprets the word *bicompact* as *compact* and vice versa.) There were many such brilliant and, at the same time, simple concepts and discoveries in the school of Aleksandrov during its initial period. It suffices, for example, to recall the *Tikhonov topology* on the Cartesian product of any number of topological spaces, or the theorem that any n-dimensional compact set can be embedded in a Euclidean space of dimension $2n+1$, proved by Pontryagin in 1931 and given by him, as he himself writes in his memoirs, to one of his students. True, as it turned out, this theorem was proved independently, and in the same year, by two well-known mathematicians – Solomon Lefschetz and Georg Nöbeling. But that is a quite common occurrence in mathematics. In the present situation, it attests to the naturalness of this topic at the time.

Aleksandrov's most significant discovery in the early years of his topological activity seems to have been the introduction of the *nerve of a covering* of a topological space, and the concept of the *projection spectrum* of simplicial mappings. This discovery brought about a revolution in the entire development of set-theoretic topology, shifting it into the geometric and combinatorial-algebraic channel. I shall give here only the definition of the nerve, so that the attentive reader may surmise that what I have said is not an exaggeration.

The nerve of a covering of a space X is a simplicial complex N_ω whose vertices are in one-to-one correspondence with the elements of the covering ω, and set of vertices $e_1, e_2, \ldots, e_k$ of the complex N_ω forms a simplex in N_ω if and only if the elements of the covering ω corresponding to these vertices have a nonempty intersection. If the covering ω' is inscribed in the covering ω (it is said to be a *refinement* of it), then a simplicial mapping $\pi_\omega^{\omega'}$ is naturally defined (the "projection" of the nerve $N_{\omega'}$ onto the nerve N_ω). Therefore, if, for example, X is bicompact and ω ranges over the directed family of all open coverings of it, then the so-called *spectrum* S of the bicompact space X is defined. It consists of the directed family of complexes N_ω and the projections $\pi_\omega^{\omega'}$ that connect these complexes. The spectrum defines a limiting space in a certain natural way, which turns out to be homeomorphic to the space X. Thus, it becomes possible to interpret all topological properties of the bicompact space X as properties of its projection spectrum S, that is, reduce them to properties of the complexes N_ω and their simplicial mappings. This means that it becomes possible to use the whole arsenal of machinery of combinatorial topology as one of the main tools for studying set-theoretic topology.

One of Aleksandrov's most outstanding results in topology was his creation of *homological dimension theory*. We recall that the homological dimension of a compact set X with respect to a coefficient group is the largest integer n such that the compact space X contains an $(n-1)$-dimensional cycle Z^{n-1}

homologous to zero in X, but not homologous to zero on some support of it. In creating this theory Aleksandrov proved his famous theorems on the so-called *essential mapping* and *obstructions*, and a number of other theorems. This entire thematics drew an enthusiastic response from Pontryagin and influenced many of his interests in topology. And even Kolmogorov, who discovered *cohomology* (actually simultaneously with J. Alexander), although he made this discovery from physical considerations — from hydrodynamics and electromagnetism — would nevertheless hardly have been able to carry it out in the form of his famous four notes in the *Comptes Rendus* without the influence of numerous conversations on topological topics with Aleksandrov. And it is only to be expected that this discovery was followed almost immediately by the rise of *cohomological dimension theory* and different versions of *duality laws*.

Among the outstanding papers of Aleksandrov, I would like to say a little about two others: "Homological properties of the position of complexes and closed sets," and "The basic duality relations for nonclosed sets of an n-dimensional space."

The first of these papers was devoted to the study of the shape and position of a complex (and a closed set) in an ambient space (and in a closed set) using homological methods. This paper was written in Kazan' in 1941–1942, where the USSR Academy of Sciences had been evacuated when World War II began, and it was published immediately in the *Izvestiya Akademii Nauk SSSR*, (mathematics series, 1942, **6**) and soon afterward in the USA. This was an abstract mathematical paper having no applications in engineering or agriculture. Nevertheless, its author was soon awarded the Stalin Prize, the highest government award for science at the time. And that was at the most difficult time in the whole history of the USSR... It furnishes a curious and edifying example of the relation of the government to science.

The second paper was written in 1946–1947 and published in *Matematicheskii Sbornik* (1947, **21**). Until this paper appeared, all homological methods in the topology of an n-dimensional space had been developed only for polyhedra and closed sets. Even the famous duality theorems had not been extended to such elementary objects as *skinned polyhedra*, for example, sets in n-dimensional space that are the union of a finite number of pairwise disjoint open simplexes of generally different dimensions. Aleksandrov set himself the task of creating a general theory of duality for the largest possible class of nonclosed sets; that is, to solve in a rather general case the problem of expressing the homological invariants of a nonclosed set A in terms of the homological invariants of its complement B. The first thing to be done was to define the corresponding homology groups for the sets A and B in a suitable way. He did this using *projection cycles* and *unlinked cycles*, which he introduced, and proved the first general law of duality for homology groups based on these cycles. A little later his student K. A. Sitnikov proved a more general theorem. However, the homology groups that occur in the duality laws of Aleksandrov and Sitnikov have a rather complicated definition. Hence, from the very beginning, Aleksandrov posed the problem of finding a large and natural class of nonclosed sets for which Pontryagin's elementary duality law holds; that is, duality in the sense of the characters $\Delta^p(A,\mathfrak{A}) \,\big|\, \Delta^q(B,\mathfrak{B})$, where Δ is the quotient group of all *true* cycles (of a given dimension) over the group of bounding cycles. Here a true cycle of a given set E is defined as a (convergent) cycle contained in some compact subset $\Phi \subseteq E$. A true cycle is a bounding cycle (homologous to zero) in E if it is homologous to zero in some compact set $\Phi' \subset E$; $\mathfrak{A}$ is an arbitrary discrete group of coefficients, $\mathfrak{B}$ is a bicompact group, $\mathfrak{A} \,|\, \mathfrak{B}$, and $p+q=n-1$. This problem was solved for the so-called *homological retracts*, which of course include all skinned polyhedra. I made a contribution of my own to the solution of this problem while still an undergraduate in the Department of Mechanics and Mathematics at Moscow University, and later defended my *kandidat* dissertation on this topic. After that, I no longer studied topology.

In his memoirs, Pontryagin wrote that Mishchenko ceased to study topology, since he realized that this subject was outside the mainstream of mathematics of the time. I must say that this is not quite what happened. In my undergraduate years I wasn't thinking about any mainstream. I studied all of Aleksandrov's papers with great interest and began to study his approach to topology with great enthusiasm. These activities gave me the joy of my first scientific discovery, my first publication in *Matematicheskii Sbornik*, and the start of my close and constant scholarly association with my advisor. I think that on the emotional and psychological levels I felt just as comfortable as my friends Gamkrelidze, Boltyanskii, and Postnikov, who were studying Pontryagin topology at the time. And I must have been imbued with a sense of the importance and necessity of my studies, just like the young mathematicians of today who are happily studying a new set of topics that is in fashion in their near surroundings.

The only reason I stopped studying topology at the time was that I came under the influence of Pontryagin, who had begun to study ideas connected with applications by then, and I followed him into that interesting world. But I will discuss that later.

So far as I know, the paper "Fundamental duality relations for nonclosed sets of an n-dimensional space" was Aleksandrov's last paper in combinatorial (homological) topology. From that point on he concentrated his efforts on various questions of set-theoretic topology and included a large group of young students in active research work. I shall not list all of the results of this time. The reader can find a number of essays, for example, in the special issue of *Uspekhi Matematicheskikh Nauk* dedicated to the 80th birthday of Pavel Sergeevich (1976, **21**(5)), or in the volume of the *Proceedings of the Steklov Institute* published in honor of the centennial of his birth (1996, **212**). I would like just to say a few words about Aleksandrov's research seminars.

Pavel Sergeevich was an extraordinary teacher. In his seminars there was always a kind of force field holding all the participants in tension and, I would say, joyous excitation. These seminars took place in the classrooms at the University, in the student dormitories, and at his "dacha" (a countryside house) in Komarovka, which he owned jointly with Kolmogorov. In Komarovka there was always classical music in the intervals between mathematical conversations. There was rowing in the summer and skiing in the winter. The seminars in the University and at the Steklov Institute were attended by both students and famous mathematicians working in related areas. I recall, for example, several seminars at which Lyudmila Vsevolodovna Keldysh spoke on open mappings that increase dimension. When she described the details of her example of a monotonic open mapping of a three-dimensional cube onto a four-dimensional cube, Pavel Sergeevich, who had previously studied the problem himself, congratulated her vehemently and said that now Peano's whole subject could be considered closed. (I remark that this area had been much in fashion almost since the very beginning of the twentieth century.)

Research and teaching were undoubtedly the fundamental components of the entire life of Pavel Sergeevich Aleksandrov. However, this life also contained bitter periods. I was an observer of one of these periods. In the mid-1960s three students of Aleksandrov underwent serious criticism at the defense of their doctoral dissertations, all at the same time. I was present at one of these defenses. The effort to reject the dissertation failed, but those who were there remember the scandal to this day. The unpleasantness came from several young (and not so young) mathematicians, and was motivated by the insufficient general mathematical literacy of the candidate. It may have been so. However, Pavel Sergeevich perceived this as a rejection of his entire set-theoretic research area of the time and was depressed, to say the least, for many weeks. I also had the impression that he had good reason for his perception.

Later on, I often recalled this story in connection with a more significant event. In the second half of the 1970s there arose a large debate over the problems of elementary mathematical education, which went on for several years. Many prominent mathematicians participated in this debate, and some of them made the accusation that Kolmogorov, if not directly introducing elements of set theory into the elementary school curriculum, was at least providing ideological support for its introduction. Here it was assumed *a priori* that the "Cantorian virus"[1] was one of the main reasons leading to the *ruin* of elementary mathematical education, and some of the articles contained even more ominous accusations. All this evoked a worried reaction in Kolmogorov and seriously clouded the last years of his life, which I know and not by hearsay.

I shall not undertake to judge this whole debate, since I have never taught in high schools and have never written any textbooks. However, I cannot help agreeing that the accusations were too emotional and not entirely justified. Kolmogorov really did have *great achievements* in the cause of mathematical education of elementary-school pupils, since it was through his efforts that many talented people were discovered, who later grew into great mathematicians. A significant portion of them would most likely have remained unknown if not for the boarding schools of Kolmogorov (or of Kolmogorov type). But the "bad" textbooks and methodological developments did not spread and disappeared of their own accord over time, along with the shades of Cantor and Bourbaki. It is likely that in the near future they will seem to be mild scarecrows in comparison with the educational reforms beginning in Russia just now.

As for Aleksandrov's students in the area of set-theoretic topology, many of them continued to work actively, became famous scientists, and achieved international recognition in the circles of *general* topologists. The majority of these students work as professors in Russian and foreign universities. Some remained in "Cantor's paradise," and others changed direction and switched to other areas — each in accordance with one's taste and talent.

In his large autobiography, published in two issues of *Uspekhi Matematicheskikh Nauk* (1979, **34**(6) and 1980, **35**(3)), Pavel Sergeevich wrote a page or so about me. I shall present a few excerpts from that page and then tell what really happened, so that the reader can get at least a partial impression of Aleksandrov's relation to his students.

> In 1939 the 17-year-old high school student Zhenya Mishchenko arrived in Komarovka. He was soon drafted into the army and went to the front, taking with him some mathematical books given to him at Komarovka. At war's end Mishchenko was demobilized only after some trouble, in which I was compelled to play a part. Immediately after demobilization he entered the Department of Mechanics and Mathematics at Moscow University and began to

[1] Not my term. — *Author's remark.*

> study mathematics eagerly. After graduating from the University he became a graduate student under my direction . . .
>
> Back in his undergraduate years Mishchenko became acquainted with Pontryagin and fell more and more under his mathematical influence. But even though he eventually became Lev Semenovich's student, he continued to be perceived by me as my student — in accordance with my firm conviction that the relations between a student and a teacher are irreversible; once they arise, they cannot be abrogated except at the cost of a catastrophe, any more than the relation between a father and a son. It always seemed to me that Mishchenko shared this point of view. Accordingly relations between us have never faltered, but have always been and remain very cordial . . .

But here is how it really happened. I was born and grew up in a remote tiny village three hundred kilometers from Moscow in a very poor family. After my elementary education was finished, I entered high school at my mother's insistence in a worker settlement not far from our village. I was a good student, but I didn't have enough mathematics textbooks; and when I found two mathematics books, *Differential Calculus* by Granville and Luzin and *Theory of Functions of a Real Variable* by Aleksandrov and Kolmogorov, in the library of the workers' club — God only knows how they came to be there — I read them and then I couldn't imagine any other future career for myself except as a mathematics teacher. By that time (I was in my ninth year) a graduate student of Andronov, the physicist Aleksandr Ivanovich Egorov,[2] arrived at our school from Gor'kii. He advised me to write a letter to *Professor P. S. Aleksandrov* in Moscow and ask him to give me some problems to think about independently. I sent the letter off and soon there arrived from Moscow an envelope containing three notes of Aleksandrov. One was to my mother, with a request to allow me to come to Moscow and a promise to send her money immediately for my railroad ticket; the second was to the director of the school, with a request to allow me to miss two or three days of school; the third was to me, with a detailed hand-drawn map from which I would be able to get from the Kursk railroad station to Yaroslavl' railroad station and then to Komarovka. A couple of weeks later I set off on my journey, which, as it turned out, determined the rest of my life.

Pavel Sergeevich greeted me very cordially. He said that, unfortunately Kolmogorov was seriously ill and was lying in his room, and that I might be

[2] He died in the war in 1943.

able to make his acquaintance later. He then had dinner with me and set me a large examination on the theory of functions. We then walked along the river, and I remember a long conversation on literature and a short examination on the German language. The next morning he himself wrote on a piece of paper for me, what he said was a difficult unsolved problem in topology, and asked me to send him the answer by post when I either solved it or was unable to solve it. After that, we went to Moscow together, and Pavel Sergeevich arranged a tour of the Department of Mechanics and Mathematics at Moscow University for me. Toward evening we walked around Moscow a little, stopped in at bookshops, where I received a gift from Pavel Sergeevich consisting of several good books (I recall the book of Schreier and Sperner *Linear Algebra in Geometric Exposition* in German and the book *Differential and Integral Calculus* by Courant. Late at night I went alone to the Kursk station.

I didn't get a complete solution of Pavel Sergeevich's problem, but I did think of something and wrote him a long letter to which I almost immediately received an encouraging reply, then another letter, and just before I left for the Army I again paid a visit to Komarovka.

I was already in the Army when the war broke out. I wound up at the front and it wasn't until 1943 that I was able to send my soldier triangle-letter to Pavel Sergeevich. His reply found me, and our correspondence, though irregular, continued until the end of the war. For me every letter of Pavel Sergeevich (Kolmogorov also wrote to me once) was the source of great joy and support in those difficult years. By the time the war ended, I was a lieutenant. An effort was made to send me to the Military Academy, and my demobilization was refused. If not for the intervention of Pavel Sergeevich — his repeated requests to the *highest* military command — I do not know how my *via dolorosa*[3] would have ended.

P. S. Aleksandrov
and E. F. Mishchenko
at Gelendzhik (1952)

My friendship with Pavel Sergeevich continued after I entered the University. He encouraged me in all my work, sometimes correcting it, and sometimes directly participating in it. In those lean years, like many other students of his, I received a gift from him nearly every summer in the form of a pass to a spa or sanatorium. And once when I fell seriously ill he and Pontryagin saw to my treatment and recovery.

[3] Literally, *Journeys through Pain*, the title of a long novel on the Russian Revolution and Civil War by Aleksei N. Tolstoy. — *Transl.*

All this rings out in my memory when I read his sparse words: "In 1939 a seventeen-year-old student came to Komarovka . . . was demobilized, . . . , became a graduate student under my direction . . . ".

I especially remember the lessons that Pavel Sergeevich gave in our musical education. At the beginning of each musical season he would purchase a large number of subscriptions at the Conservatory for concerts of classical music and pass them out to his students; and he himself always came to the concerts. He knew that even if some of us were coming at first only for fear that he would notice their empty seats nearby; later on *they would come of their own accord to hear the music*.

I still sometimes go to the Conservatory and once in a while I see someone I know there who began by counting the pipes in the large organ at their first concerts and later became active visitors to the Large Hall.

In the afterword to his autobiography Pavel Sergeevich wrote:

> *To my students*:
>
> *I have already mentioned that I entered the University so that I could devote myself to teaching in a high school after I graduated, become a teacher of mathematics in a gymnasium. As things turned out for me, I almost never taught in a high school but at a higher level, at Moscow University. I worked practically my whole life, combining teaching as far as I was able with research. As time passed, the first of these components* (*teaching and research*) *acquired ever greater relative weight and in the end, approximately when the third generation of my students came (and even a little earlier) it filled my life completely. My research was always nourished by the emotional content of my life, and the latter began to consist almost entirely of my students. And so now I thank them all for everything that they brought into my life, most of all for simply being there and continuing to be there.*

Pavel Sergeevich Aleksandrov died on 16 November 1982 in the arms of his students A. A. Mal'tsev and F. Gadzhiev. His ashes are buried in a small rural cemetery in the grave of his mother, not far from the city of Pushkino near Moscow. His students still sometimes go to the grave and recall that unforgettable time when they studied with him. And almost all of them believes secretly that he personally was the center of concern and attention of his extraordinary teacher. I also will not part with that illusion.

2.

LEV SEMENOVICH PONTRYAGIN (1908–1988) began his scientific career as a second-year student in the Department of Physics and Mathematics at Moscow University under the direction of P. S. Aleksandrov. His interests in this early period were concentrated around two central problems of algebraic (combinatorial) topology of the time — topological duality theorems and dimension theory. His very first mathematical papers proclaimed the birth of a new school of research within the school of Aleksandrov — the topological school of Pontryagin, which was to become famous worldwide and indisputably exerted a direct or indirect influence on the development of many central areas of mathematics.

A complete survey of the topological papers of Pontryagin would require an entire book, but possibly such a book is not needed now, since we have the three-volume Russian edition *L. S. Pontryagin. Selected Works* (Moscow: Nauka, 1986) and the four-volume English version *L. S. Pontryagin. Selected Works* (New York etc.: Gordon and Breach, 1986). These selections of his works, assembled by Gamkrelidze, together constitute nearly a complete collection of the works of Pontryagin and are accessible to all. Nevertheless, I shall write a brief essay using the survey written by four of Pontryagin's students — Anosov, Gamkrelidze, Mishchenko, and Postnikov — and published in the first volume of the abovementioned Russian edition of the selected works.

Let me begin with a few words about Pontryagin's early papers in duality theory and topological algebra. To gain a full appreciation of the work, it is proper to recall that when his career began the concept of a homology group in topology was virtually unused — it had been replaced by the Betti numbers modulo different bases and the torsion coefficients, and the Alexander duality law was stated as the fact that for a polyhedron $K \subset \mathbb{R}^n$ the Betti numbers mod 2 of dimension $n-r-1$ are equal to the numbers for its complement $\mathbb{R}^n \setminus K$ of dimension r. Thus, $p^r(\mathbb{R}^n \setminus K) = p^{n-r-1}(K)$.

Pontryagin made this law deeper, extending Alexander duality to a duality between the r-dimensional and $(n-r-1)$-dimensional *homology groups* mod 2 of the polyhedra $\mathbb{R}^n \setminus K$ and K. This extension, which he achieved using linking numbers, led to the isomorphism of the corresponding groups.

Pontryagin then went further, extending these considerations, still mod 2, but for polyhedra K embedded not in $\mathbb{R}^n$, but in an arbitrary closed n-dimensional manifold M^n. The solution of this problem required (apparently for the first time in the history of topology) the consideration of the topological properties of continuous mappings, which was to become one of the main sources of homological algebra.

One of Pontryagin's significant results was the *cycle removing theorem*, which asserts that if an r-dimensional cycle Z^r of M^n has zero intersection index with every $(n-r)$-dimensional cycle in K, then Z^r can be homologically "removed" from the polyhedron K; that is, there exists an r-dimensional cycle homologous to Z^r in M^n and located entirely within $M^n \setminus K$. This theorem later found application in the topological theory of variational problems. It is clear from what has been said just how far one of the central problems of algebraic topology in the late 1920s had been advanced by a 19-year-old second-year student.

Among the early topological papers of Pontryagin are his papers on dimension theory. Here he constructed examples of compact metric spaces having different dimension over different moduli, which were then used to construct *dimensionally defective continua*, refuting the conjecture that when the Cartesian product of topological spaces is taken, their dimensions add. The abovementioned theorem that every n-dimensional compact space can be homeomorphically embedded in $\mathbb{R}^{2n+1}$ should also be classified among Pontryagin's dimension-theory papers.

But let us return to duality theory. After the early papers strengthening Alexander's law of duality mod 2, Pontryagin made a long and difficult route to establish duality relations modulo an arbitrary m, and then also for the complete homology groups with integer coefficients. The solution of this last problem required the introduction of a new homological invariant of a compact space F — its homology group with a compact, rather than discrete, coefficient group — which made it possible to dispense with the view of duality as an isomorphism and define it as *duality in the sense of Pontryagin*. This radical step, which gave a complete solution to all the duality problems in the area and also the long-standing problem of a satisfactory definition of the homology groups of compact metric spaces, was taken by Pontryagin in 1931–1932 and consisted of the following.

The coefficients used in constructing the homology group $H^p(F)$ are taken not from the discrete group of residues mod 2, but from the compact topological group of rotations of a circle. The group $H^p(F)$ itself is also a compact topological group in this case. It turns out that the group $H^p(F)$ and the integer group $H^{n-p-1}(\mathbb{R}^n \setminus F)$ are *dual in the sense of Pontryagin*, that is, each is the *character group* of the other.

Pontryagin's research in topological duality theorems was essentially perfected with this theorem. This research gave a complete solution of the central problem of algebraic topology during the 1930s, and was simultaneously a powerful method of studying general homological problems of topology.

An immediate and logical extension of the papers on duality theory was the creation by Pontryagin of the general theory of characters of locally compact commutative groups. Its central result was the theorem that every compact commutative topological group is the character group of some discrete group. The proof of the theorem was based on the construction by Haar of an invariant measure (1933), which played an essential role in the development of topological algebra.

The general theory of characters enabled Pontryagin to elucidate the structure of compact and locally compact groups, obtaining definitive results in the compact and commutative cases. From these results, there followed in particular the positive solution of Hilbert's fifth problem for the case of compact and commutative locally compact groups.

The outcome of all this activity was the famous monograph of Pontryagin *Continuous Groups*, which was first published in 1938 and then reprinted many times in the USSR, as well as in many other countries in all the major European languages. This book is a classic, which formed the scientific worldview of many mathematicians, and is still of interest today!

Another paper on topological algebra contains the remarkable theorem of Pontryagin that asserts that every locally compact connected division algebra is isomorphic to one of the classical division algebras — the field of real numbers, the field of complex numbers, or, in the noncommutative case, the division algebra of quaternions. This theorem was proposed by Kolmogorov as an important element in the axiomatization of spaces of constant curvature (in particular, projective spaces), and he "ordered" Pontryagin to prove it. The full proof took a lot of time, although Pontryagin got the commutative case almost immediately, in the course of one week. Near the end of his life Lev Semenovich wrote that he regarded that theorem as one of his best papers in topology. But I wonder which of his papers is not "one of the best."

I have already mentioned the early papers of Pontryagin on dimension theory, in particular, homological dimension theory, which dovetails with some outstanding papers of Aleksandrov. These papers gave Lev Semenovich an impetus to study *homotopic* problems of topology in the mid-1930s.

The central problem of the initial period of development of homotopy theory was the problem of homotopic classification of mappings of spheres onto spheres of lower dimension. Pontryagin arrived at this problem while carrying out an order of Aleksandrov to give a local characterization of the dimension of a compact set lying in $\mathbb{R}^n$ in terms of the homological characteristics of its complement.

Lev Semenovich at first tried without success to solve the problem of homotopic classification of S^{n+k} on S^n by homological methods, but then, after learning of a paper by Heinz Hopf on classes of mappings of S^3 onto S^2, switched over entirely to homotopic methods.

He first proved that the classification of the mappings of S^3 onto S^2 obtained by Hopf was complete, that is, gave all the classes of mappings, and then he obtained a surprising result — the number of classes of mappings of S^{n+1} onto S^n for $n \geqslant 3$ is 2 (1937). He obtained this same result for the classification of mappings of S^{n+2} onto S^n, at first, however, with an erroneous proposition that he corrected only in 1950.

However, the central problem — the problem of classification of mappings of S^{n+k} onto S^n for $k \geqslant 3$ did not yield. It led Pontryagin to the method of *framed manifolds*, the discovery of new invariants of smooth manifolds — *the Pontryagin characteristic classes*, and the theory of *fiber spaces*. This meant the appearance of a new and very important division of contemporary mathematics — differential topology, in which there immediately appeared alongside Hopf other bright names — Stiefel, Whitney, Chern.

Using the method of framed manifolds, mathematicians had succeeded by the early 1950s in classifying the mappings of S^{n+k} onto S^n only for $k = 1$ and 2 (by Pontryagin himself), and $k = 3$ (by V. A. Rokhlin). For $k > 3$ the method required information on smooth manifolds of dimension greater than 3, which was not available at the time. Since that time, the most profound results in the theory of smooth manifolds have been obtained by a combination of the differential-geometric method of Pontryagin and Thom, and Leray's algebraic method of *spectral sequences*.

At present, characteristic classes are the central object, not only of differential topology, but of all modern differential geometry, and the theory of fiber spaces long ago became an important method of study in various divisions of mathematics.

For a long time the problem of topological invariance of the characteristic classes was one of the central problems in the topology of manifolds. Pontryagin himself studied it, but it was solved only in the mid-1960s by S. P. Novikov, using methods developed since the 1950s. It turned out that if the rational numbers are chosen as the coefficient field, the characteristic classes are indeed topological invariants of the manifold.

Of the nonhomotopic papers of Pontryagin, I would like to point out another remarkable paper from 1935. That paper gives the solution of a problem of É. Cartan on the computation of the homology groups of compact group manifolds of the four basic series of compact Lie groups. The main idea of the

A visit with Marston Morse in Princeton (1964). *Seated*: Solomon Lefschetz, L. S. Pontryagin; *Standing*: Mrs. Lefschetz, E. F. Mishchenko, Miss Morse, Marston Morse, A. I. Pontryagina, R. V. Gamkrelidze

solution of this problem was based on Morse's method of defining a smooth function on a manifold with isolated critical points, and on constructing the trajectories orthogonal to the level surfaces of the function.

In 1955, Pontryagin published his last paper on topology, "Smooth manifolds and their application in homotopy theory" (*Proceedings of the Steklov Institute*, **95**). By that time he had switched over completely to research in applied areas. In this he was followed by two of his outstanding students — V. G. Boltyanskii and R. V. Gamkrelidze.

Boltyanskii was already the author of papers in dimension theory and homotopy theory, which formed part of his doctoral dissertation. Gamkrelidze also had written a doctoral dissertation, after writing several papers in algebraic geometry — on Chern cycles of algebraic manifolds. Subsequently, they both became leading figures in the mathematical theory of optimal control and enjoyed worldwide fame.

Two other outstanding students of Pontryagin — Rokhlin and Postnikov — had set out on their own voyage somewhat earlier. Many now consider Rokhlin the "best mathematician of his generation, who also strongly influenced the entire development of mathematics in Russia" (V. I. Arnold, "On V. A. Rokhlin," in: *Selected Works of V. A. Rokhlin*, Moscow Center for Continuous Mathematical Education, 1999).

Formally, Pontryagin was not Rokhlin's academic advisor, but Rokhlin can be considered his student without stretching the truth. In any case, at the beginning of his career Rokhlin was in constant contact only with Pontryagin, and this contact had a decisive influence on his choice of the topic for his research. In developing Pontryagin's method of framed manifolds, Rokhlin obtained the first brilliant results in the smooth topology of three-dimensional manifolds, which essentially led to the later creation of bordism theory. Nearly all of Rokhlin's topological papers in the early period are sprinkled with phrases such as: "the construction of mappings according to Pontryagin"; "the Pontryagin method of reducing homotopic problems to problems of fields of frames"; "Pontryagin's conjecture that a closed three-dimensional manifold is the boundary of . . . "; "Pontryagin's vector problem"; "the combinatorial invariance of the Pontryagin classes"; "the Hirzebruch–Pontryagin classes"; and so forth.

Lev Semenovich once told me an interesting story connected with Rokhlin, and I think it would be in order to repeat it here. The story, in my opinion, is not a bad indication of the characteristic relations between Pontryagin and his student when the latter had fallen into extreme difficulty.

Shortly before the outbreak of World War II, Lev Semenovich already knew Rokhlin as the best student attending his lectures and seminars in topology. During the war Rokhlin found himself at the front, was taken prisoner by the Germans, liberated by the Americans, and then found himself in a Soviet filtration camp, like the majority of the surviving prisoners. When Pontryagin learned of this, he risked his own safety, with some assistance from Aleksandrov and Kolmogorov, and took extreme measures to get Rokhlin freed. He appealed twice to Beria, who was the head of the *KGB* at the time. After getting Rokhlin freed, he provided him with a permit to live in Moscow and hired him at the Steklov Institute as his own assistant.

Pontryagin appreciated Postnikov's talent from their first acquaintance, but did not pay sufficient attention to his major papers. It was the British topologist Peter Hilton who first called attention to these papers in *Mathematical Reviews*. The first to do so in the USSR was Shafarevich. In 1960 he nominated Postnikov's papers on homotopic topology for the Lenin Prize, which Postnikov received in 1961.

Here is a summary of Postnikov's major result. The simplest homotopic invariants of a (connected) space X are its homotopy groups $\pi_n X$. However, they do not characterize the homotopy type: there exist homotopically inequivalent spaces having the same groups. Such spaces necessarily have nonzero homotopy groups in at least two dimensions, while spaces, whose homotopy groups are all trivial with only one exception $\pi = \pi_{n_0} X$, have the same homotopy type (which is denoted $K(\pi, n_0)$). Postnikov's main theorem asserts that, up to homotopy type, every space X can be uniquely decomposed into a sequence of fibering $X_{n+1} \to S_n$ (now called the *Postnikov tower*) whose fibers are the spaces $K(\pi_{n+1}X, n+1)$. Each such fibering is characterized by some cohomology class $k_n \in H^{n+2}(X_n, \pi_{n+1}X)$, now called the *Postnikov invariant*, and the homotopy type of the space X can be unambiguously recovered from its homotopy groups and Postnikov invariants.

However, let us return to the beginning of the second period in Pontryagin's career — his switch from topology to applied mathematics. This transition was abrupt, and surprised many people. Actually, it was natural and in complete accord with the whole nature of Lev Semenovich. The fact that new methods in topology, different from those of Pontryagin, had appeared in the West (and, in particular, had enabled J. Leray to classify the mappings of the sphere S^{n+k} onto S^n for any k) was not the main reason, or even a secondary reason for changing areas. The main reason lay deeper. Here is what Pontryagin himself said at the sunset of his life:

> Despite the fact that I had successfully, perhaps even brilliantly, conducted research work just after graduating from the university, worry began to gnaw at me. I could not answer the question of what it was all needed for, the things that I was doing.
>
> The most vivid imagination could not lead me to believe that homological dimension theory would ever be of any practical use. But it was of practical uses, that is, practical applications of mathematics, that I dreamed... At that time — it must have been around 1932 — Andronov suddenly came to me, a young, talented, energetic, brilliant physicist, with a proposal to do some joint research. He told me about Poincaré limit cycles, periodic trajectories and things of that sort... Under his influence I held a joint appointment for a year as a member of the staff of the Institute of Physics and wrote a paper on dynamical systems that are nearly Hamiltonian. [*The Life of Lev Semenovich Pontryagin, Mathematician, Written by Himself*, Moscow: "Prima V" Private Publisher, 1998]

Further,

> The question of what should be studied is more acute for mathematicians perhaps than for specialists in other areas of knowledge. Mathematics, which arises as a purely applied science, still has, as its main task at present, to study the material world around us in order to use it for the needs of humanity.
>
> At the same time, it has its own internal logic of development, in accordance with which mathematicians create concepts and even whole divisions of their subject that are the product of purely mental activity, in no way connected with the material world around us and having no applications at present. These divisions often fit together wonderfully and have a certain kind of beauty, but that kind of beauty is not a justification for their existence. Mathematics is not music, whose beauty is accessible to the great mass of people. Mathematical beauty can be understood only by a few specialists . . .
>
> In this situation, the question of the choice of topics for research becomes very anxiety-producing for mathematicians. I believe that if not all, then at least many mathematicians should turn to the original sources in their work, that is, to the applications of mathematics. This is necessary both to justify its existence and to inject a fresh stream into research.

Thus the thought that, even if one does not invent new engineering machinery, one may at least glean the statement of new mathematical problems from the needs of natural science and technology, had been latent in Pontryagin's consciousness for many years. And in the early 1950s he made a firm decision. From that point on everything happened naturally — a new research school of Pontryagin arose and began to develop rapidly, soon bringing him worldwide glory, not only in different circles of mathematicians, but in the wider society of scholars in other specialties, including engineers studying what was then called the *new technology*. A central event in the activity of this school was undoubtedly Pontryagin's discovery of a maximum principle, which has entered the history of mathematics as the *Pontryagin maximum principle*. A separate article by Gamkrelidze devoted to this famous mathematical theorem is published in the present volume.

In recent years an innumerable collection of books and articles on the mathematical theory of optimal processes has appeared both in Russia and in the West. The point of departure for these books is the Pontryagin maximum principle.

This current is immense and is spreading in different directions. Here I would like to note just one side of the maximum principle, whose significance has been clarified comparatively recently. The question is that its original Hamiltonian form is invariant both relative to smooth changes of variable in the state space and relative to feedback transformations, which in geometry correspond to gauge transformations.

For that reason, the maximum principle immediately became a very important tool of geometric control theory, which arose at the turn of the 1970s in the USA, and starting in the mid-1970s has also been actively developed in Russia, France, and other countries. This theory studies first of all smooth nonlinear systems on manifolds, and by coordinate-free methods. Originally, the main successes of the geometric theory were connected with the penetration of control into the problem and with higher-order optimality conditions, as well as with the characterization of controllable systems equivalent to linear systems with respect to some transformation group.

Much later, in the 1980s, a way was found to construct the fundamental invariants of the nonlinear systems responsible for the structure of an optimal synthesis. In particular, people succeeded in constructing a canonical, generally speaking nonlinear, connection associated with a controllable system and providing a great generalization of the Levi-Civita connection from Riemannian geometry, and for defining the "curvature" of optimal control problems. The linearization of the maximum principle, which was in a certain sense artificial, is a basis of these constructions.

It is interesting that by following this route one can express the characteristic Pontryagin classes in terms of the curvature of an optimal control problem. In other words, one is generalizing the famous relations once obtained by Pontryagin and Chern in the context of Riemannian geometry — that is, for problems of minimizing Riemannian length. Thus it is suddenly revealed that Pontryagin's research areas, which he himself regarded as completely independent of each other, actually have a deep internal connection. Work in this new area is being carried on at present by Gamkrelidze and Agrachev.

Another student of Pontryagin — M. I. Zelikin — made fundamental advances in the solution of the problem of constructing an optimal synthesis for multidimensional nonlinear problems, especially in the presence of an infinite number of switchings in a finite time interval, which does not admit a direct application of the maximum principle. In his papers he makes use of both the classical machinery of optimal control theory and the modern geometric methods such as the geometry of Lie groups and homogeneous spaces, the theory of foliations, the resolution of singularities of degenerate mappings, and others.

I note finally that this work is being continued in Moscow by the students of Pontryagin's students — A. V. Arutyunov, S. M. Aseev, the late V. I. Blagodatskikh, and M. S. Nikol'skii. In the area of *nonsmooth analysis*, whose development was stimulated to a significant degree by mathematical control theory, successful work is being done by Yu. S. Ledyaev, in close collaboration with the famous French mathematician F. Clarke — one of the founders of nonsmooth analysis.

However, it seems to me that the summit of the development of the Pontryagin–Gamkrelidze–Boltyanskii[4] mathematical theory of optimal control, as originally interpreted by its founders, has already been reached and interest is now shifting to more abstract areas. How significant this progress will be, only time will tell. But the very first papers of Gamkrelidze and Agrachev promise many interesting things.

Let us now examine in more detail another significant area in Pontryagin's school of research — the theory of singular perturbations in differential equations. Pontryagin became acquainted with Poincaré's theory of small perturbations from Andronov. He very much enjoyed telling beginners about the phase plane of a second-order differential equation, about regular — smooth or analytic — perturbations of this plane by a small quantity ε, how the solution of the perturbed system can be expanded in integer powers of ε, about bifurcations of equilibrium positions, about the concept of a *structurally stable system*, introduced into mathematics jointly with Andronov, and the like.

In 1952, Lev Semenovich and I studied the book *Theory of Oscillations* by Andronov, Vitt, and Khaikin, and learned about radiotechnological devices in which periodic motions occur that are not of limit-cycle type, but the so-called *relaxation oscillations* — periodic motions containing alternating portions of slow phase variations and rapid, almost instantaneous ones. We succeeded in describing the work of a few such systems in a purely mathematical way, without introducing any physical hypotheses, using differential equations containing a small parameter ε on the higher-order derivatives.

Thus we arrived at the general problem of studying systems of differential equations of the form

$$
\begin{aligned}
\varepsilon\dot{x} &= f(x,y),\\
\dot{y} &= g(x,y),
\end{aligned}
$$

[4] At this point I prefer to list the founders of the mathematical theory of optimal control in the chronological order of the early stages, rather than alphabetical order: 1) Pontryagin — discovery of the maximum principle; 2) Gamkrelidze — proof of the maximum principle for linear systems and studies of the second variation; 3) Boltyanskii — proof of the maximum principle in the nonlinear case.

where x and y are vectors in $\mathbb{R}^k$ and $\mathbb{R}^l$ respectively. Here, in the beginning, we were interested mostly in relaxation oscillations: the conditions for them to arise, how to compute them approximately, the study of bifurcations in neighborhoods of points where there is a jump, and the like. We studied these problems for three or four years, and in 1955 published our note "Periodic solutions of systems of differential equations near to discontinuous ones" in the *Doklady Akademii Nauk SSSR* (**102**(5), 889–891).

Kolmogorov, who at first did not approve my work with the "vacuum tube generator" and the like, after reading this note, immediately formed a high opinion of it and, as Aleksandrov told me, even said that several doctoral dissertations would grow out of this note. Much later, when the monograph *Mathematical Theory of Optimal Processes* by Pontryagin, Boltyanskii, Gamkrelidze, and Mishchenko was nominated for the Lenin Prize, Kolmogorov proposed that our note be attached to the nomination, and this was done.

Now let me tell a bit about our joint work. In the winter of 1951, Lev Semenovich, after one of the seminars led by him together with Aleksandrov, invited me to go skating at the rink in the center of Moscow on Petrovka Street. I was still an undergraduate, but I had already read *Continuous Groups* and was very nervous, foreseeing a high-level conversation. However, it turned out to be very simple and friendly, unusually quiet, and we skated placidly for an hour or two holding hands. Having a great deal of experience in cross-country skiing (acquired in the North during the war) I somewhat hesitantly proposed to Lev Semenovich that he come skiing with me. Without a second's hesitation he agreed, and I was utterly amazed at the ease with which he ignored his great physical handicap — total blindness.

L. S. Pontryagin and P. S. Aleksandrov at a seminar

Several days later the project got under way, and everything went surprisingly well both for him and for me. After that, we roamed around various places in the Moscow suburbs two or three times a week every winter for seven or eight years, with me leading the way, sometimes breaking a fresh cross-country trail, sometimes over an established one, and Pontryagin behind me. And nearly always during these skiing trips we were constantly calculating, proving lemmas and theorems, only occasionally interrupting these activities for conversations about literature and other topics or to get across ravines. I remember one occasion when we got lost in the woods and ravines on the way from the Uzkoe

Sanatorium to the place where the new University was being built on the Lenin Hills, and that trip lasted six or seven hours.

In the end, all of our conversations while on skis reduced to singularly perturbed systems. We tried different versions of gluing trajectories together, computed in our heads the asymptotics of several special solutions of the Riccati equation (although, as I learned much later, we could have used ready-made formulas from the theory of Bessel functions) and the like. I never took a pencil and paper with me, and we sometimes had to stop and write out computations in the snow.

I must say that in the first winter of our research it was unbelievably difficult for me, and I was close to despair. I had studied differential equations using the university textbooks of Stepanov and Petrovskii (which, by the way, do not cause me to wax nostalgic even today), and Lev Semenovich was forced to finish teaching me and teach me all over again, since for him a differential equation meant primarily a dynamical system and Poincaré had been his favorite mathematician for a long time. I submitted to the re-education with difficulty, but Lev Semenovich never scolded me for being slow and was finally satisfied with me. Despite the fact that all our research at first was dominated by him, he put my name first on our note in the *Doklady*.

After our research on the cross-country ski tracks, Pontryagin almost never returned to the theory of singular perturbations. There were only two or three sporadic exceptions in his work with his graduate students. However, this whole area owes its conceptual summit to Pontryagin, and we note that there were many people working in this area: Van der Pol (Netherlands); Dorodnitsyn; Haag (France); Tikhonov with his students A. B. Vasil'eva, V. M. Volosov, and V. F. Butuzov; M. Cartwright (Britain); Stoker (USA). Although in later years I myself and my students (N. Kh. Rozov and A. Yu. Kolesov) wrote many papers that greatly enlarged the sphere of research (in particular, by extending it to hyperbolic and parabolic equations, and studying many new phenomena), our first achievements still seem particularly important to me. To be sure, even my later research and the efforts of my students Rozov and Kolesov were not easy. To get an idea how the method of analytic study of the behavior of trajectories of singularly perturbed systems differs from Poincaré's method, I give as an example the asymptotic formula for the period of a relaxation oscillation:

$$T = T_0 + \sum_{n=2}^{\infty} \varepsilon^{\frac{n}{3}} \sum_{\nu=0}^{\pi(n-2)} T_{n,\nu} \ln\frac{1}{\varepsilon},$$

where $\pi(q)$ is a certain special integer-valued function of its integer argument q and $T_{n,\nu}$ are numerical coefficients for whose computation a recursive procedure is indicated. This formula, of course, is not for practical applications, but it shows how peculiar and complicated the departure of a perturbed cycle from the unperturbed may be.

Differential equations with a small parameter were also studied by Anosov, a student of Pontryagin and partly a student of mine. His inclination to study any mathematical problem that he took up with microscopic attention to detail revealed itself in his early student investigations. Even in his second year, Anosov began to participate actively in Pontryagin's research seminar and wrote, seemingly under my direction, his first small research paper. When I praised him for the result, he announced solemnly that he would do his doctoral dissertation (note: he did not say senior thesis or *kandidat* dissertation, but doctoral dissertation) on *structurally stable systems*. I was secretly delighted with such self-confidence, and after a year or two I realized that he would fulfill his plan and even more. In the late 1950s he wrote two good papers on the theory of singularly perturbed systems, one of which — on averaging in systems with rapidly oscillating solutions — later received significant extensions in the papers of A. I. Neishtadt, which also became widely known.

After entering the elite group of mathematicians of his generation at a very early age, immediately after writing the monograph *Geodesic Flows on Surfaces of Negative Curvature*, Anosov remains in this group of — alas, no longer young — mathematicians to this day. Having fallen into the sphere of those enchanted with the hyperbolic revolution in dynamical systems, he continues to regard the Smale horseshoe as one of his idols. Sometimes he makes an excursion into other areas. Thus, I think, it was not without his influence and participation that the results of Bolibruch on Hilbert's 21st problem became known rapidly.

Pontryagin tried many times to attract Anosov into the mathematical theory of control, but he firmly and consistently refused, although he always came to Pontryagin's seminar and once in passing even thought up a simple example of a differential game called "the boy and the crocodile," the study of which was later undertaken by many in their *kandidat* dissertations (although, despite its outward simplicity, the problem has not yet been solved completely).

Game-theory problems, strictly speaking are not optimization problems, even though each of them contains an objective control realized either through human will or by mechanisms. However, their mathematical formulation also arises naturally from the needs of applications, just like the statement of problems in the mathematical theory of optimal processes. Thus, the practical problem of one airplane pursuing another (or evading another) can be formulated as a *differential pursuit–evasion game*[5] of two points of a phase space whose laws of motion in $\mathbb{R}^n$ are given by two systems of differential equations

$$\dot{x} = f(x,u), \tag{a}$$
$$\dot{y} = g(y,v), \tag{b}$$

where u and v are control parameters.

In the school of Pontryagin interest in differential games of this kind arose in the early 1950s. However, the obvious difficulties of the problem, and the concentration of effort at the time on developing the maximum principle somewhat dampened this interest. It was only in 1956 that Lev Semenovich made several new attempts that did not at first lead to any clear success.

Lev Semenovich and I made one of those attempts jointly, after simplifying, as we thought, the statement of the problem. To be specific, we replaced the object (b) with a random Markov point ξ with a given transition density wandering in the same space $\mathbb{R}^n$, and posed the problem as follows: *What can be said about the control $u(t)$ of the motion $x(t)$ that maximizes the probability that the random point ξ will enter a small neighborhood $V_{x(t)}$ of the controlled point $x(t)$ during some time interval $t_1 \leqslant t \leqslant t_2$?*

Since this probability is obviously a functional of the control, it appeared that the problem could be solved using the maximum principle. We computed this functional, in the process passing through many stages of an approximate solution of a boundary-value problem for the parabolic Fokker–Planck–Kolmogorov equation, and the answer turned out to be horribly cumbersome.

Kolmogorov, after reading our note in the *Doklady*, proposed (in a letter to me) a simpler formula, which he had obtained from intuitive physical considerations, but without a rigorous proof. A delicate situation arose, and Pontryagin asked me to find a mistake either in our work or in the work of Kolmogorov. Fortunately, there were no mistakes, and the one reduced to the other by means of a small lemma.

After that, as the result of some “shuttle” talks with both Pontryagin and Kolmogorov, I was told to write a note of the three authors for the *Doklady*. I did it, thereby becoming not only the intermediary between two great mathematicians, but also a participant in a small adventure in their joint research.

Unfortunately, our formulas, which were perhaps of some interest for the theory of probability, did nothing for the theory of differential games, since the maximum principle is far from providing a complete answer to the question

[5] This term seems to have been introduced almost simultaneously by Isaacs in the USA and Pontryagin in the USSR.

of the nature of the control that minimizes a given functional. That is why Pontryagin continued his search for a genuine result. This result appeared in 1966 in the form of a very complicated theorem in whose proof Lev Semenovich made use of the methods of optimal control theory. However, the application of that theorem even to a linear differential pursuit game turned out to be complicated.

Here is what Lev Semenovich himself wrote on this point:

> In the late 1970s Mishchenko and I applied the theory I had constructed to a linear differential game. The result we obtained at first suggested a condition that would be sufficient for the pursuit game to terminate.
>
> By such a long roundabout route we had arrived at a solution of a linear differential pursuit game.
>
> On this long and difficult route there were no flashes of insight, that is, sudden guesses. Everything yielded with extreme difficulty. Even more difficult was the route to the solution of the evasion problem even for a linear differential game. Mishchenko and I traveled this route almost to the end. All our attempts to connect the evasion game and the pursuit game were fruitless. The two problems had to be studied completely independently of each other. [*Proceedings of the Steklov Institute*, 1985, **169**]

It is possible that the reason these attempts were fruitless lay in the Pontryagin formalization of the game itself and, in particular, in the concept of a strategy.

Simultaneously with the Pontryagin formalization there appeared other formalizations. The most successful, it seems to me, although not as simple, was the formalization of N. N. Krasovskii. He and his students managed to avoid contact with the unsolvability of the pursuit–evasion problem and succeeded in getting the solution to many applied problems.

Nikolai Nikolaevich Krasovskii is the founder and leader of the Sverdlovsk (Ekaterinburg) school of research, in which the mathematical theory of control processes was the main area of research.

At an early stage the research in this school was stimulated to a significant degree by the Pontryagin maximum principle, whose statement Nikolai Nikolaevich seems to have learned at one of Pontryagin's seminars in Moscow, even before it appeared in print. He immediately realized that he was dealing with a great discovery and, when he got back to Sverdlovsk after his trip to Moscow, he switched the attention of his students into the area of control theory, which, however, coincided with specific requests from many engineers and designers working in the Sverdlovsk region at the time to create the *new technology*. Soon he himself and his students published the first articles and monographs on the mathematical theory of control, going far beyond the confines of the circle of problems that Pontryagin's school was working on.

At present the Ekaterinburg school of mathematicians, which is headed by Nikolai Nikolaevich Krasovskii and has produced such well known scholars as Yu. S. Osipov, A. B. Kurzhanskii, A. I. Subbotin, A. V. Kryazhimskii, A. G. Chentsov, and others, is deservedly regarded as one of the most significant in Russia. The influence of Pontryagin's scientific ideas on its activity is admitted, I believe, by the entire group of that school.

* * *

Pontryagin lived a long and extraordinary life. He described it in two autobiographical works: *A Brief Description of the Life of L. S. Pontryagin, Compiled by Himself* (*Uspekhi Matematicheskikh Nauk*, 1978, **33**(6)) and *The Life of Lev Semenovich Pontryagin, Mathematician, Written by Himself* (Moscow: "Prima V" Private Publisher, 1998). From them the reader may obtain a rather complete picture of the personality of Pontryagin.

Some parts of these autobiographies have evoked gossip in various mathematical circles. Pontryagin's social activity has been discussed with particular vigor (and by no means in calm tones), as has his work on the executive committee of the International Mathematical Union, of which he was a member from 1970 to 1978. Such discussions have even appeared on the pages of certain international nonmathematical publications from time to time. However, all this, even if it is of interest for historical *meta*mathematical research, has nevertheless only a remote relation to the history of mathematics as a science.

I think it is now generally recognized that, by virtue of the results of his papers and his scientific influence, Lev Semenovich Pontryagin occupies a secure place among the great mathematicians of the twentieth century.

Yu. V. Nesterenko

Hilbert's Seventh Problem

Translated by L. P. Kotova

To Alan Baker on his sixtieth birthday

In Paris on 8 August 1900, at a joint session of the sections of history and bibliography, pedagogy and methodology of the Second International Congress of Mathematicians, D. Hilbert gave a lecture entitled "Mathematical Problems." It is generally recognized that this event, which occurred at the very beginning of the twentieth century, exerted a tremendous influence on the subsequent development of mathematics. In this article we shall talk about the consequences of only one problem posed by Hilbert in his lecture, namely the seventh one: "Irrationality and transcendence of certain numbers."

It should be noted that by the end of the nineteenth century C. Hermite and F. Lindemann had solved completely the question of transcendental values of the function e^z at algebraic points:

Theorem 1. *For each algebraic number $\alpha \neq 0$ a value of the exponential function e^{α} is transcendental.*

This statement contains transcendence of the numbers e and π, as particular cases, and transcendence of natural logarithms of algebraic numbers as well. Moreover, Lindemann claimed, and in 1885 K. Weierstrass published the proof of a more general fact:

Theorem 2. *If algebraic numbers $\alpha_1,\dots,\alpha_m$ are linearly independent over the field of rational numbers, then there are no algebraic relations with rational coefficients among the values $e^{\alpha_1},\dots,e^{\alpha_m}$. In other words, the values of the exponential function are algebraically independent over the field of rational numbers $\mathbb{Q}$.*

Many mathematicians, and Hilbert was among them, simplified and perfected proofs of these results. The next principal step was to study the values of the exponential function at transcendental points. It is this problem which was formulated by Hilbert.

Hilbert's seventh problem. *Prove that a^b, where an algebraic base a differs from 0 and 1, and b is an algebraic irrational number, is a transcendental number.* For example, the numbers $2^{\sqrt{2}}$ and $e^{\pi} = i^{-2i}$ are transcendental.

It is easy to see that this statement can be formulated differently:

A logarithm $\log_\alpha \beta$, where α, β are algebraic numbers and $\alpha \neq 0, 1$, is either a rational or a transcendental number.

A. O. Gel'fond

In 1929 A. O. Gel'fond proved the required statement for an imaginary quadratic b. In particular, it has been proved that e^{π} is transcendental. In 1930 R. O. Kuz'min extended Gel'fond's idea for the case of a real quadratic b. The final solution of the problem was obtained in 1934 by Gel'fond and independently by T. Schneider.

Hilbert's seventh problem gave rise to two directions of research in the framework of modern theory of transcendental numbers corresponding to the two formulations given above. We shall start from the results which generalize the statement that logarithms of algebraic numbers with an algebraic base are transcendental.

1. Bounds of Linear Forms Containing Logarithms of Algebraic Numbers

In 1969 A. Baker proved the following theorem.

Theorem 3. *Let $\ln \alpha_1,\dots,\ln \alpha_m$ be fixed natural logarithms of algebraic numbers $\alpha_1,\dots,\alpha_m$. If these logarithms are linearly independent over $\mathbb{Q}$, then the* *numbers*

$$1, \ln \alpha_1, \ldots, \ln \alpha_m$$

are linearly independent over the field of algebraic numbers.

It is clear that this result generalizes the second formulation of Hilbert's seventh problem. This gives rise to some corollaries which are given below.

Corollary 3.1. *If $\alpha_1, \ldots, \alpha_m, \beta_1, \ldots, \beta_m$ are algebraic numbers and*

$$\gamma = \beta_1 \ln \alpha_1 + \cdots + \beta_m \ln \alpha_m \neq 0,$$

then γ is a transcendental number.

This means, the number

$$\int_0^1 \frac{\mathrm{d}x}{x^3+1} = \frac{1}{3} \ln 2 + \frac{\pi}{3\sqrt{3}},$$

is transcendental, and in general any integral

$$\int_a^b \frac{A(x)}{B(x)}\,\mathrm{d}x,$$

is transcendental. (Here a, b are real algebraic numbers, the polynomials $A(x)$, $B(x)$ are non-negative on the interval $[a;b]$ with $\deg A < \deg B$, and the polynomial $B(x)$ does not have multiple roots.)

Corollary 3.2. *If $\alpha_1, \ldots, \alpha_m, \beta_0, \beta_1, \ldots, \beta_m$ are algebraic numbers and $\beta_0 \neq 0$, then*

$$\mathrm{e}^{\beta_0} \alpha_1^{\beta_1} \cdots \alpha_m^{\beta_m}$$

is a transcendental number.

Corollary 3.3. *Let $\alpha_1, \ldots, \alpha_m$ be algebraic numbers different from* 0 *and* 1*, and let $\beta_1, \ldots, \beta_m$ also be algebraic numbers with $1, \beta_1, \ldots, \beta_m$ linearly independent over $\mathbb{Q}$. Then*

$$\alpha_1^{\beta_1} \cdots \alpha_m^{\beta_m}$$

is a transcendental number.

The quantitative variant of Theorem 3 has even more interesting and important consequences.

Perhaps it seems verisimilar, that the terms of two sequences 2^r and 3^k cannot be sufficiently close to each other at large degrees r and k. More generally, one may pose a question about the distance between products of powers of the fixed numbers $\alpha_1, \dots, \alpha_{n-1}$. Or how close to 1 are the numbers $\alpha_1^{\beta_1} \cdots \alpha_{n-1}^{\beta_{n-1}}$, if the β_i are negative or even arbitrary algebraic numbers? And, lastly, how small can a nonzero linear combination $\beta_1 \ln \alpha_1 + \cdots + \beta_n \ln \alpha_n$ be? In the above case $\alpha_n = 1$ and $\ln \alpha_n = 2\pi i$.

When Gel'fond obtained the solution of Hilbert's seventh problem, he immediately posed a question about lower bounds of the modulus of a nonzero linear form

$$\Lambda = \beta_0 + \beta_1 \ln \alpha_1 + \cdots + \beta_n \ln \alpha_n \tag{1}$$

The point is that one has to obtain the bounds of $|\Lambda|$ in dependence of the arithmetic characteristics of the algebraic numbers α_k, β_k, that is, in dependence of their degrees and heights. In the following, the degree and height of an algebraic number α are denoted by $\deg \alpha$ and $H(\alpha)$ respectively.

During 1935–1952, Gel'fond published several bounds in the case $n = 2$, $\beta_0 = 0$ which were gradually improved. Many times he emphasized the importance of the general case for various applications. For example, in 1948 in a paper written together with Yu. Linnik, it was shown how a similar result in the case $n = 3$, $\beta_0 = 0$ could lead to obtaining all imaginary quadratic fields belonging with the class number (a problem going back to Gauss).

A. Baker

In the 1960s, A. Baker developed Gel'fond's ideas, and obtained the first results for bounds in the general case. The beautiful Theorem 3 mentioned above, and its corollaries, were just a by-product of this activity. Later on many articles were published, in which bounds were perfected in dependence of various parameters important for different applications. A. Baker himself, and also N. I. Feldman, H. Stark, M. Waldschmidt, and other mathematicians, made essential technical improvements in the proofs, which helped to perfect the initial results. The best bound among those published so far was obtained in 1993 by Baker and Wüstholz.[1] Below we shall give a special case of this result where the form (1) is homogeneous, that is, $\beta_0 = 0$ and the coefficients β_j are rational.

Theorem 4. *Let* $\alpha_1, \dots, \alpha_n$ *be algebraic numbers, and let* $\ln \alpha_1, \dots, \ln \alpha_n$ *be principal branches of their logarithms with* $D = \deg \mathbb{Q}(\alpha_1, \dots, \alpha_n)$. *Then for any*

[1] Recently the bound has been improved by E. Matveev.

integers $b_1, \dots, b_n$ *which satisfy the condition* $\Lambda = b_1 \ln \alpha_1 + \dots + b_n \ln \alpha_n \neq 0$ *the inequality*

$$\ln|\Lambda| \geqslant -(16nD)^{2n+4} \ln A_1 \cdots \ln A_n \ln H$$

is valid, where $A_j = \max(H(\alpha_j), \mathrm{e})$ *and* $H = \max(|b_1|, \dots, |b_n|, \mathrm{e})$.

Such bounds were first applied for an effective solution of Diophantine equations. In some cases the bounds of linear forms containing logarithms of algebraic numbers allowed one to find bounds for these solutions. For example, this applies to the Thue equation $F(x,y) = A$, where $F(x,y)$ is a homogeneous polynomial with integer coefficients and A is a fixed integer; or to equations of the form $y^m = f(x)$, where $m \geqslant 2$ and the polynomial $f(x)$ has integer coefficients. As a rule, these bounds are very large when solutions are obtained by a brute-force search. For example, it has been proved (A. Baker) that all integer solutions of the equation

$$x^2 - y^3 = k, \qquad k \in \mathbb{Z}, \quad k \neq 0,$$

satisfy the inequality

$$|x| + |y| < \exp\left(10^{10^5} |k|^{10^4}\right).$$

Nevertheless, it is possible sometimes to find all solutions by employing various accompanying circumstances. For example, the following result was proved by A. Baker and H. Davenport in 1968.

Theorem 5. *All solutions of the system of Diophantine equations*

$$3x^2 - 2 = y^2, \qquad 8x^2 - 7 = z^2$$

in natural numbers are the following triples

$$(1,1,1), \quad (11,19,31).$$

Another example is connected with the so-called Catalan problem. In 1844 E. Catalan assumed that there are no two sequential integers which are degrees of natural numbers except $(8,9)$. In other words, that the equation

$$x^m - y^n = 1, \qquad x, y, m, n > 1,$$

has only one solution expressed in integers: $x = 3$, $y = 2$, $m = 2$, $n = 3$. In 1976 R. Tijdeman, using the bounds of linear forms containing logarithms of algebraic numbers, proved that the collection of solutions to Catalan's equation is finite.

He obtained effective bounds of these solutions. Later on many mathematicians improved the bounds. To date the best result is the following

$$16 \cdot 10^6 \leqslant \min(m,n) \leqslant 8 \cdot 10^{11}.$$

Perfecting the upper bounds in this problem is connected with the theory of linear forms containing logarithms of algebraic numbers; and the progress in obtaining lower bounds requires usage of a specific character of an equation and involves laborious computer calculations. It is quite probable that Catalan's problem will be solved in the near future.[2]

The next fundamental problem, known as the *abc*-conjecture, links additive and multiplicative properties of integers. It was formulated by J. Oesterlé (in a less strong form) and D. Masser, and seems to be very difficult.

The *abc*-conjecture. *For any $\varepsilon > 0$ and any positive coprime integers a,b,c, which satisfy the condition $a+b=c$, the inequality*

$$c < \gamma_0 G^{1+\varepsilon},$$

is valid, where G is a product of all prime divisors of the number abc and γ_0 depends only on ε.

Effective bounds for solutions to Fermat's equation, and even for the more general equation

$$ax^\ell + by^m + cz^n = 0,$$

where a, b, c are fixed nonzero coefficients, and x, y, z, ℓ, m, n are positive variables satisfying the condition $(1/\ell) + (1/m) + (1/n) < 1$, follow from *abc*-conjecture with an effective dependence γ_0 on ε. Besides, this conjecture allows one to get other proofs of Mordell's conjecture and Roth's theorem on approximation of algebraic numbers by rational numbers.

In 1991 C. Stewart and Yu Kun-rui proved the inequality

$$c < \exp(G^{2/3} + \gamma/\ln\ln G),$$

where γ is an effective positive constant, by using bounds of linear forms in logarithms of algebraic numbers.

The theory of linear forms containing logarithms is transferred to elliptic and Abelian functions. Corresponding results also have interesting applications for the investigation of Diophantine equations, and also for matters concerning transcendence of elliptic and Abelian integrals.

[2] In 2002 the problem was completely solved by P. Mihăilescu.

In 1983 G. Wüstholz generalized Theorem 3. He proved that an analytic subgroup of a commutative algebraic group contains a non-trivial algebraic point if and only if it contains a nontrivial algebraic subgroup (all groups are defined on the field of algebraic numbers).

2. Generalizations to Other Classes of Functions

In 1934 Gel'fond and Schneider suggested a method to solve Hilbert's seventh problem, which allowed various generalizations. Later on Schneider published several such theorems. Here we give only one of them in a formulation due to S. Lang.

Theorem 6. *Let* $\mathbf{K}$ *be an algebraic number field of degree* D *and let* $f_1(z),\dots,f_m(z)$ *be functions meromorphic on the complex plane of order not higher* ρ*. Assume that the transcendence degree of the field* $\mathbb{C}(f_1(z),\dots,f_m(z))$ *is not less than 2, and the ring* $\mathbf{K}[f_1(z),\dots,f_m(z)]$ *is closed with respect to differentiation. If* $\xi_1,\dots,\xi_n$ *are different complex numbers satisfying the condition*

$$f_i(\xi_j)\in\mathbf{K},\qquad 1\leqslant i\leqslant m,\quad 1\leqslant j\leqslant n,$$

then

$$n\leqslant 10\rho D.$$

The choice $f_1(z)=z$, $f_2(z)=\mathrm{e}^z$, $\xi_k=k\alpha$ leads to Lindemann's theorem, and the choice $f_1(z)=\mathrm{e}^z$, $f_2(z)=\mathrm{e}^{bz}$, $\xi_k=k\ln a$ leads to the solution of Hilbert's seventh problem.

In 1970 E. Bombieri transferred Theorem 6 to functions of many complex variables. The points at which such functions take values belonging to a fixed field of a finite degree over $\mathbb{Q}$, are contained in an algebraic manifold whose degree is bounded by some explicit constant.

The most interesting corollaries of Theorem 6 are connected with elliptic functions. In 1937 Schneider proved elliptic analogs of Hermite–Lindemann theorem and Hilbert's seventh problem. One more statement proved by Schneider is related to the values of a modular function $j(\tau)$, $\tau\in\mathbb{C}$, $\Im\tau>0$. He also proved the following theorem.

Theorem 7. *The function* $j(\tau)$ *takes the transcendental values at algebraic points of the upper complex half-plane which are different from the imaginary quadratic numbers.*

T. Schneider

In fact, the only proof of this statement known up to now is related to elliptic functions and Theorem 6. About 50 years ago Schneider posed a problem concerning a purely "modular" proof — that is, a proof without elliptic functions, which employs only modular forms and a modular variable τ. This matter has remained open since that time. In 1995, while trying to find such a proof, the French mathematicians K. Barré, F. Gramain, G. Diaz, and G. Philibert solved another problem, on the values of a modular function, which was posed by K. Mahler (1969) in the complex case and by Yu. Manin (1971) in p-adic case. The corresponding statement is:

Theorem 8. *For any complex number τ with $\Im\tau > 0$, at least one of the numbers $\mathrm{e}^{\pi i \tau}$ and $j(\tau)$ is transcendental.*

The proof of this statement is based on modular arguments and Gel'fond's and Schneider's ideas used in solving Hilbert's seventh problem.

3. Algebraic Independence

It is said that the complex numbers $\omega_1, \dots, \omega_m$, $m \geqslant 1$, are *algebraically dependent* over the field of rational numbers $\mathbb{Q}$, if there is a nontrivial polynomial $P \in \mathbb{Q}[x_1, \dots, x_m]$ such that $P(\omega_1, \dots, \omega_m) = 0$. If there is no such a polynomial, it is said that the numbers $\omega_1, \dots, \omega_m$ are *algebraically independent*. In the case $m = 1$ the terms *algebraic* or *transcendental* numbers are used respectively. For example, the numbers $\sin 1$ and $\cos 1$ are algebraically dependent because $\sin^2 1 + \cos^2 1 - 1 = 0$, but each of them is transcendental. The Lindemann–Weierstrass theorem on the values of the exponential function at algebraic points, formulated above, gives an example of algebraically independent numbers. If $\omega_1, \dots, \omega_m$ are algebraically independent numbers then for each polynomial $P \in \mathbb{Q}[x_1, \dots, x_m]$, $P \neq 0$, the value $P(\omega_1, \dots, \omega_m)$ is transcendental.

Gel'fond was the first to investigate the matter of algebraic independence of the values of the exponential function at points which are not necessarily algebraic numbers. In relation to this topic, he made the following two conjectures.

Conjecture 1. *Let α be an algebraic number different from 0 and 1, and let $\beta_1, \dots, \beta_m$ be algebraic numbers linearly independent with 1 over $\mathbb{Q}$. Then the* *numbers*

$$\alpha^{\beta_1}, \dots, \alpha^{\beta_m}$$

are algebraically independent over $\mathbb{Q}$.

Conjecture 2. *Suppose that* $\alpha_1, \dots, \alpha_m$ *are non-zero algebraic numbers and*

$$\log \alpha_1, \dots, \log \alpha_m \tag{2}$$

are fixed values of their logarithms which are linearly independent over $\mathbb{Q}$. *Then the numbers* (2) *are algebraically independent over* $\mathbb{Q}$.

In the case $m = 1$ the first conjecture coincides with Hilbert's seventh problem, and this is a natural analog of the Lindemann–Weierstrass theorem. The second conjecture generalizes the Hermite–Lindemann theorem. These two conjectures have not been proved for any $m \geqslant 2$.

In 1949 Gel'fond proved the first conjecture in the case $m = 2$, $\beta_1 = \beta$, $\beta_2 = \beta^2$ where β is a cubic irrational number. The best result in this direction belongs to G. Diaz (1989):

Theorem 9. *Let* α *be an algebraic number different from* 0 *and* 1, *and let* β *be an algebraic number for which* $\deg \beta = d \geqslant 2$. *Then*

$$\operatorname{tr}\deg \mathbb{Q}\big(\alpha^{\beta}, \alpha^{\beta^2}, \dots, \alpha^{\beta^{d-1}}\big) \geqslant \left[\frac{d+1}{2}\right].$$

Here $[x]$ denotes the largest integer $\leqslant x$.

The theorem crowns a long chain of results obtained by A. A. Shmelev, R. Tijdeman, D. Brownawell, M. Waldschmidt, S. Lang, G. V. Chudnovskii, E. Reyssat, R. Endell, Yu. V. Nesterenko, and P. Philippon by successive perfection of Gel'fond's ideas.

As for the second conjecture, one should note that there are no results in this direction except to the aforementioned Theorem 3 proved by Baker.

The results on algebraic independence can be transferred to the values of elliptic functions. For example, in 1983 G. Wüstholz and P. Philippon proved an analog of the Lindemann–Weierstrass theorem in this case. There are also results specific to elliptic functions. The following theorem was published by G. Chudnovskii.

Theorem 10. *Let* $\wp(z)$ *be an elliptic Weierstrass function with invariants* g_2, g_3. *Let* ω *be a nonzero period of* $\wp(z)$ *and* η *the corresponding quasi-period. Then there are at least two algebraically independent numbers over* $\mathbb{Q}$ *among the numbers*

$$g_2,\ g_3,\ \frac{\omega}{\pi},\ \frac{\eta}{\pi}.$$

In particular, this theorem says that for algebraic invariants g_2, g_3 the numbers ω/π, η/π are algebraically independent, and if complex multiplication is defined in $\wp(z)$ then the numbers π and ω are algebraically independent as well.

Let γ be a closed path on the Riemannian surface of an elliptic curve

$$y^2 = 4x^3 - g_2x - g_3, \qquad \Delta = g_2^3 - 27g_3^2 \neq 0, \qquad g_2, g_3 \in \overline{\mathbb{Q}}. \tag{3}$$

Then the numbers

$$\omega = \int_\gamma \frac{\mathrm{d}x}{\sqrt{4x^3 - g_2x - g_3}}, \qquad \eta = \int_\gamma \frac{x\,\mathrm{d}x}{\sqrt{4x^3 - g_2x - g_3}} \tag{4}$$

are the period and quasi-period respectively of the corresponding elliptic function $\wp(z)$. According to Theorem 10, for the curve $y^2 = 4x^3 - 4x$ the numbers π and $\Gamma(1/4)$ are algebraically independent. In particular, the number $\Gamma(1/4)$ is transcendental. Similarly, applying Theorem 10 to the curve $y^2 = 4x^3 - 4$ we obtain that π and $\Gamma(1/3)$ are algebraically independent, and $\Gamma(1/3)$ is transcendental.

Eisenstein's series of weight $2k$ $(k \geqslant 1)$ is defined by the sum

$$E_{2k}(\tau) = \frac{1}{2\zeta(2k)} \sum_{m\in\mathbb{Z}} \sum_{\substack{n\in\mathbb{Z} \\ (m,n)\neq(0,0)}} \frac{1}{(m\tau+n)^{2k}}, \quad k \geqslant 1, \quad \tau \in \mathbb{C}, \ \Im\tau > 0.$$

If $k \geqslant 2$ this function is a modular form of weight $2k$ with respect to the group $\mathrm{SL}(2,\mathbb{Z})$. The following statement, proved by Nesterenko in 1996, is a generalization of Theorem 10.

Theorem 11. *Let $\tau \in \mathbb{C}$ with $\Im\tau > 0$. Then there are at least three algebraically independent numbers over $\mathbb{Q}$ among four numbers*

$$\mathrm{e}^{\pi\mathrm{i}\tau}, \ E_2(\tau), \ E_4(\tau), \ E_6(\tau).$$

One of the consequences of this theorem is algebraic independence of the numbers

$$\left\{\pi, \mathrm{e}^{\pi\sqrt{3}}, \Gamma\left(\frac{1}{3}\right)\right\} \quad \text{and} \quad \left\{\pi, \mathrm{e}^{\pi}, \Gamma\left(\frac{1}{4}\right)\right\},$$

and also of the numbers π and $\mathrm{e}^{\pi\sqrt{d}}$ for any natural number d.

Denote $D = \frac{1}{2\pi\mathrm{i}}\frac{\partial}{\partial\tau}$. The next statement (D. Bertrand) also follows from Theorem 11.

Corollary 11.1. *Let $f(\tau)$ be a nonconstant meromorphic modular form with respect to some congruence subgroup $\Gamma \subset \mathrm{SL}_2(\mathbb{Z})$, defined over $\overline{\mathbb{Q}}$. For any number $\alpha \in \mathbb{C}$, $\Im\alpha > 0$, different from the poles of $f(\tau)$, such that $\mathrm{e}^{2\pi \mathrm{i}\alpha} \in \overline{\mathbb{Q}}$, the numbers $f(\alpha)$, $Df(\alpha)$, and $D^2 f(\alpha)$ are algebraically independent over $\mathbb{Q}$.*

Special cases of this statement and some examples were found by Bertrand himself, and also by D. Duverney, Ke. Nishioka, Ku. Nishioka, and I. Shiokawa.

Because $\Delta(\tau) = 1728^{-1}\left(E_4(\tau)^3 - E_6(\tau)^2\right)$ is a modular form of weight 12 with respect to $\mathrm{SL}(2,\mathbb{Z})$, Corollary 11.1 is valid for $\Delta(\tau)$ and Dedekind's η-function

$$\eta(\tau) = \Delta(\tau)^{1/24} = \mathrm{e}^{\pi \mathrm{i}\tau/12} \prod_{n=1}^{\infty} \left(1 - \mathrm{e}^{2\pi \mathrm{i} n\tau}\right).$$

The values of the Rogers–Ramanujian continued fraction

$$RR(\alpha) = 1 + \cfrac{\alpha}{1 + \cfrac{\alpha^2}{1 + \cfrac{\alpha^3}{1 + \ddots}}}$$

can be expressed in terms of the above η-function. So we can state that the number $RR(\alpha)$ is transcendental for any algebraic α, $0 < |\alpha| < 1$.

Another example is related to θ-function

$$\theta_3 = 1 + 2\sum_{n=0}^{\infty} \mathrm{e}^{\pi \mathrm{i} n^2 \tau}.$$

A function $f(\tau) = \theta_3^2$ is a modular form of weight 1 with respect to the congruence subgroup $\Gamma(4)$. Hence the following statement holds:

Corollary 11.2. *For any algebraic number α, $0 < |\alpha| < 1$, the numbers*

$$\sum_{n\geqslant 0} \alpha^{n^2}, \quad \sum_{n\geqslant 1} n^2 \alpha^{n^2}, \quad \sum_{n\geqslant 1} n^4 \alpha^{n^2}$$

are algebraically independent. Specifically, the sum

$$\sum_{n\geqslant 0} \alpha^{n^2} \tag{5}$$

is a transcendental number.

As long ago as 1851, while constructing examples of transcendental numbers, J. Liouville used the series (5) with $\alpha = \ell^{-1}$, $\ell \in \mathbb{Z}$, $\ell > 1$ as an example, for which his method allowed only proof of irrationality.

The following corollary of Theorem 11 generalizes the Mahler–Manin problem.

Corollary 11.3. *For any* $\tau \in \mathbb{C}$ *with* $\Im\tau > 0$ *which is not equivalent to* i *and* $\mathrm{e}^{2\pi\mathrm{i}/3}$ *with respect to* $\mathrm{SL}(2,\mathbb{Z})$*, there are at least three algebraically independent numbers over* $\mathbb{Q}$ *among the numbers*

$$\mathrm{e}^{\pi\mathrm{i}\tau},\ j(\tau),\ Dj(\tau),\ D^2 j(\tau).$$

4. Hypergeometric Functions

There are only a few cases for which it is possible to prove algebraic independence of the values of Gauss' hypergeometric function $F(a,b,c;z)$ and its derivative defined in the unit circle by the series

$$F(a,b,c;z) = \sum_{n=0}^{\infty} \frac{(a)_n (b)_n}{(c)_n n!} z^n. \tag{6}$$

For example, if we apply Theorem 10 to the elliptic curve

$$y^2 = 4x(1-x)(\lambda^{-1}-x), \qquad \lambda \in \overline{\mathbb{Q}}, \quad \lambda \neq 0,1, \tag{7}$$

we obtain that the numbers

$$\frac{1}{\pi}\int_{\gamma} x^{-1/2}(1-x)^{-1/2}(1-\lambda x)^{-1/2}\,\mathrm{d}x, \quad \frac{1}{\pi}\int_{\gamma} x^{1/2}(1-x)^{-1/2}(1-\lambda x)^{-1/2}\,\mathrm{d}x \tag{8}$$

are algebraically independent over $\mathbb{Q}$. Here γ is a closed path on a Riemannian surface of the elliptic curve (7) which is not homologous to zero.

It is well known that one can choose paths of integration γ_1, γ_2 such that

$$\frac{1}{\pi}\int_{\gamma_1} x^{-1/2}(1-x)^{-1/2}(1-\lambda x)^{-1/2}\,\mathrm{d}x = F\left(\frac{1}{2},\frac{1}{2},1;\lambda\right)$$

and

$$\frac{1}{\pi}\int_{\gamma_2} x^{-1/2}(1-x)^{-1/2}(1-\lambda x)^{-1/2}\,\mathrm{d}x = \mathrm{i}F\left(\frac{1}{2},\frac{1}{2},1;1-\lambda\right),$$

where $|\arg\lambda| < \pi$, $|\arg(1-\lambda)| < \pi$. From this follows:

Theorem 12. *For any* $\alpha \in \overline{\mathbb{Q}}$, $\alpha \neq 0,1$ *and for any branch of the hypergeometric function the numbers*

$$F\left(\frac{1}{2},\frac{1}{2},1;\alpha\right), \quad \textit{and} \quad F'\left(\frac{1}{2},\frac{1}{2},1;\alpha\right)$$

are algebraically independent.

The function $F\left(\frac{1}{12},\frac{5}{12},z\right)$ possesses the same property. It is a solution of the differential equation

$$z(1-z)y'' + \left(1-\frac{3}{2}z\right)y' - \frac{5}{144}y = 0, \tag{9}$$

which has another solution $F\left(\frac{1}{12},\frac{5}{12},\frac{1}{2};1-z\right)$. These two solutions can be uniformized by Eisenstein's series.

Theorem 13. *The identities*

$$F\left(\frac{1}{12},\frac{5}{12},1;\frac{1728}{j(\tau)}\right) = E_4(\tau)^{1/4}, \tag{10}$$

$$F\left(\frac{1}{12},\frac{5}{12},\frac{1}{2};\frac{E_6^2}{E_4^3}(\tau)\right) = \frac{\tau+\mathrm{i}}{2\mathrm{i}}\left(\frac{E_4(\tau)}{E_4(i)}\right)^{1/4} \tag{11}$$

hold in the upper complex half-plane $\tau \in \mathbb{C}$, $\Im\tau > 0$.

Here we use the branch $E_4(\tau)^{1/4}$ which is real and positive on the imaginary axis $\tau = \mathrm{i}t$, $t > 0$. For these values of the variable τ the quantities $1728/j(\tau) = \Delta(\tau)/E_4(\tau)^3$ and $E_6^2/E_4^3 = 1 - 1728/j(\tau)$ are real, positive, and less than unity. One should take branches of Gauss' functions (6) at $\tau = \mathrm{i}t$, $t > 0$ in the identities (10) and (11).

The identity (10) and Theorem 11 provide another proof of algebraic independence of the values of the function $F(1/12,5/12,1;z)$ and its derivative at algebraic points α different from 0 and 1.

The identity (11) explains another phenomenon, first noticed by F. Beukers and J. Wolfart.

Theorem 14. *There is an infinite set of algebraic points* α *at which the transcendental function* $F(1/12,5/12,1/2;z)$ *takes algebraic values.*

This statement holds because for any natural n the values $E_4(n\mathrm{i})/E_4(\mathrm{i})$ and $j(n\mathrm{i})$ are algebraic numbers. For example, at $\tau = 2\mathrm{i}$ one obtains from (11)

$$F\left(\frac{1}{12},\frac{5}{12},\frac{1}{2};\frac{1323}{1331}\right) = \frac{3}{4}\sqrt[4]{11}.$$

In a more general situation, uniformization of hypergeometric functions is related to functions which are automorphic with respect to the monodromy group of a corresponding differential equation. In this connection it seems very interesting to investigate transcendence and algebraic independence of the values of automorphic functions.

References and more detailed information concerning the above matters are given in the two books cited below.

Bibliography

[1] N. I. Feldman, Yu. V. Nesterenko. Transcendental numbers. In: *Number Theory IV* (eds. A. N. Parshin and I. R. Shafarevich). Berlin: Springer, 1998 (Encyclopædia Math. Sci., **44**).

[2] *Introduction to Algebraic Independence Theory* (eds. Yu. V. Nesterenko and P. Philippon). Berlin: Springer, 2001 (Lecture Notes in Math., **1752**).

S. M. Nikol'skii

The Great Kolmogorov

Translated by R. Cooke

A remarkable phenomenon in the twentieth century mathematics, in my opinion, is the great Nikolai Andreevich Kolmogorov, one of the greatest mathematicians on a world wide scale.

Much has already been written about the greatness of Kolmogorov, and I myself have written some of it. Several years ago the large book *Kolmogorov in Reminiscences* [1] went out of print. Its authors – mostly his students – are now prominent scholars, each of whom describes this great man from his own perspective. One can only regret that this book was published in a rather small print run, and therefore many libraries do not have it. Among the articles in the book, one substantial systematic survey of the life and creative activity of Kolmogorov stands out. It was written by his student Albert Nikolaevich Shiryaev, a corresponding member of the Russian Academy of Sciences. It would be worthwhile to reprint this survey, together with the bibliographical data, in a larger publication.

Kolmogorov made fundamental contributions to all areas of mathematics and founded entire areas himself, which are now being investigated by many mathematical schools, both in Russia and abroad. His role in probability theory is especially large. Due to Kolmogorov's research, probability theory was turned into an independent mathematical subject.

These *Reminiscences* also contain an article of mine. Here I shall confine myself to some particular remarks on the life and work of Andrei Nikolaevich.

I mention that I was a student of his, and I am apparently the oldest of his students still living. I was a peculiar student, because Andrei Nikolaevich was only two years older than I. How that came about I won't describe here, since I have written about it in the aforementioned article in the *Reminiscences* and in articles in the *Uspekhi Matematicheskikh Nauk* [2], [3].

I have said that Kolmogorov made a contribution to all areas of mathematics. Formally, he has no papers in number theory. But this lack is more than made up by the earliest research of Andrei Nikolaevich, when he was five years old. All by himself he discovered the following remarkable table:

$$\begin{aligned} 1 &= 1^2 \\ 1+3 &= 2^2 \\ 1+3+5 &= 3^2 \\ &\dots \end{aligned}$$

I personally cannot imagine a five-year-old penetrating so deeply into the essence of the natural numbers. But here you see that there was such a child — it was Andrei Kolmogorov.

Andrei Nikolaevich got into mathematics through the theory of functions of a real variable during his undergraduate years. He was a student of Nikolai Nikolaevich Luzin. Luzin always respected and valued him for his great scientific abilities. And if, later on, Luzin was able to dislike Kolmogorov, that was only because Kolmogorov had digressed from the problems of real function theory proper (Luzin's ideas) for the sake of developing other mathematical subjects that Luzin regarded as inferior.

Andrei Nikolaevich did not obey Nikolai Nikolaevich, although he respected him as a scholar and was grateful to him as a student is grateful to a teacher, to the end of his life. Even when he was young, Andrei Nikolaevich felt that mathematics is much more than merely the ideas of Luzin, even though these ideas were quite important. Those who wish might develop only them, and Andrei Nikolaevich was willing to help with the strength of his powerful mind.

Nevertheless, Andrei Nikolaevich could not help returning to the problems of real function theory from time to time. The main problem that Kolmogorov solved while he was in the school of Luzin was to construct an example of a Lebesgue-integrable periodic function whose Fourier series diverges at every point. In 1922, when he was still a student, Kolmogorov had found a function and proved divergence of its Fourier series almost everywhere [4]. Then, in 1926 (as a graduate student), he modified his reasoning, obtaining a Lebesgue-integrable function whose Fourier series diverges everywhere [5].

With this result Kolmogorov had obtained a negative answer to a question posed by Luzin. Nina Karlovna Bari, a famous member of Luzin's school, notes in her treatise that "all of us (in Luzin's school) were impressed by this result of Kolmogorov." Later I happened to be at a meeting at Moscow University in honor of Andrei Nikolaevich's fiftieth birthday. During this meeting, there were storms of applause for the honoree, who by that time had made fundamental contributions to many areas of mathematics. In his closing speech Andrei Nikolaevich stressed that he regarded his result on the divergent Fourier series as the most difficult result he had achieved during thirty years, the one that demanded the greatest mental concentration and the creation of a complicated mathematical construction. I would point out that in our time S. V. Bochkarev has obtained a further essential development of this Kolmogorov's result by solving the corresponding problem for general orthogonal series.

However, only a few years after his fiftieth birthday, Andrei Nikolaevich obtained another result [6] in the theory of functions of a real variable, which he called his most difficult result in the sense of the technical difficulties to be overcome. Here is that result. Every function f of n variables that is continuous on the cube $[0,1]^n$ can be represented in the form

$$f(x_1,\dots,x_n) = \sum_{k=0}^{2n+1} X_k\left(\sum_{l=0}^{n} \varphi_{kl}(x_l)\right),$$

where X_k and φ_{kl} are continuous functions of one variable. Moreover, the functions φ_{kl} are given in advance and are independent of f.

This is indeed a very difficult and unexpected result, giving rise to a new picture of the possibilities of continuous functions of several variables. It turns out that computing any continuous function of several variables can be reduced to computing functions of one variable interconnected by function compositions.

We note that this result was preceded by certain other results, obtained by Kolmogorov and his student V. I. Arnold, which were essential at a certain stage.

* * *

Several years ago I learned that in 1991 the *Proceedings of the Royal Society* (London) published a special collection of articles under the title *Kolmogorov's Ideas 50 Years On* [7]. The title referred to the ideas of Kolmogorov on turbulence in fluids and gases, which he published in the *Doklady Akademii Nauk SSSR* in 1941 [8]–[10]. It was pointed out in the very first article in this collection (I am writing from memory) that Kolmogorov's results obtained in the articles mentioned above are among the most important ones in the modern theory of turbulence.

There recently appeared a book with the title *Turbulence: The Legacy of A. N. Kolmogorov* [11] by a prominent specialist in turbulence, U. Frisch, who is a corresponding member of the French Academy of Sciences. This book is a high-level textbook for both students — of mathematics, physics, astrophysics, and geophysics — and professionals — scholars and engineers.

Kolmogorov figures in these books on the one hand as a mathematician who preferred the probability-theoretic approach to the difficult problems of turbulence, and on the other hand as a physicist, since this approach is based on certain physical assumptions formulated by Kolmogorov.

It is known that this theory encountered criticism in its time from the famous theoretical physicist Lev Davidovich Landau. But, after the lapse of half a century, here is what Frisch has to say on that score: "The central figure in this book on turbulence is the great Russian scholar Andrei Nikolaevich Kolmogorov." Thus time has been on the side of Kolmogorov.

* * *

Frisch calls the period 1941–1942 the "Kazan' period" of Kolmogorov's activity in the theory of turbulence. Let me relate a few facts about this period. Andrei Nikolaevich was indeed associated with Kazan' from the the second half of 1941, when the Mathematical Institute of the USSR Academy of Sciences, where he worked, was in wartime evacuation. At that time Kolmogorov had an administrative position as Secretary of the Physics and Mathematics Division of the USSR Academy of Sciences. In that capacity he made the rounds of all the institutes of the Division — mathematical and physical. During this time Kolmogorov, travelling frequently between Moscow and Kazan', still managed to give lectures in the non-evacuated branch of Moscow University. But even then he found time for research, which, as we see, was quite productive. In addition he had students. Thus in 1941 he directed the doctoral dissertations of three students: A. I. Mal'tsev in algebra and logic, A. M. Obukhov in turbulence, and me in approximation theory.

Anatolii Ivanovich Mal'tsev and I may not have burdened him excessively at the time. Our topics had been formed earlier, and all Andrei Nikolaevich had to do was to check over our results — it should be said, however, that he did this with careful attention and interest. It was different with Obukhov. He was working on a topic that Kolmogorov himself was working on intensively at the time, and he later became Andrei Nikolaevich's main assistant on turbulence in meteorology. I recall Obukhov at the beginning of the "Kazan' period" (autumn 1941), when he lived in a dormitory room at Kazan' University in which there was only one bed. There was a blackboard on the wall, on which for some reason he carried out intensive calculations at night. Anatolii Ivanovich Mal'tsev and I also lived at close quarters in a small room, which we furnished from the shelves of Kazan' University.

Incidentally, Andrei Nikolaevich Kolmogorov made his discoveries during the day and slept at night.

* * *

I would like to call attention to another fact. Soon after the war began, in August of 1941, I was sent out of Moscow to the west of Maloyaroslavets to dig tank traps — a structure extending for hundreds of kilometers from north to south. Just before leaving, not knowing what fate was in store for me, I decided to submit some material on my future doctoral dissertation to Andrei Nikolaevich through the secretary of the Institute. But in a twist of fate the Germans breached our tank traps in early October, work on them was halted, and I wound up in Moscow, where I found some members of the Mathematical Institute who had not yet managed to get themselves evacuated. Kolmogorov was also in Moscow at this time. But panic broke out on 16 October: it was believed that the Germans were already invading the city. Even so, I went to the Institute that day and learned that Andrei Nikolaevich and our director, Academician Sobolev, had left Moscow hurriedly the evening before. They had received a telephone call on the evening of 15 October informing them that they must go immediately to Pavelets Station and bring only carry-on luggage — a special train was waiting to take them from Moscow to Kazan'. In late October Anatolii Ivanovich and I also arrived in Kazan'.

At our first meeting in Kazan', Andrei Nikolaevich invited me to tea at his home in the evening. He was living in a crowded location with his friend Pavel Sergeevich Aleksandrov, his aunt Vera Yakovlevna and Pavel Sergeevich's sister, Varvara Sergeevna. A corner of the room had been marked off for research by two bookshelves. It was in this nook behind the bookshelves that those discoveries were born that have now been immortalized in special collections of foreign academies.

After tea Andrei Nikolaevich went behind the bookshelf and returned with — what do you know! — my manuscript. He said that the material in this manuscript was completely sufficient for a doctoral dissertation and advised me to break off my doctoral studies (which were intended to last another two years) and defend the dissertation — it was wartime and it would not be reasonable to draw out this affair. As it happened, Andrei Nikolaevich said approximately the same to Anatolii Ivanovich. Two months later we defended our doctoral dissertations.

But here is the remarkable thing in all this. On that tragic evening of 15 October, hastily leaving his house in Moscow, Andrei Nikolaevich had put my manuscript into his suitcase. He could have put an extra pair of trousers there — many would have done so. This is only one example of the deep humanitarian concern that Andrei Nikolaevich exhibited toward his students. All of us, and we are many, keep in our hearts the memory of our Teacher, and will do so to the end of our lives.

* * *

In Kazan' I was mostly occupied with my mathematical concerns. But Andrei Nikolaevich nevertheless occasionally entrusted me with approximate investigations of certain differential equations which I understood were of an exploratory nature. It seems to me that, in any case, during his "Kazan' period" Andrei Nikolaevich was trying very hard to obtain a theory of turbulence having, to the extent possible, a purely mathematical character, free of special physical assumptions. But it didn't work out, and those physical assumptions turned out to be fundamental and they are still important today.

Andrei Nikolaevich studied carefully the numerical experimental results available in the literature, including those of Prandtl. He himself did not set up any physical experiments, in fact, that was impossible at the time. But later on he spent an extended period sailing the oceans on a special research ship and oversaw directly the experiments of young scholars on turbulence in the oceans. [1]

* * *

After the war, starting in 1947 I lectured in the Moscow Institute of Physics and Technology alongside Academician Landau. Thus it happened that for nearly two years we shared a car ride from Dolgoprudnyi into Moscow. Lev Davidovich sat in the back seat and I alongside him. He was a very pleasant and engaging conversationalist. We often spoke about our teaching duties. In particular, Lev Davidovich stressed his firm conviction that the subject of "Mechanics" in physics departments should be a part of "Physics"; that is, lectures on mechanics for physicists should be given by physicists. He had no such words for mathematics, although in a number of cases he criticized the way mathematicians lecture on their subject to physicists. But there was an exception — probability theory. Lev Davidovich believed that mathematicians should not lecture on probability theory to physicists — they always lecture to physicists in the wrong way; physicists themselves should lecture on the probability theory that they need.

[1] Reminiscences and documents on these expeditions of Andrei Nikolaevich have recently been published in *Extraordinary Phenomenon: A Book About Kolmogorov* [12]. — *Eds.*

Lev Davidovich also talked about Kolmogorov. He expressed great respect for the mathematical work of Kolmogorov, including his work in probability. However, he added that Kolmogorov had no business meddling in physics with his probability theory.

But life goes on and introduces its own corrections.

* * *

For a long time International Congresses of Mathematicians have been taking place in the even years not divisible by 4. There was an interruption during World War II, and the first post-war congress took place in 1950. Soviet mathematicians did not participate in it (Stalin wouldn't allow it). But at the next congress in 1954 in Amsterdam a small delegation from the Soviet Union nevertheless appeared. It consisted of only three mathematicians: P. S. Aleksandrov, A. N. Kolmogorov, and I. Pavel Sergeevich and I gave sectional addresses, and Andrei Nikolaevich a plenary address. There were only two plenary addresses at this congress: John von Neumann spoke at the opening of the congress and Kolmogorov at the closing session.

Kolmogorov spoke on the theory of dynamical systems. In the main he presented his own results on these questions, which were published in two notes ([13] and [14]) in the *Doklady Akademii Nauk SSSR* during 1953 and 1954, and were to become famous. They assert that the majority of quasi-periodic motions of Hamiltonian systems are stable. In his talk Kolmogorov noted that this research had arisen mainly due to the influence of the classical results of N. N. Bogolyubov and N. M. Krylov (1937).

This result of Kolmogorov was to be developed in his own work and in the work of V. I. Arnold and Jürgen Moser. The totality of this research has now been given the eccentric name KAM (Kolmogorov–Arnold–Moser).

I note that the research of Bogolyubov and Krylov just mentioned is also being continued along these lines in work headed by Yu. A. Mitropol'skii and A. M. Samoilenko.

At the congress I had the opportunity to notice the immense respect for Andrei Nikolaevich and Pavel Sergeevich shown by the organizing committee of the congress and, in general, from leading mathematicians of the world. For the most part, this was a renewal of contacts established during the 1920s and early 1930s, when the two of them were abroad for an extended period.

We traveled to Amsterdam by train, with long stops at a time when our country had not yet had time to heal its wounds after the war. In Brest the stop lasted so long that we had time for a swim in the Bug river and even to go for a boat ride.

Whenever they found themselves near water, Andrei Nikolaevich and Pavel Sergeevich could not resist taking a dip. The temperature of the water made no difference to them, as long as it wasn't frozen. Of course, to everything there is a season. It was a time of robust good health and vigorous creative activity.

* * *

From the *Reminiscences* [1] I learned details about Kolmogorov's early childhood, some of which I hadn't known at all and others I had only guessed.

The earliest childhood of Andrei Nikolaevich was, of course, tragic: he lost his mother immediately after being born, and his father died young somewhere far away. His compensation was the love of his grandmother and grandfather, and the absolutely devoted and selfless care he received from two aunts, who were quite young girls. One of them, Vera Yakovlevna, adopted the young Andrei and devoted her life to him; she had no family of her own. His childhood was also brightened by living in the rich noble house of his grandfather; a landowner, a nobleman, and Marshal of the Nobility for the Uglich region of Yaroslavl' Province.

From Andrei Nikolaevich himself I learned the following facts about his early childhood.

• On holidays a district police officer would arrive at the kitchen, evidently the head of the local police. He would come to give his good wishes to Kolmogorov's grandfather. The servants would notify the grandfather, who would then take out a 25-ruble note and ask them to give it to the officer. The officer would take the 25-ruble note from the maid, ask her to convey his thanks to the grandfather, and leave.

• Gendarmes burst into the house and carried out a search that was especially diligent in the room of the aunts where little Andrei was lying in his cradle. They didn't find anything, but they forgot to look in the cradle underneath the bottom of the future great mathematician. Forbidden literature was hidden there.

• When he was 80 years old, Andrei Nikolaevich asked me, "Have you ever ridden a horse?"

"Yes," I answered, "but not since I was nine years old." My father (a woodsman) had a pair of special horses. When they needed to drink, they would set me on them, and I would ride to the river.

"And you, Andrei Nikolaevich," I asked, "have you ever ridden a horse?"

"My grandfather bought me a pony," was the answer. (I didn't ask whether one pony or two.[2])

---

[2] The word *poni* is indeclinable; and since Russian has no articles, Kolmogorov's reply could be interpreted as "My grandfather bought me ponies." — *Transl.*

But still, some questions remained.

In the *Reminiscences* you can read that aunt Vera Yakovlevna adopted little Andrei and he received the family name of his adopted mother and grandfather — Kolmogorov.

Evidently, this was before the Revolution. So, did Andrei also receive noble status? Please keep in mind that this was not merely a provincial title, but a title of ancient noble lineage. It would have been natural in such a case for the grandfather to enroll young Andrei in a "suitable" school, perhaps even in the Page Corps or the Lyceum. But instead Andrei was sent to the private gymnasium of Madame Repman, which was the cheapest gymnasium in Moscow — despite such wealth.

In general we know hardly anything about the grandfather. When did he die? Before the revolution or after? If after, under what circumstances?

However, I have excerpted from the *Reminiscences* the following words of Andrei Nikolaevich: "If there existed a better world, in which people were reunited with the dead for eternal life, of course I would most of all like to meet my grandfather and grandmother who raised me, whose love and kindness have been more than sufficient for my entire life." "And, of course, I would like to make an appointment with by beloved teacher" (but that is another topic — Kolmogorov and education — see below).

* * *

The great mathematicians Euler, Chebyshev, and Lobachevskii have had, and continue to have, professional biographers. It is time that Kolmogorov acquired his own professional biographers. They will have to dig into the archives, for Andrei Nikolaevich was also a man of affairs — he conducted secret research, gave important consultations, carried out government assignments, and held government office. For example, the period when he was Secretary of the Physics and Mathematics Division of the Academy deserves more detailed study.

Kolmogorov was elected an Academician in 1939. He was never a corresponding member, despite having long since earned that title. But here is the curious thing: immediately after he was made a member of the Academy, he was elected Secretary of the Physics and Mathematics Division, even though there were deserving older academicians in the Division. These matters were not debated during Soviet times; they were preliminarily discussed by the state authorities. It looks very much as if Stalin himself took part in this. Thus, by digging into documents one might find something for History. However, these documents may all have been shredded long ago. This could have happened, and it would not contradict Kolmogorov's own surmises on that question.

At some point (it seems as if it was in 1943) Andrei Nikolaevich said, in my presence, "After twenty years no one will know what actually happened in our country." I, of course, tried to argue the point, but Andrei Nikolaevich said to me, "You are not writing your memoirs."

* * *

Education and popular mathematics always occupied a prominent place in the life of Kolmogorov.

When Andrei was very young, it would have been natural for his grandfather to hire a French or German tutor for his grandson. But it didn't happen that way. On their own initiative the young aunts took over the education of little Andrei, guided, however, by the very latest pedagogical achievements. The education was democratic — children of neighbors were taught at the same time. It is certain that the grandfather participated in this process. One way or another, he was an honorary patron of the schools and all seems to have turned out well.

The gymnasium of Madame Repman also turned out well — better than the state gymnasia, where the instruction was at a measured pace, but very dry. Here there was a wide range of general studies in a living liberal context, and as it happens, there was an opportunity to create the conditions for the accelerated advancement of Andrei Kolmogorov in mathematics.

The Revolution broke out during Kolmogorov's last years in the gymnasium. We do not know specifically what happened to the Kolmogorov family at that time.

In his student years (1922–1925) young Kolmogorov earned his living (and most likely, not only his own living) by teaching mathematics and physics in school (in Potylikha). It was, of course, easy for him to teach in the sense of knowledge, but he was also interested in pedagogy. The boys and girls first shied away from him, but nevertheless elected him their class advisor. In the end, they visited the Crimea and other places, led by Kolmogorov.

Note especially that all this "pedagogical" activity of Kolmogorov occurred just when he had obtained his famous result on a divergent Fourier series. This burst of pedagogical activity occurred simultaneously with a powerful burst in science. Or is it perhaps that in the Crimea he finally realized why the Fourier series of an integrable function may diverge almost everywhere?

The span of more than 40 years, from the 1920s to the late 1960s, was a period of vigorous research by Kolmogorov. But in the 1930s he managed to write (jointly with Pavel Sergeevich Aleksandrov and by request of *Narkompros*[3]) the textbook *Algebra*. It would be a good idea to reprint this book.

[3] People's Commissariat for Education. — *Transl.*

In addition, in the 1930s (and also at the end of his life) besides his scientific discoveries, Andrei Nikolaevich was seriously engaged in the organization of the *Large Soviet Encyclopedia* as the editor-in-chief for mathematics. He wrote fundamental articles for the *Encyclopedia of Mathematics* — "Mathematical statistics" and so forth.

The majority of his articles have been automatically reprinted up to now, not only in new editions of the *Large Soviet Encyclopedia* but also in other encyclopedias (mathematical, educational, and so forth).

The 1950s were also years of great scientific triumphs for him. But in addition to this, Kolmogorov — the dean of Mechanics and Mathematics at Moscow University — made crucial changes in the curricula, which became firmly established, not only at Moscow University but in all the universities of the Soviet Union. Kolmogorov wrote (jointly with Fomin) his famous textbooks on analysis, which became universally used in Soviet universities, and also in many foreign universities.

Thus we see that Andrei Nikolaevich has great achievements in the cause of higher education of mathematicians, which fortunately were fully recognized during his lifetime. One could list these achievements in detail — one could tell how he made a point of introducing statistics, logic, information theory and so forth into the education of students in the Department of Mechanics and Mathematics; one could talk about the curriculum changes that he proposed for this department and its counterparts at other Soviet universities; one could talk about the measures he proposed for recruiting not only senior students but also first- and second-year students into science, and so forth.

After the mid-1960s Andrei Nikolaevich shifted the center of gravity of his activity into high school education. He organized an All-Union Physico-Mathematical School for gifted children at Moscow University. He was in this school for an extended period almost daily, having turned into a village teacher, educator, and field-trip leader. Among the former pupils of that school are thousands holding the degree of *kandidat* and dozens holding doctorates in the physico-mathematical sciences. At this point, one can only encourage School No. 18 in Moscow, which bears the name of Kolmogorov, to uphold the standards established during his lifetime. It will not be easy without Kolmogorov.

Yet another great contribution of Kolmogorov to school education is provided by his numerous articles and popular booklets, written especially for secondary and high school students, teachers, and all who desire to elevate their general physico-mathematical education or obtain some necessary information.

More than 300 such articles and booklets are listed in the *Reminiscences* [1]. In each of these articles you will find profound and original thoughts. One cannot help thinking that it would be very worthwhile to gather these articles and booklets into a single collection and publish them.

At the end of his life Andrei Nikolaevich was working for school education as the chairman of the commission on the reform of school teaching; first a commission of the USSR Academy of Sciences, and later of the USSR Ministry of Education. Here, unfortunately, he did not succeed in implementing his ideas fully. The textbooks developed as the foundation of the reform were unsuccessful, and in the end were replaced by new ones, but now under different leadership.

It was Andrei Nikolaevich's misfortune that the defects just mentioned were blamed on him at the highest levels of authority. But this was completely unjust. The major links in the chain of reform at the time were actually beyond the influence of Kolmogorov. Take, for example, the algebra textbook for years 6–8. It encountered what may have been the most severe attacks, and they were justified. In these textbooks, the fascination with set theory is carried to ridiculous extremes. Such a simple concept as a function received a definition that was absolutely beyond the grasp of students, and even teachers. But the principal author of these textbooks was a respected professor of mathematics at Moscow University, who occupied in addition a very high rung on the ladder of the educational bureaucracy. He, of course, was completely independent of Kolmogorov. It may be that Andrei Nikolaevich, as chairman of the commission, was forced to endorse this textbook. But that, of course, was a mere formality, without substance.

The time had passed when Kolmogorov could read 35 books by Kiselev in two days. (And, I claim, he really did read them, from the first page to the last.) Parkinson's disease and glaucoma were now beginning to take their toll on Kolmogorov. And that, of course, was preceded by a long period of physical decline. In that state, a person will not read anyone else's textbook.

I note that it was under Kolmogorov's influence that the elements of mathematical analysis were definitively introduced into high-school mathematics. No one has yet encroached upon this innovation by Kolmogorov. Along with his younger colleagues (B. M. Ivlev, A. M. Abramov, and others) Kolmogorov wrote a textbook for years 9–10 on a new subject in secondary-school mathematics, which came to be called "algebra and the elements of analysis." This textbook, so far as I know, was not attacked. It could not have been attacked, because it was written by knowledgeable mathematicians, good school-level pedagogues without any ambitions.

I would like once more to call attention to the legacy that Kolmogorov left to school students, teachers, and lovers of mathematics in general. They are simple, profound judgments that expand a person's vision and lead to true science. Our task is to promote the popularization of this legacy.

The matter is simpler in regard to Kolmogorov's own scientific achievements — schools of mathematics preserve, and are often based on, the discoveries of Kolmogorov. In this sense he will occupy a leading position in mathematics for many years to come — like our great mathematicians Euler, Chebyshev, and Lobachevskii. And, most likely, in no way inferior to them.

Bibliography

[1] *Kolmogorov in Reminiscences* (ed. A. N. Shiryaev). Moscow: Fizmatlit, 1993 (Russian).

[2] S. M. Nikol'skii. Fragments of reminiscences about A. I. Mal'tsev. *Uspekhi Matem. Nauk*, 1972, **27**(4), 223–230 (Russian).

[3] S. M. Nikol'skii. P. S. Aleksandrov and A. N. Kolmogorov in Dnepropetrovsk. *Uspekhi Matem. Nauk*, 1983, **38**(4), 37–49 (Russian).

[4] A. N. Kolmogorov. Une série de Fourier–Lebesgue divergente presque partout. *Fund. Math.*, 1923, **4**, 324–328.

[5] A. N. Kolmogorov, G. A. Seliverstov. Sur la convergence des séries de Fourier. *Atti Accad. Naz. Lincei Rend.*, 1926, **3**, 307–310.

[6] A. N. Kolmogorov. On the representation of continuous functions of many variables by superposition of continuous functions of one variable and addition. *Doklady Akad. Nauk SSSR*, 1957, **114**, 953–956 (Russian).

[7] *Turbulence and Stochastic Processes: Kolmogorov's Ideas 50 Years On* (eds. J. C. R. Hunt, O. M. Phillips and D. Williams). *Proc. Roy. Soc. London*, Ser. A, 1991, **434**, 240 pp.

[8] A. N. Kolmogorov. The local structure of turbulence in an incompressible viscous fluid at very high Reynolds numbers. *Doklady Akad. Nauk SSSR*, 1941, **30**, 299–303 (Russian).

[9] A. N. Kolmogorov. On the degeneracy of isotropic turbulence in an incompressible viscous fluid. *Doklady Akad. Nauk SSSR*, 1941, **31**, 538–541 (Russian).

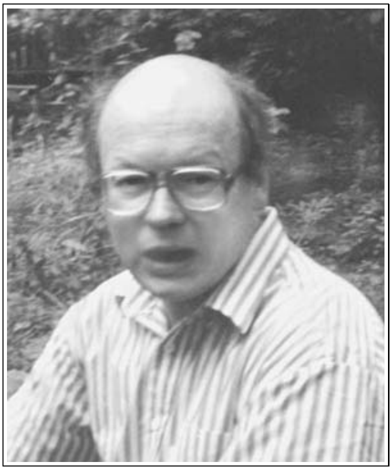
[10] A. N. Kolmogorov. Energy dispersion in locally isotropic turbulence. *Doklady Akad. Nauk SSSR*, 1941, **32**(1), 19–21 (Russian).

[11] U. Frisch. *Turbulence: The Legacy of A. N. Kolmogorov*. Cambridge: Cambridge University Press, 1995.

[12] *Extraordinary Phenomenon: A Book About Kolmogorov* (ed. V. M. Tikhomirov). Moscow: PHASIS ∗ MIROS (Moscow Institute for Development of Education Systems), 1999 (Russian).

[13] A. N. Kolmogorov. On dynamical systems with an integral invariant on the torus. *Doklady Akad. Nauk SSSR*, 1953, **93**, 763–766 (Russian).

[14] A. N. Kolmogorov. On the conservation of quasi-periodic motions under a small variation in the Hamiltonian. *Doklady Akad. Nauk SSSR*, 1954, **98**(4), 527–530 (Russian).



A. N. Parshin

Numbers as Functions: The Development of an Idea in the Moscow School of Algebraic Geometry

Translated by R. Cooke

Where numbers come from nobody knows. Ethnographers have traveled through all the countries of the world, up and down, backward and forward, and have found people for whom the concepts “one,” “two,” and “many” are sufficient. And yet, these people have refined arts, subtle mythology, and nontrivial crafts. They are people quite as much as we are, but without that “one, two, three,” and so on to infinity. Prometheus did not bring them “the science of number, the most important of all sciences.”

However, there is no need to travel the world to see the sharp difference between the first numbers and those that followed. Language has retained enough evidence of that. Thus, in many linguistic families the etymology and grammatical forms of the first three or four cardinal numbers are fundamentally different from those of all other numbers. Moreover, among all the peoples of the world the “first” numbers are burdened with a rich symbolism and have their own individual character. In the sterile series of natural numbers all this disappears completely.

That there are infinitely many numbers seems to have been recognized for the first time in ancient Greece. Euclid’s *Elements* even contain a proof that the series of prime numbers is infinite. Here “infinite” is understood as a potential infinity, a non-finiteness. To the modern person the origin of numbers is completely comprehensible — they arose from counting “things” or “objects” (but where does counting come from?). It also seems obvious to modern people that, once having begun to count, they are unable to stop. To imagine a finite closed universe of numbers in *real* life is, after all, not easy. Still, in mathematics there are, for example, finite fields.

Whatever the situation with numbers, they are originally a discrete, countable object. All those irrationalities and continua that the Greeks struggled with arose later in history. But the concept of a function seems to have arisen as the incarnation of something continuous, the trajectory of a stone that is thrown, or a line drawn in the sand with a finger. Functions are connected with motion.

However, the subsequent evolution of algebra and many functions turned into something discrete, amenable to algorithmization by means of some sort of Maple V or PARI. Many have speculated on the relation of the discrete and the continuous in mathematics. Hermann Weyl wrote about these two modes of understanding. And André Weil related the following “argument” between Claude Chevalley and Oscar Zariski. What is a curve? They went to the board and wrote the following:

Chevalley $f(x,y) = 0$

Zariski

We can see both answers. Both are generated by a sweep of the hand, but the formula can also be read aloud. Thus we are faced with the ancient quarrel between the ear and the eye, the world of language and the world of vision.[1]

The analogy between numbers and functions that forms the subject of this article is an even greater leap ... In this article the continuous and the discrete enter on both sides — into numbers and into functions. Algebra and analysis work hand in hand here.

The basis of our discussion is a lecture delivered by the author at the conference “Matériaux pour l’histoire des mathématiques au XX-ième siècle,” which took place at L’Université de Nice Sophia-Antipolis in Nice in January 1996. My task was to describe one area in the development of arithmetical algebraic geometry in Moscow during the 1950s and 1960s. I made no attempt to present

[1] The current generation adds here the computer keyboard and the mouse.

any complete historical study of the development of algebraic geometry during this "golden age of the Moscow mathematical school."

We shall begin by explaining the meaning of the analogy between numbers and functions, starting with the simplest concepts. In the second part we study a nontrivial example: the explicit formula for the law of reciprocity. In the third part we shall become acquainted with certain aspects of the "social" life of the Moscow school, in particular, with certain seminars, lectures, and books. In the final part we shall examine another example of this analogy: arithmetical surfaces, an example that is indisputably the summit of this area. As for the time frame, I shall hardly pass beyond the early 1970s.

The fact is that the smooth development of this idea — the analogy between numbers and functions — which began in the last third of the nineteenth century, underwent a sharp jump in the 1960s. It was recognized that the preceding development had occurred in the framework of a one-dimensional world. It became clear that one could and should pass to other dimensions. How exactly this jump took place we wish to relate here.

The interested reader may consult [1], [54], [45], and [46] to get acquainted with the subsequent development of these ideas, which tended to be broad rather than deep. These same articles contain some results that we have omitted.

1. The Analogy

To understand the origin of the analogy between numbers and functions, let us look at the following table:

$$\begin{array}{lll}
f \in \mathbb{Z} \subset \mathbb{Q} & F \in \mathbb{F}_p[t] \subset \mathbb{F}_p(t) & F \in \mathbb{C}[t] \subset \mathbb{C}(t) \\
f = (\pm) p_1^{\nu_1} \cdots p_n^{\nu_n} & F = a P_1^{\nu_1} \cdots P_n^{\nu_n} & F = a(t-t_1)^{\nu_1} \cdots (t-t_n)^{\nu_n} \\
f \neq 0 & F \neq 0, \quad a \in \mathbb{F}_p^* & F \neq 0, \quad a \in \mathbb{C}^*
\end{array}$$

Here we are comparing the ring $\mathbb{Z}$ of integers and the rings of polynomials $\mathbb{F}_p[t]$ and $\mathbb{C}[t]$ in one variable t (with coefficients, of course, in the finite field $\mathbb{F}_p$ consisting of p elements and the field $\mathbb{C}$ of complex numbers).[2] The nonzero elements of these rings (the numbers f and functions F) can be expanded as the product of prime numbers p and irreducible polynomials respectively. Over the field $\mathbb{F}_p$ the latter correspond to conjugate elements of the algebraic closure of

[2] Here and below k^* is the set of nonzero elements of the field k, that is, its multiplicative group.

the field $\mathbb{F}_p$. Over the field $\mathbb{C}$ they are linear polynomials $t - t_0$, where t_0 is an arbitrary point on the complex line $\mathbb{C}$.

The integers ν_k that occur in the given expansion have the following fundamental property: they are *valuations*[3] in the sense that

- $\nu(fg) = \nu(f) + \nu(g)$,
- $\nu(f+g) \geqslant \min\big(\nu(f), \nu(g)\big)$.

For our rings the valuations ν assume nonnegative values, and they can be uniquely extended to the quotient fields (the field $\mathbb{Q}$ of rational numbers and the fields $\mathbb{F}_p(t)$ and $\mathbb{C}(t)$ of rational functions) as homomorphisms onto the group $\mathbb{Z}$. Accordingly, in these fields we have expansions of their elements into products that generalize the expansions considered above. This is the first observation showing that number rings and rings of functions have certain properties in common.

We now call attention to the fact that in the case of $\mathbb{C}$ the set of valuations coincides completely with the set of points of the complex affine line. The same is true for the affine line over the finite field $\mathbb{F}_p$ if such points are taken as the maximal ideals of the ring $\mathbb{F}_p[t]$. Each such ideal is a principal ideal, that is, consists of the multiples of some irreducible polynomial P. In this case, the base field is not algebraically closed, and the correct definition of the points differs from the rectilinear definition: a point is characterized by its coordinate, that is, an element of the algebraic closure of the field $\mathbb{F}_p$.

We may also attempt to use our geometric intuition and introduce a geometric object in the case of the ring $\mathbb{Z}$. We denote it by $\mathrm{Spec}(\mathbb{Z})$, and we shall at first regard it as the set of prime numbers $p = 2, 3, 5, \dots$ or prime ideals $(p) \subset \mathbb{Z}$. In this way our table expands to the following:

ν_p	ν_P	ν_P
$p \in \mathrm{Spec}(\mathbb{Z})$	$P \in \mathrm{Spec}(\mathbb{F}_p[t])$	$P = (t - t_0) \in \mathrm{Spec}\,\mathbb{C}[t])$
	affine line over $\mathbb{F}_p$	affine line over $\mathbb{C}$

[3] Such valuations are usually called *non-Archimedean*. Corresponding to them are the multiplicative (non-Archimedean) *norms* $|f| = p^{-\nu(f)}$ for which $|fg| = |f| \cdot |g|$ and $|f+g| \leqslant \max\big(|f|, |g|\big)$. If this last condition is weakened to $|f+g| \leqslant |f| + |g|$, we obtain the well-known definition of a norm.

For any point P on the affine line over $\mathbb{C}$ we have the power-series expansion of a rational function F:

$$F = \sum_{i_0}^{\infty} a_i (t - t_0)^i, \quad \text{where} \quad i_0 = \nu_P(F), \quad a_i \in \mathbb{C}.$$

There is also an expansion of this type in the case of the line over $\mathbb{F}_p$. The analogous construction for the field $\mathbb{Q}$ is the p-adic representation of rational numbers:

$$f = \sum_{i_0}^{\infty} a_i p^i, \quad \text{where} \quad i_0 = \nu_p(f) \text{ and } a_i \in 0, 1, \ldots, p-1.$$

Both expansions are connected with field embeddings: $\mathbb{Q}$ into the field $\mathbb{Q}_p$ of p-adic numbers, and $\mathbb{F}_p(t)$ and $\mathbb{C}(t)$ into the fields of power series $\mathbb{F}_p\big((t)\big)$ and $\mathbb{C}\big((t)\big)$. These embeddings are the completions of the fields relative to the metrics defined by the valuations (see [5], [10]):

$$\rho(x, y) = p^{-\nu(x-y)}$$

in the case of the fields $\mathbb{Q}_p$ or $\mathbb{F}_p\big((t)\big)$, and

$$\rho(x, y) = c^{-\nu(x-y)}$$

in the case of $\mathbb{C}\big((t)\big)$. (Here $c \neq 0$ is an arbitrary fixed constant.)

As numerical variants of power series the p-adic numbers were introduced by Kurt Hensel [21]. The analogy between power series and the expansions of rational numbers in powers of p (for $p = 10$) had been considered earlier by Isaac Newton [34].

A more profound manifestation of this analogy is the global property of valuations known in number theory as the *product formula*. To obtain it we must enlarge our objects in order to make them "compact" or "complete." In the case of the affine line it is necessary to embed it in the projective line $\mathbb{P}^1$ by adding a point at "infinity." It corresponds to the valuation

$$\nu_\infty(f) = \deg(f).$$

The point at "infinity" has no meaning as an ideal of the ring of polynomials in t. (The set of such ideals is exhausted by the points of the affine line.) However, our projective line $\mathbb{P}^1$ contains another affine line (the complement of the point $t = 0$), which corresponds to the subring $\mathbb{F}_p[t^{-1}]$ or $\mathbb{C}[t^{-1}]$ of the field of rational functions. And the "infinite" point corresponds to the ideal (t^{-1}) of this ring. Thus, in the case of a field of functions all points are arranged "in the same way": they correspond to *non-Archimedean* valuations of the field of rational functions, and in this way we obtain *all* valuations of the field.

In the case of the field $\mathbb{Q}$ our geometric object $\mathrm{Spec}(\mathbb{Z})$ is also not "compact." The prime numbers correspond to all non-Archimedean valuations of the field $\mathbb{Q}$. But there is also an Archimedean valuation: [4]

$$\nu_\infty(f) = -\log|f|, \quad f \in \mathbb{Q}^*,$$

and by a theorem of Ostrovskii we now have all valuations of the field $\mathbb{Q}$. The fundamental difference with the geometric case is that in the numerical situation the point at "infinity" has no meaning as an ideal of some subring of the field $\mathbb{Q}$.

The product formula for the field $\mathbb{Q}$ has the form

$$\left(\prod_{p\in\mathrm{Spec}(\mathbb{Z})} p^{-\nu(f)}\right) \times |f| = 1, \qquad f \in \mathbb{Q}^*.$$

To compare it with the corresponding formula for the projective line, we pass from the product to the sum

$$\sum_{p\in\mathrm{Spec}(\mathbb{Z})} \nu_p(f)\log p + \nu_\infty(f) = 0.$$

For the projective line X over $\mathbb{F}_p$ we have

$$\sum_{P\in X} \nu_P(f)\deg P + \nu_\infty(f) = 0,$$

and for the projective line over $\mathbb{C}$

$$\sum_{P} \nu_P(f) + \nu_\infty(f) = 0.$$

This means that the polynomial f has a number of zeros equal to its degree.

The projective line is a special case of an algebraic curve, and the ring $\mathbb{Z}$ is a special case of rings of integers in fields of algebraic numbers (finite extensions of the field $\mathbb{Q}$). These two concepts combine in the language of the theory of schemes as schemes of dimension 1.

A scheme is a space with a sheaf of rings — the structure sheaf $\mathscr{O}_X$ of regular functions on the scheme X. For each open set $U \subset X$ we know the set

[4] If K is any field, we shall take the (Archimedean) valuation to be $\log|f|$, where $|f|$ is a norm on K.

of functions that are regular on U (namely $\mathscr{O}_X(U)$). In algebraic geometry, the fiber of the sheaf $\mathscr{O}_{X,x}$ at the point $x \in X$ consists of the rational functions that do not have a pole at that point. In the example $X = \mathrm{Spec}(\mathbb{Z})$ that we have considered above, one may set

$$\mathscr{O}_{X,p} = \left\{ f \in \mathbb{Q} : f = m/n \text{ with } m,n \in \mathbb{Z} \text{ and } (n,p) = 1 \right\}.$$

In general, schemes X (of finite type) over the ring $\mathrm{Spec}(\mathbb{Z})$ are the basic object of arithmetical algebraic geometry. There are two types of schemes. Roughly speaking, they are "sets" defined by equations with finite coefficients and sets defined by equations with integer coefficients. We shall denote these two cases below as *geometric* (or *functional*) and *arithmetical* respectively.

Our basic examples $\mathrm{Spec}(\mathbb{F}_p(t))$ and $\mathrm{Spec}(\mathbb{Z})$ happen to be the simplest representatives of these two types. The original classification of schemes is done according to dimension. By that we mean the absolute dimension over the ring $\mathbb{Z}$. For affine schemes it coincides with the Krull dimension of the corresponding ring (that is, the length of a maximal chain of prime ideals).[5]

The examples with which we began our exposition happen to be schemes of dimension 1. Ordered chains of prime ideals in $\mathbb{Z}$, $\mathbb{F}_p[t]$, and $\mathbb{C}[t]$ have length 1. For example, in $\mathbb{Z}$ we have $0 \subset (p)$, and in $\mathbb{C}[t]$ we have $(0) \subset (t - t_0)$.

From the point of view of arithmetic, the ring $\mathbb{C}[t]$ is not of arithmetic type. However, we have included it in our picture as the example of a geometric object closest to our intuition.

One of the routes in arithmetic consists of a transition from varieties over the field $\mathbb{C}$ to varieties over $\mathbb{F}_q$, and then to schemes over $\mathrm{Spec}(\mathbb{Z})$. Such an approach suggests the correct statements of theorems that are valid in both situations, and sometimes also the methods of proving them.

The terminology of schemes arose only in the mid-twentieth century, but attempts to unify number theory and algebraic geometry into a single subject had been made much earlier. Probably the first person to recognize the importance of the concept of dimension for arithmetic was Kronecker. As early as in the nineteenth century he attempted to develop arithmetic not only for dimension 1 but also for arbitrary dimensions. This program was neglected for a long time and was resurrected only in the mid-twentieth century.

We can point to two ground-breaking lectures at International Congresses of Mathematicians in which this problem was discussed. The first was by A. Weil at the 1950 Congress in Cambridge, Massachusetts [52]. Weil described

---

[5] We recall that an ideal $\wp$ of a ring A is prime if the quotient ring $A/\wp$ has no zero divisors and is not the zero ring $\{0\}$. That is, the ring A itself is not an ideal.

Kronecker's goals as follows: "He was, in fact, attempting to describe and to initiate a new branch of mathematics, which would contain both number theory and algebraic geometry as special cases." The second lecture was by I. R. Shafarevich and was given at the Stockholm Congress in 1962 [41].

Between these two events, A. Grothendieck created the theory of schemes ([20] and [14]). I think that Weil's lecture had some influence on Grothendieck. As for Shafarevich's lecture, he was now able to use the language of schemes as the foundation for the further development of arithmetic.

Using the concept of a scheme, we can describe our analogy by the following table, where we compare schemes X of the same dimension from both parts of the table:

$\dim(X)$	geometric case	arithmetical case
...	...	...
2	algebraic surfaces/$\mathbb{F}_q$	arithmetical surfaces
1	algebraic curves/$\mathbb{F}_q$	arithmetical curves = finite coverings of $\mathrm{Spec}(\mathbb{Z})$
0	$\mathrm{Spec}(\mathbb{F}_q)$	$\mathrm{Spec}(\mathbb{F}_1)$

Here $\mathbb{F}_1$ is the "field" of one element (see below).

This table is the result (or starting point) of a completely new way of looking at the analogy between numbers and functions. Over a period of almost 80 years only the row of the table relating to dimension 1 was known and studied.

The leading role in this development belonged to D. Hilbert ([22], [23]). This analogy was one of his favorite ideas, and it was thanks to Hilbert that it achieved fame and became one of the central ideas in the development of number theory during the twentieth century.

In this section, and those that follow, we shall discuss this Hilbert period, and then pass to the description of the jump to other dimensions that occurred in the 1960s.

Let us now repeat the constructions given above in the more general situation of arbitrary curves (or schemes) of dimension 1. Let X be an algebraic curve over a finite field $\mathbb{F}_q$, let $K = \mathbb{F}_q(X)$ be the field of rational functions on X, and let $\nu_x \colon K^\star \to \mathbb{Z}$ be the valuations corresponding to closed points $x \in X$. If we assume that X is a *projective* curve, we have the "sum formula"

$$\sum_{x \in X} v_x(f) \log \# k(x) = 0, \quad f \in K^\star,$$

or the product formula

$$\prod_{x \in X} |f|_x = 1,$$

where

$$|f|_x = \# k(x)^{-v_x(f)}.$$

Here $k(x) = \mathscr{O}_{X,x}/m_x$ is the field of residues of the local ring $\mathscr{O}_{X,x}$ at the point $x \in X$, m_x is a maximal ideal, and $k(x)$ is a finite extension of the field $\mathbb{F}_q$. In the geometric case we can use either the curve X itself (the point of view of algebraic geometry) or the field K of rational functions on X (the point of view of algebra). According to a well known result, these are two descriptions of the same object. (Every field of algebraic functions of one variable has a unique projective nonsingular curve X as a model.)

If we turn to arithmetic, we can observe that the algebraic point of view was dominant for a long time. The object of study was a field K of algebraic numbers, that is, a finite extension of the field $\mathbb{Q}$ of rational numbers. But now we can also use the geometric point of view, that is, the point of view of the theory of schemes. This has the following appearance.

Let X be the set of prime ideals $\wp$ of the ring Λ_K of integers in the field K. To each $\wp \in X$ there corresponds a valuation $v_\wp$, namely

$$v_\wp(f) = \log |f|_\wp, \quad f \in \mathbb{Q}^*,$$

where

$$|f|_\wp = \#(\Lambda_K/\wp)$$

is the corresponding norm. It is easy to see that for $K = \mathbb{Q}$ this definition coincides with the previous definition.

As before, the product of $|f|_\wp$ over all $\wp$, is again not equal to 1. But now we must adjoin a finite number of "infinite" points ∞, where ∞ is a certain embedding of the field K in the field $\mathbb{C}$ of complex numbers. The number of such embeddings equals the degree $[K : \mathbb{Q}]$ of the field K over $\mathbb{Q}$. If the embedding ∞ is real, that is, has the form $K \subset \mathbb{R} \subset \mathbb{C}$, then the norm equals

$$|f|_\infty = |f|_{\mathbb{R}} = |f|.$$

Otherwise we have

$$|f|_\infty = |f|_{\mathbb{C}} = |f|^2.$$

We then have the product formula

$$\prod_{x\in X'\cup\infty} |f|_x = 1.$$

Of course, all these evaluations have a simple interpretation. They correspond to *all* possible completions of the field K, namely, $\wp$-adic fields $K_\wp$, real fields $\mathbb{R}$, and complex fields $\mathbb{C}$. For the field $\mathbb{Q}$ we have a unique embedding $\mathbb{Q}\subset\mathbb{R}$.

A scheme structure on X is defined by the sheaf $\mathscr{O}_X$ whose fibers are

$$\mathscr{O}_{X,\wp} = \{f\in K : v_\wp(f)\geqslant 0\}.$$

The rings $\mathscr{O}_{X,\wp}$ contain a maximal ideal $m_x = \{f\in K : v_\wp(f)\geqslant 0\}$, and completing with respect to it, we obtain a complete local ring $\hat{\mathscr{O}}_{X,\wp}$. Its field of fractions will be the completion of K with respect to the valuation $v_\wp$. For "infinite" points there is no such construction; there are only the fields $\mathbb{R}$ and $\mathbb{C}$, but no subrings in them. For that same reason we cannot introduce the structure of a scheme on the entire set $X\cup\{\infty\}$ of "points" of the field K.

A field extension $K\supset\mathbb{Q}$ gives a mapping of degree $[K:\mathbb{Q}]$

$$X\cup\{\infty\}\to \mathrm{Spec}(\mathbb{Z})\cup\infty,$$

and above an infinite point of the scheme $\mathrm{Spec}(\mathbb{Z})\cup\infty$ lie exactly $[K:\mathbb{Q}]$ infinite points of the scheme $X\cup\{\infty\}$.

But we want to move in the opposite direction, from geometry to arithmetic. And the theory of schemes makes it possible for us to apply the language of geometry in the situation of number theory. If $X=\mathrm{Spec}(\mathbb{Z})$, the closed points x of the scheme X are the primes p, and we have a canonical isomorphism:

$$k(x)\cong\mathbb{F}_p.$$

Here $k(x)=\mathscr{O}_x/m_x$, where m_x is the maximal ideal of $\mathscr{O}_x$.

We can speak of rational *numbers* $f\in\mathbb{Q}$ as rational *functions* on the "curve" X with values $f(x)\in k(x)$. The fundamental difference from real curves is that the values $f(x)$ of our function belong to different fields $\mathbb{F}_q$, as x ranges over the "curve" X. The fields $\mathbb{F}_q$ are different from each other. They are not extensions of the same finite field $\mathbb{F}_p$, as was the case with curves. They contain as a common subfield only the "field" $\mathbb{F}_1$ consisting of one element. We included it in our table under dimension 0 as the final object in the category of schemes of arithmetical type. [6]

[6] Surprisingly, this is not a vacuous concept. It has a rich structure. For example, the higher K-groups $K_*(\mathbb{F}_1)$ are defined, and they coincide with the stable homotopy groups of spheres [47].

2. The Reciprocity Law

Up to now we have spoken only about the simplest aspects of the analogy between numbers and functions. A much more profound fact is the product formula for the normed residue symbol

$$\left(\frac{\lambda,\mu}{\wp}\right),$$

discovered by Hilbert. In [22] he wrote, "The reciprocity law

$$\prod_{\wp}\left(\frac{\lambda,\mu}{\wp}\right)=1$$

reminds one of the Cauchy integral theorem, according to which the integral of a function over a path enclosing all of its singularities always yields the value 0. One of the known proofs of the ordinary quadratic reciprocity law suggests an intrinsic connection between this number-theoretic law and Cauchy's fundamental function-theoretic theorem."[7]

This idea was realized by Shafarevich in his purely local construction of the Hilbert symbol. The proof of reciprocity given by him was a far-reaching extension of the corresponding result for residues [40]. This result is an important contribution to the solution of Hilbert's ninth problem. (See the statement of it in [24] and [17]; and see commentaries on it in the Russian edition of [24].) Shafarevich was probably the first in the Soviet Union to take this analogy seriously.

He used it in a highly non-trivial manner, since it was necessary to compare p-adic number fields whose multiplicative groups had a complex structure with the much simpler fields of power series. Shafarevich's paper begins with the quotation from Hilbert given above. He then corrects Hilbert, showing that the analog of the product formula must be a formula for the sum of the residues rather than the Cauchy integral theorem.

---

[7] "Das Reziprozitätsgesetz in der Fassung

$$\left[\prod_{\mathfrak{w}}\left(\frac{\nu,\mu}{\mathfrak{w}}\right)=[\nu,\mu][\nu',\mu']\cdots[\nu^{(n-1)},\mu^{(n-1)}]\right]$$

erinnert an den Cauchyschen Integralsatz in der Funktionentheorie, demzufolge ein complexes Integral, um alle einzelnen Singularitäten einer Funktion geführt, insgesamt stets den Wert 0 gibt. Einer der bekannten Beweise des gewöhnlichen quadratischen Reziprozitätsgesetzes weist auf einen inneren Zusammenhang zwischen jenem zahlentheoretischen Gesetz und Cauchys funktionentheoretischen Fundamentalsatz hin." (David Hilbert. *Gesammelte Abhandlungen*. Erster Band. Zahlentheorie. New York: Chelsea, 1965 (reprint), pp. 367–368.) — *Transl.*

We first recall the well known constructions from class-field theory. This theory is a method of describing Abelian extensions of a field K of arithmetic type, such as $\mathbb{Q}$ or $\mathbb{F}_p(t)$ (that is, extensions L/K with a commutative Galois group $\mathrm{Gal}(L/K)$). In this case it is called global class-field theory.

If K is a number field, it can be embedded in the completion $K_\wp$ for all prime ideals $\wp$, as we saw above. In this section we shall be dealing only with the fields $K_\wp$. The field $K_\wp$ is called a *local field*, and describing its Abelian extensions is a local class-field theory problem. To this end, let us consider a maximal Abelian extension $K_\wp^{ab}$ as the union of all finite Abelian extensions. The problem is to describe its Galois group over $K_\wp$ using a construction intrinsically connected with the field $K_\wp$ rather than with its extensions.

The main result of local class-field theory is the existence of a canonical homomorphism

$$\varphi : K_\wp^* \to \mathrm{Gal}(K_\wp^{ab}/K_\wp),$$

which has trivial kernel and dense image. Global class-field theory for the field K then reduces in a natural way to the local theories for all the fields $K_\wp$ (see, for example, [10]).

We shall show how the mapping φ defines the Hilbert symbol. Let us assume that a p^n-th root of unity ζ belongs to our field. Here p is the characteristic of the field of residues. We take two numbers λ and μ from $K_\wp$. In this situation we have an Abelian extension $K(x)/K$, where $x^{p^n} = \lambda$. Its Galois group G is the cyclic group of order p^n, and for every $\sigma \in G$ we have

$$\sigma(x) = (\text{some power of the root } \zeta)x.$$

From class-field theory we obtain a mapping

$$K_\wp^* \to \mathrm{Gal}(K_\wp^{ab}/K_\wp) \to G,$$

which we also denote φ. We can now define the Hilbert symbol by the condition

$$\left(\sqrt[p^n]{\lambda}\right)^{\varphi(\mu)} = \left(\frac{\lambda,\mu}{\wp}\right) \sqrt[p^n]{\lambda},$$

where $\sqrt[p^n]{\lambda} = x$, and the result is independent of the choice of x. Thus, to define this symbol, we must *leave* our local field and work with its extensions. The problem posed by Hilbert was to obtain an explicit expression entirely within the field $K_\wp$, and then use it to reverse this process by constructing the mapping φ and developing class-field theory.

Further, if we take λ and μ from the original global field K rather than the local field, we obtain symbols for all prime ideals $\wp$. To obtain a global reciprocity law, one must also define a symbol for infinite points ∞. If we also define a symbol for them (which is much simpler to do), we obtain the reciprocity law described by Hilbert (see above).

In particular, let $K = \mathbb{Q}$, $p = 2$, $n = 1$, and take as λ and μ two odd primes a and b. The only factors that remain in the infinite product of the general reciprocity law are those corresponding to $\wp = (a)$, (b), and ∞. Hilbert's law then reduces precisely to the quadratic reciprocity law of Gauss

$$\left(\frac{a}{b}\right)\left(\frac{b}{a}\right) = (-1)^{\frac{a-1}{2}\cdot\frac{b-1}{2}},$$

where $\left(\frac{a}{b}\right)$ is the Legendre symbol. I now pass to the explanation of Shafarevich's construction and the way in which it is connected with the residues of differential forms on Riemann surfaces. Shafarevich considered the case $n = 1$. The general case, just like the application to the construction of class-field theory starting with the local definition of the symbols, was considered by A. I. Lapin ([29], [30]).[8]

For brevity, we shall denote our local field by K. It is the field of fractions of a discrete valuation ring $\mathcal{O}$ with maximal ideal $\wp$ and with the field of residues $\mathcal{O}/\wp = \mathbb{F}_q$. We denote the generator of the ideal $\wp$ by π.

The multiplicative group K^* has the following structure:

$$K^* = \{\pi^m\}\mathcal{O}^* = \{\pi^m\}RU,$$

where the set R consists of multiplicative representatives of the field of residues $\mathbb{F}_q$, and $U = 1 + \wp$ is called the group of principal units.

The Hilbert symbol has two important properties that are useful for its computation, namely

$$\left(\frac{\lambda\cdot\lambda',\mu}{\wp}\right) = \left(\frac{\lambda,\mu}{\wp}\right)\left(\frac{\lambda',\mu}{\wp}\right)$$

and

$$\left(\frac{(\lambda)^{p^n},\mu}{\wp}\right) = \left(\frac{\lambda,\mu}{\wp}\right)^{p^n} = 1.$$

The same is true for the second argument μ.

[8] His first paper was written in 1950, when he was in detention. The question of the possibility of publishing it was discussed in the Central Committee of the Communist Party of the USSR at the request of the Academy of Sciences and the Ministry of Internal Affairs. After the question had been considered by three members of the *Politburo*, one of whom was L. P. Beria, permission was given to publish it under a pseudonym. However, by then Lapin had been freed, and the paper was published in the usual way. The materials of this correspondence were recently discovered in the archives of the Central Committee. (See *Voprosy Istorii Estestvoznaniya i Tekhniki*, 2001, No. 2, 116–128.)

These properties show that to compute this symbol we must find some system of generators for the group U/U^{p^n}. (For the group R we have $R = R^{p^n}$.) For this purpose, Shafarevich used the Artin–Hasse exponentials $E(\alpha, x)$ and the variant of them $E(\alpha)$. They are defined for elements α of a maximal unramified subring $\mathcal{O}_{nr} \subset \mathcal{O}$ and $x \in \wp$. These functions are homomorphisms from the ring $\mathcal{O}_{nr}$ into the group of units U. We shall see that they are the analogs of the exponential functions. We shall find the following abbreviation useful:

$$\lambda \approx \mu \Longleftrightarrow \lambda\mu^{-1} \in K^{p^n}.$$

We have the following fundamental expansions:

$$\lambda \approx \pi^a E(\alpha) \prod_{\substack{1 \leqslant i < pe/p-1 \\ p \nmid i}} E(\alpha_i, \pi^i),$$

$$\mu \approx \pi^b E(\beta) \prod_{\substack{1 \leqslant i < pe/p-1 \\ p \nmid i}} E(\beta_i, \pi^i).$$

The integers a and b are defined modulo p^n, and the values of all E-functions are defined modulo p^n-powers. If we introduce the homomorphism $\delta : U/U^{p^n} \to U/U^{p^n}$ with $\delta(\lambda) = E(\alpha)$ and $\delta(\mu) = E(\beta)$, the required local expression will have the following appearance:

$$\left(\frac{\lambda, \mu}{\wp}\right) = E(\beta)^a E(\alpha)^{-b} E(\gamma),$$

where

$$E(\gamma) \approx \delta\left(\prod_{i,j} E(i\alpha_i\beta_j, \pi^{i+j})\right).$$

The main thing is to show that the result is independent of the choice of the prime element π. This is true, but the proof is complicated and rather long. Even so, it remained unclear how to find the value of γ explicitly. This was done later by two mathematicians independently of each other, H. Brückner in Germany and S. V. Vostokov in Leningrad ([7], [8], [51]).

To understand the analogy with Riemann surfaces, let us consider a point P on such a surface, a local coordinate t, and the corresponding field $K = \mathbb{C}((t))$ of Laurent series. For the multiplicative group of the field K we have:

$$K^* = \{t^m\}\mathbb{C}^* U,$$

and all elements $\lambda, \mu \in U$ have an expansion

$$\lambda = \exp(A) = \prod_{i \geqslant 1} \exp(\alpha_i t^i), \quad \alpha_i \in \mathbb{C},$$
$$\mu = \exp(B) = \prod_{j \geqslant 1} \exp(\beta_j t^j), \quad \beta_j \in \mathbb{C}.$$

In the field K there are two simple operations: taking the derivative $\partial = \mathrm{d}/\mathrm{d}t$, and the residue $\operatorname{res}(\sum \alpha_i t^i) = \alpha_{-1}$. The analogy with the residue of a differential form at the point P can now be seen from the following table:

$$\begin{aligned}
\exp(\alpha_i t^i) &\sim E(\alpha_i, \pi^i),\\
\exp(\beta_j t^j) &\sim E(\beta_j, \pi^j),\\
\exp(B\,\partial A) &\sim \prod_{i,j} E(i\alpha_i\beta_j, \pi^{i+j}),\\
\operatorname{res}\big(\exp(B\,\partial A)\big) &\sim \delta\Big(\prod_{i,j} E(i\alpha_k\beta_j, \pi^{i+j})\Big).
\end{aligned}$$

To compare the left-hand side and the right-hand side in the last row, one must note that

$$A\partial B = \sum_{i,j} j\alpha_i\beta_j t^{i+j-1} \quad \text{and} \quad \operatorname{res}(A\partial B) = \sum_{i+j=0} j\alpha_i\beta_j.$$

We see that both methods are completely *parallel*. To be specific, the operation δ plays the role of the residue. But the second construction in the number field K is, of course, much more complicated. In particular, the numbers $E(\alpha)$ and $E(\beta)$ have disappeared from our table. It is natural to compare their role in the definition of the Hilbert symbols with the so-called tame symbol in the field $C((t))$ rather than with the residue at the point P.

3. The General Situation in the 1950s and 1960s

The 1950s were a period of reawakened interest in algebraic geometry in the Soviet Union (although it may not be quite accurate to speak of a "reawakening," since up to the 1950s few people in the USSR were interested in algebraic geometry).[9]

[9] One can mention only N. G. Chebotarev and, in particular, his book [11], and the papers of I. G. Petrovskii and O. A. Oleinik on the topology of real algebraic varieties, written just after World War II. See their survey in [35].

Nevertheless, after World War II there were several people seriously interested in algebraic geometry, first among them I. R. Shafarevich, who tried to study the available literature. To show how difficult this was to do, say, in the late 1940s, there was a seminar at Moscow University in which several mathematicians, Shafarevich among them, attempted to understand the proof of the Mordell–Weil theorem, but were unable to do so.

One cause of this situation is understandable — the strict isolation from the rest of the world. For example, when mathematicians from the whole world met in the International Congress at Cambridge, Massachusetts, for the first time after the war, there was no one there from the USSR. There was only a telegram communicating that “Soviet mathematicians, who are extremely busy with their current research, cannot participate in the Congress” [38]. This was the very Congress at which A. Weil gave the lecture we mentioned above. In the late 1950s the situation improved somewhat, but, of course, strong restrictions remained.

The few visits of Western mathematicians, among whom one must mention Erich Kähler, exerted a great influence on the development of ideas during the 1950s. Because of the rarity of direct contacts, the study of the literature was very important. So far as I know, the notes of the Cartan seminar [9] were difficult to find in Moscow, but they were studied very thoroughly. The book of Hodge on harmonic integrals [25] and the lecture notes of Siegel on automorphic functions of several complex variables [43] were also very popular. The latter were translated into Russian in 1954 by I. I. Pyatetskii-Shapiro [44], and in the mid-1950s Shafarevich and Pyatetskii-Shapiro conducted a seminar on this book, which, from the point of view of understanding the proofs of the theorems in the book, turned out to be much more successful. Perhaps the work of Pyatetskii-Shapiro on bounded domains and his solution of Cartan’s problem on the existence of nonsymmetric bounded domains were the result of this activity. (See his reminiscences of this time in the collection [55].) In 1960 and 1961 a large seminar on the theory of deformations of complex structures, which had recently been created by K. Kodaira and D. Spencer, was organized in Moscow University by E. B. Dynkin, M. M. Postnikov, and I. R. Shafarevich.

But the most important aspect for our history is the interest in the classical theory of algebraic surfaces. This is understandable from the point of view of the analogy discussed above. The constructions described in Section 1 belong to classical algebraic number theory, and thus belong to dimension 1 according to our classification. Shafarevich later began to study Diophantine equations, in particular, elliptic curves of algebraic number fields, and realized the necessity of moving on to higher dimensions. After all, schemes of dimension 2 must correspond to curves over such fields. Fortunately, the concept of a scheme itself had only just arisen. This program was concisely formulated in his Stockholm lecture, to which we referred earlier. To understand arithmetic in dimension 2, one must first have a clear picture for algebraic surfaces. That is, we must have a theory of the corresponding geometric objects — algebraic surfaces, primarily over the field of complex numbers, and then over other fields.

Such a theory already existed in the work of Italian mathematicians who studied algebraic geometry — G. Castelnuovo, F. Enriques, F. Severi, and others. But the main definitions and proofs of the Italian geometers were not sufficiently rigorous and were sometimes simply incomprehensible. In fact, this subject was a rather isolated area of mathematics, having its own rules and laws, which were rejected by the greater part of the mathematical community. The rise of the theory of sheaves, which came out of complex-analytic geometry (the paper of J.-P. Serre [39]) and the analytic methods in the papers of Kodaira and Spencer, made it possible to give new, rigorous proofs of many results of the Italian school. It suffices to compare the lecture of B. Segre at the 1954 International Congress in Amsterdam, which belongs entirely to the earlier epoch, with the lecture of Grothendieck at the 1958 Congress in Edinburgh, to get a sense of the revolution that had taken place.

In the early 1960s Shafarevich organized a seminar at Moscow University, in which the classical works of the Italian mathematicians on the theory of algebraic surfaces were studied. The main source was the book of Enriques [16]. This seminar was conducted in two stages, during the 1961–62 and 1962–63 academic years. It is interesting that about the same time (more precisely, in the late 1950s) interest in the results of the Italians in the area of algebraic surfaces also arose in the USA, in the schools of O. Zariski and K. Kodaira.

The result of the two-year study was the publication of the book *Algebraic Surfaces* [2], which appeared in 1965 in the *Trudy Matem. Inst. im. Steklova*. This volume contained the following chapters:

1. Birational transformations (A. B. Zhizhchenko);

2. Minimal models (A. B. Zhizhchenko);

3. Rationality criteria (A. B. Zhizhchenko);

4. Ruled surfaces (I. R. Shafarevich);

5. Minimal models of ruled and rational surfaces (Yu. I. Manin, A. N. Tyurin, Yu. R. Vainberg);

6. Surfaces of general type (B. G. Moishezon);

7. Surfaces with a pencil of elliptic curves (I. R. Shafarevich);
8. Algebraic surfaces with $\kappa = 0$ (B. G. Averbukh);
9. The space of moduli of complex surfaces with $q = 0$ and $K = 0$ (G. N. Tyurina);
10. Enriques surfaces (B. G. Averbukh).

The book gave a complete exposition of the classification of algebraic surfaces, as it had been done by the Italians, with results proved in the language of sheaves. In some places the classical propositions were corrected or extended. Remarkably, this seminar and the book served as the main impetus for the further development of algebraic geometry in Moscow. We give here just a short list of the further research that grew out of it:

- rational surfaces and multidimensional varieties (with the solution of Lüroth's problem [10] and the classification of Fano varieties) (Yu. I. Manin and V. A. Iskovskikh);
- the theory of vector bundles on algebraic curves and surfaces (A. N. Tyurin, F. A. Bogomolov);
- $K3$ surfaces (G. N. Tyurina, I. R. Shafarevich, I. I. Pyatetskii-Shapiro, V. V. Nikulin, A. N. Rudakov, V. S. Kulikov, and others);
- elliptic sheaves and the main homogeneous spaces (I. R. Shafarevich, O. N. Vvedenskii);
- multidimensional birational and analytic geometry (including criteria for ampleness) (B. G. Moishezon);
- minimal models for arithmetical surfaces (I. R. Shafarevich).

[10] Shafarevich heard the statement of this problem from Chebotarev, who had been interested in it for some time. In particular, Chebotarev had discussed the problem in his lecture at the Zürich Congress in 1932. The problem is to explain how a subfield of the field of rational functions $k(x_1, \ldots, x_n)$ in n variables can again be a field of the same type. This is true for $n = 1$ and for $n = 2$, $k = \mathbb{C}$. The proof of this last fact was given by the Italians and used the full power of the theory of surfaces. Manin and Iskovskikh constructed counterexamples in dimension 3. Independently, the problem was solved by P. A. Griffiths and H. C. Clemens, M. Artin and D. Mumford.

The majority of papers in this list were written after the seminar and under its influence. The papers of Shafarevich on the theory of the principal homogeneous spaces, which preceded the seminar, are an exception. They arose out of his interest in Diophantine equations, primarily the theory of elliptic curves. As early as 1956, in a lecture given at the third All-Union Mathematical Congress, Shafarevich pointed out the analogy between embedding problems in the Galois theory of algebraic number fields and the classification problem for elliptic curves defined over such fields. What these problems had in common was their statement in the languages of Galois cohomology and the presence of local invariants connected with the completions of the base number field. (For more details, see [15].) It was natural to pass from these arithmetical problems to the study of elliptic curves over a field of algebraic functions, and that is what the surfaces with a pencil of elliptic curves in the preceding list amount to.

A more detailed exposition of the subsequent development of algebraic geometry in Moscow can be found in [1], [15], [26]. The general atmosphere of this era is well described in [55].

4. Arithmetical Surfaces

I. R. Shafarevich

The development of the last area in our list was of great significance for number theory. In his 1966 lectures in Bombay (now Mumbai) Shafarevich [42] gave a systematic development of the fundamental concepts and results from the theory of algebraic surfaces for the case of arithmetical surfaces. In these lectures, using the language of schemes, he constructed a theory of intersections, and defined and studied birational transformations and minimal models. [11]

As an illustration, we give the simplest example of an arithmetical surface arising from the affine line over the field $\mathbb{Q}$. Let $X = \operatorname{Spec} \mathbb{Z}[t]$. This is a scheme of dimension 2, and it is mapped onto $B = \operatorname{Spec} \mathbb{Z}$. The fibers of this mapping over points $p \in B$ are affine lines over the finite fields $\mathbb{F}_p$. In Fig. 1 we represent the points of the fibers (which are simultaneously points of the scheme X) with coordinates in finite fields (that is, residues $\bmod p$). The

[11] Some of these results were obtained independently by S. Lichtenbaum [32] in the USA and P. Deligne [12] in France.

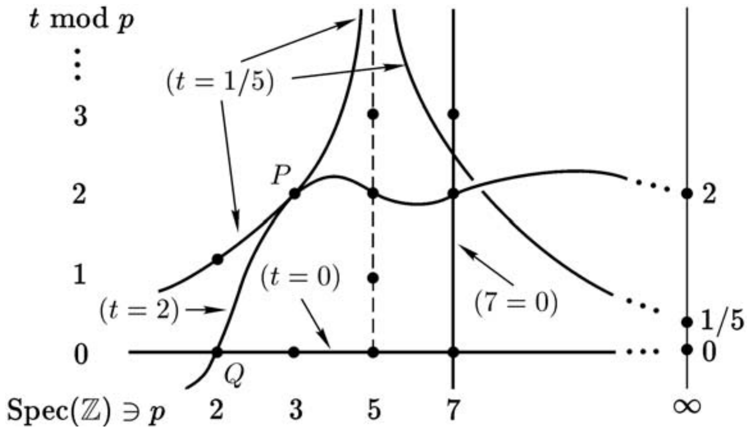


Fig. 1

"surface" X contains "curves" defined by equations of the form $f = \text{const}$, where $f \in \mathbb{Z}[t]$.

The curves $t = 0$ and $t = 2$ intersect at a point Q of the fiber over $p = 2$ and have first-order tangency there; that is, they are transversal. The curves $t = 1/5$ and $t = 2$ intersect at a point P of the fiber over $p = 3$ and have second-order tangency. Indeed,

$$2 \equiv 0 \mod 2, \qquad 2 \not\equiv 0 \mod 2^2,$$
$$5 \cdot 2 \equiv 1 \mod 3, \qquad 5 \cdot 2 \equiv 1 \mod 3^2, \qquad 5 \cdot 2 \not\equiv 1 \mod 3^3.$$

In the last case, in a neighborhood of the fiber over $p = 3$ we have the 3-adic series expansions

$$2 = 2 + 0 \cdot 3 + 0 \cdot 3 + \cdots,$$
$$1/5 = 2 + 0 \cdot 3 + 1 \cdot 3^2 + \cdots.$$

The general definition of the index of intersection of two curves $C = (f = 0)$ and $D = (g = 0)$ at a point x looks as follows:

$$(C \cdot D)_x = \log \# k(x) \cdot \operatorname{length} \mathbb{Z}[t]/(f, g), \tag{1}$$

where $\log \# k(x)$ is introduced in analogy with the one-dimensional case (see Section 1). Of course, this definition makes sense only if the curves C and D intersect in a finite set of points, that is, have no common components. To give the definition in the general case in algebraic geometry one normally uses the shift method, thereby bringing the curves into general position. As a shift one uses the adjunction of the divisor of a rational function, since the index of intersection of any curve with a divisor of a function on a complete surface is zero. This last property is a generalization of a property of divisors of functions on curves to the case of a surface: their degree is zero (see Section 1). As we have seen, for this last property to hold one must have a complete or compact curve.

Accordingly, in the two-dimensional situation one must have something like a complete surface. However, only incomplete schemes defined over an affine base — the spectrum of the ring of integers of the field of algebraic numbers — were considered in Shafarevich's lectures. It was clear from the very beginning that such an approach is only a partial analog of the situation with algebraic surfaces. At the end of the lectures the problem was posed: to find a complete analog of an algebraic surface and construct a theory of intersections for it. Let us consider this problem in more detail.

Comparing the geometric and arithmetical cases in dimension 1, we saw that the complete analog of a projective curve X is the set $X = X' \cup \infty$, and the structure of a scheme is present only on the subset X'. The point ∞ is adjoined to X' "by hand," so to speak. It is unknown what structure must be on the entire set X. It seems that the theory of schemes is unsuitable for this purpose.

This is also true for higher dimensions. The complete object in the right-hand column of the table above, which corresponds to projective algebraic surfaces, consists of the arithmetical surfaces introduced by S. Yu. Arakelov [3], [4] in the early 1970s. It is not very convenient to compare them directly with algebraic surfaces. For such a comparison an algebraic surface X is usually endowed with the structure of a pencil of algebraic curves parameterized by a projective nonsingular curve B. Thus we have a mapping

$$f: X \to B,$$

whose fibers are projective curves and almost all them are nonsingular curves of the same genus g.

Let us now compare this mapping with the structure mapping

$$f: X' \to \mathrm{Spec}(\mathbb{Z})$$

for an arithmetical surface. Since the basis "curve" $\mathrm{Spec}(\mathbb{Z})$ is not complete, this means that the two-dimensional scheme X' is also incomplete, and thus cannot be regarded as a precise analog of the algebraic surface X. In exactly the same way as in the case of dimension 1, we need to adjoin something.

To understand Arakelov's idea, let us return to the case of an algebraic surface X with the mapping f and divide the basis curve B into two distinct parts B' and S, where B' is an open subset and S is a finite subset. We wish to regard B' as the analog of $\mathrm{Spec}(\mathbb{Z})$ and $f^{-1}(B')$ as the analog of X'. We now seek the missing part of the arithmetical surface that corresponds to the part of X lying over S. To solve this problem we need to study this piece of the algebraic surface X more attentively.

With the mapping f one can connect an algebraic curve Y defined over the field K of rational functions of the curve B. (In the theory of schemes this construction, which was known earlier in classical algebraic geometry, is called the transition to the generic fiber, and it admits a simple and rigorous definition.) If $b \in B$, we have the curve $Y_{(b)}$ obtained by replacing the base field K by the local field K_b,

$$Y \otimes_K K_b,$$

and the two-dimensional scheme $X_{(b)}$,

$$X \otimes_B \mathrm{Spec}(\mathscr{O}_b),$$

obtained by replacing the base curve B by an "infinitesimal" neighborhood $\mathrm{Spec}(\mathscr{O}_b)$ of the point b.

Now let $b \in S$. We can then compare the field K_b with the fields that are the completions of the field of algebraic numbers at "infinity." In the arithmetical case we have no analogs for the schemes $X_{(b)}$, but we can define the curves $Y_{(b)}$ by the same formula as above. For the field $\mathbb{Q}$ this has the following appearance

$$Y_\infty = Y \otimes_{\mathbb{Q}} \mathbb{R} \subset Y \otimes_{\mathbb{Q}} \mathbb{C}.$$

Thus we obtain Riemann surfaces over the field $\mathbb{C}$. Arakelov assumed that the choice of some Hermitian metric on the Riemann surfaces Y_∞ can be regarded as replacing the nonexistent model $X_{(\infty)}$. Such an approach can be explained as follows. In the geometric case for the curves $Y_{(b)}$, there is a bijective correspondence

$$\left\{\text{sections of the projection } X_{(b)} \to \mathrm{Spec}(\mathscr{O}_b)\right\} \longleftrightarrow Y_{(b)}(K_b)$$

between sections of the mapping f and rational points of the curve $Y_{(b)}$ (see Fig. 2 below). For any two distinct sections C and D their index of intersection is defined (see Eq. (1)) and can be used as a metric on the curve $Y_{(b)}$. The choice of a different model $X_{(b)}$ gives another metric on $Y_{(b)}$. Thus, one can try to regard the set of models $X_{(b)}$ as a certain set of metrics on $Y_{(b)}$. Such an approach to the interpretation of Arakelov's theory arose much later [13]. The description of an exact correspondence between models and metrics was given only in [56] and [48].

We now give a table that will be more precise than the general picture given above.

geometric case	arithmetical case
projective nonsingular curve B with finite subset $S \subset B$	spectrum of the ring Λ of integers and embeddings of number field K
projective algebraic surfaces X over $\mathbb{F}_q$ with mapping $f\colon X \to B$ onto B	arithmetical surfaces
surface $X' = f^{-1}(X - S)$ with mapping $f\|_{X'}$ onto $X - S$	two-dimensional scheme X' over $\mathrm{Spec}(\Lambda)$
algebraic curve $Y_{(b)}$ with $b \in S$	compact Riemann surfaces $Y_{(\infty)}$ corresponding to embeddings of K into $\mathbb{C}$
schemes $X_{(b)}$ with $b \in S$	Hermitian metrics on surfaces $Y_{(\infty)}$

Arakelov then defined such concepts as divisor, divisor of a function and differential form, linear equivalence, index of intersection, and canonical class. He proved an analog of the adjunction formula and also stated an analog of the Riemann–Roch theorem in [4].

Arakelov's construction lay undisturbed for nearly ten years, and only in the early 1980s did it serve as the point of departure for further development in the papers of G. Faltings. We refer to [54], [18], [19], [45], [46], [49], and [37], where these later events are related. This line exerted a powerful influence on the development of number theory and also on the development of elementary-particle physics [6], demonstrating again the notorious "unreasonable effectiveness of mathematics in the natural sciences."

5. Height and Arakelov's Theory

In this section we shall explain the origins of Arakelov's theory, starting from the concept of height — a basic tool of the theory of Diophantine equations.

Let X be a projective algebraic variety defined over a global field K of dimension 1 (in other words, K is either the field of algebraic numbers or the field $K = k(B)$ of algebraic functions on some curve B defined over the field of constants). And let D be a divisor on X (that is, an integer linear combination of subvarieties of codimension 1).

A *height* is a function

$$h_{X,D}\colon X(K) \to \mathbb{R}$$

on the set of rational points $X(K)$ depending on the choice of the divisor D on X. Actually, the height is not uniquely determined by these data. We shall write $f \approx g$ for two numerical functions if $f - g$ is a bounded function. The height is defined as an equivalence class of functions relative to such an equivalence relation. (For details, see [27].)

Here is a simple but important example: Let $X \subset \mathbb{P}^n$, $K = \mathbb{Q}$, and let D be a hyperplane section. Then the point $P \in X(K)$ is $(x_0 : \cdots : x_n) \in \mathbb{P}^n(\mathbb{Q})$, where the x_i are relatively prime integers. We have

$$h_{X,D} = \max_i \log |x_i|.$$

From this one can see that the number of points of a bounded height is finite — a very important property, which makes it possible to obtain various kinds of finiteness theorems for Diophantine equations.

More generally, for a global field K with a set of valuations v (in which we include infinite points in the numerical case) and norms $|\cdot|_v$ corresponding to them, the height of a point in projective space is defined as the product

$$h(P) = \prod_v \max_i \log |x_i|_v,$$

and the product formula (see Section 1)

$$\prod_v |x|_v = 1, \quad x \in K^*$$

shows that this expression is well defined (but depends, for example, on the choice of the system of projective coordinates).

The height has the following properties.

i) INVARIANCE UNDER LINEAR EQUIVALENCE: if $D' = D + (f)$, where (f) is the divisor of the function f, then

$$h_{X,D'} \approx h_{X,D}.$$

ii) FUNCTORIALITY: if $f\colon X \to Y$ is a morphism of algebraic varieties and D is a divisor on Y, then

$$h_{X,f^*D} \approx f^* h_{X,D},$$

where f^* is the inverse image of the divisors or functions respectively.

iii) FINITENESS: if the divisor D is a hyperplane section, then for every $h \in \mathbb{R}$ the set
$$\{P \in X(K) : h_{x,D}(P) \leqslant h\}$$
is finite.

Using these properties A. Weil proved that the group $A(K)$ of rational points on an Abelian variety A over a global field K is finitely generated[12] (the Mordell–Weil theorem).

If we are in the geometric situation (according to the preceding classification), the base field K has the form $k(B)$, and there exist a projective variety Y and a morphism $f : Y \to B$ with a general fiber X. Under reasonable hypotheses on X and Y (irreducibility and flatness of the morphism f) there is a bijective correspondence
$$\{\text{section of the mapping } f : Y \to B\} \longleftrightarrow X(K)$$
between sections C and rational points P on X. The divisor D also defines a certain divisor $\bar{D}$ on Y (Fig. 2).

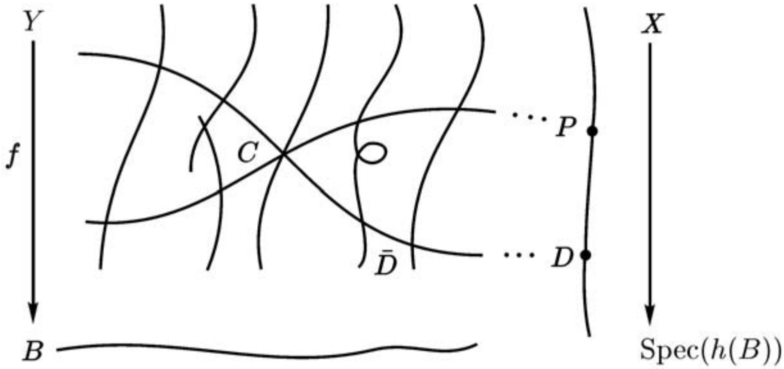


Fig. 2

Under these conditions we have

iv) $h_{X,D} \approx (C,D)_Y$.

(For this equality to make sense it is, of course, necessary to have a theory of intersections on the variety Y, for example, to assume that Y is a nonsingular variety.)

[12] Modulo the K/k-trace in the geometric case. See [27].

Thus, in essence, the height is the index of intersection and this circumstance can be used in different areas. If X is an algebraic curve, then Y is a surface, its model from the preceding section. This connection suggests that the height may serve as a starting point for a construction of a theory of intersections on arithmetical surfaces.

An obvious defect of the height is the approximateness of its definition and its functoriality, only up to the equivalence relation indicated above. J. Tate invented a definition of height on Abelian varieties A: this is a canonical function $\hat{h}_{A,D}$ on the set of rational points that behaves functorially relative to homomorphisms of Abelian varieties and is such that $\hat{h}_{A,D} \approx h_{A,D}$.

For the index of intersection in the geometric case we have the obvious expansion

$$C._Y D = \sum_{b \in B} C._b D$$

over the indices of intersection in all the fibers of the mapping f. A. Néron [33] found a new approach to the construction of Tate's height on Abelian varieties, which simultaneously gave a local expansion for it over points of the base B (or valuations ν of the base field K):

$$\hat{h}(P) = \sum_{\nu} h_{\nu}(P) + \sum_{\infty} h_{\infty}(P), \quad P \in A(K).$$

We remark that in contrast to the global function $\hat{h}(P)$ the local components are not defined for all $P \in A(K)$ but only for $P \in (A - D)(K)$, becoming infinite on the divisor D.

In the numerical case the definition of local components is quite varied, depending on the nature of the point (non-Archimedean ν or Archimedean infinity ∞). For $\hat{h}_{\nu}(P)$ one uses the index of intersection on a special nonsingular model of an Abelian variety A over the ring of integers of the base field K (Néron's minimal model).

Analysis first enters the game at infinity. Let A be an Abelian variety over the field of complex numbers $\mathbb{C}$, and let D be a positive divisor on A (that is, $C = \sum_i n_i D_i$, where $n_i \in \mathbb{Z}$, $n_i \geqslant 0$, and D_i is an irreducible subvariety of codimension 1). Then A is a complex torus $\mathbb{C}^n/\Gamma$, where Γ is a discrete subgroup of rank $2n$ in $\mathbb{C}^n$.

On A itself the divisor D is not the divisor of poles of any holomorphic function, but one can find such a function on the universal covering $\mathbb{C}^n$. And, what is important, this function can be constructed in a canonical manner.

The divisor D is an algebraic cohomology class, that is, an element of the space $H^{1,1} \subset H^2(A, \mathbb{C})$ according to the Hodge decomposition in cohomologies of the variety A. On an Abelian variety the space $H^{1,1}$ consists of Hermitian matrices. If H is the matrix corresponding to the divisor D, there exists a unique (suitably normalized) theta-function $\theta(z) = \theta_D(z)$ on $\mathbb{C}^n$ having the following properties:

(i) the divisor of the poles of (θ_D) is D;

(ii) $\theta(z+\gamma) = \theta(z)\exp\big(\pi H(\gamma,z) + \pi/2H(\gamma,\gamma)\big)\cdot\chi(\gamma)$, where $z \in \mathbb{C}^n$, $\gamma \in \Gamma$, and $|\chi(\gamma)| = 1$. (For details, see [53].)

The local component of the height $\hat{h}_\infty(P)$ can now be defined as follows [33]:

$$\hat{h}_\infty(z) = -\log|\theta_D(z)| + \pi H(z,z).$$

It follows from property (ii) of the theta-function that this function is invariant relative to Γ, that is, it is a function of the point $P \in A(\mathbb{C})$. Moreover, locally on A, in a neighborhood of each point of the divisor D we have

$$\hat{h}_\infty(P) \approx \log|\text{holomorphic equation for } D|. \tag{2}$$

One can now look at Néron's construction from a different point of view. How can the function $\hat{h}_\infty$ be distinguished among all the smooth real-valued functions on $(A-D)(\mathbb{C})$ satisfying (2)? We remark that all functions having the property (2) differ from one another by a function that is bounded and smooth on *all* of A.

It is not difficult to see that the condition that distinguishes $\hat{h}_\infty$ is the Poisson equation

$$\Delta\hat{h}_\infty = \text{const} \quad \text{outside } D, \qquad \text{or} \qquad \Delta\hat{h}_\infty = \delta_D \quad \text{on all of } A. \tag{3}$$

Here

$$\Delta = \sum_{i,j} \frac{\partial^2}{\partial z_i \partial \bar{z}_j}$$

is the Laplacian corresponding to the flat metric on $\mathbb{C}^n$, which, being Γ-invariant, can be lowered to A; δ_D is the delta-function corresponding to the subvariety D.

This observation suggests that the definition of the local components $\hat{h}_\infty(P)$ can be given for any algebraic variety X if one fixes some Hermitian metric on it. Then, for every divisor D there exists a function $\hat{h}_{\infty,D}$ satisfying conditions (2) and (3) that is unique (up to a constant). Such a definition was introduced in [36] and served as the point of departure for the development of Arakelov's theory.

We can now describe Arakelov's theory as follows. An arithmetical surface $\widetilde{X}$ consists of a nonsingular two-dimensional scheme X and a mapping of it $f: X \to B$ onto the one-dimensional scheme $B = \mathrm{Spec}(\Lambda_K)$, where Λ_K is the ring of integers of the field K of algebraic numbers. We denote the set of infinite points of the field K by B_∞, and for each $v \in B_\infty$ we choose a Hermitian metric μ_v on the Riemann surface $X_v = X \otimes_v \mathbb{C}$.

An Arakelov divisor $\widetilde{C}$ on $\widetilde{X}$ is a linear combination of an ordinary divisor C on X and the "infinite" fibers X_v, and the latter occur with real coefficients. Let

$$\widetilde{C} = C + \sum_{v \in B_\infty} a_v X_v, \quad \widetilde{D} = D + \sum_{v \in B_\infty} b_v X_v$$

be two Arakelov divisors. Assume that C and D intersect in a finite set of points. Then

$$\widetilde{C} \cdot \widetilde{D} = C._X D + \sum_{v \in B_\infty} (C \cdot D)_v + \sum_{v \in B_\infty} a_v \deg D/B + \sum_{v \in B_\infty} b_v \deg C/B,$$

where $C._X D$ is the index of intersection on the scheme X, and the archimedean indices $(C \cdot D)_v$ are defined using the Green's functions $G(P,Q)$ $(= G_v(P,Q))$ constructed with respect to the metric μ_v.

We recall that a Green's function on a Riemann surface $X = X_v$ is determined uniquely by the following conditions:

1) G is a smooth real-valued positive function on $X \times X$ minus its diagonal;

2) if z is a local holomorphic coordinate near the point P_0 on X, then the function $G(P,Q)$ near (P_0, P_0) has the form

$$|z(P) - z(Q)| \cdot (\text{smooth nonvanishing function});$$

3) $\Delta_Q \log G(P,Q) = \mathrm{d}\mu/\mathrm{d}z \wedge \mathrm{d}\bar{z}$, where $\Delta_Q = (1/\pi \mathrm{i})(\partial^2/\partial z \partial \bar{z})$ is the Laplacian and $\mathrm{d}\mu$ is the volume element that arises from the metric μ_v.

Let us set

$$(C \cdot D)_v = \sum_{P,Q} n_P m_Q \log G_v(P,Q),$$

if $C = \sum n_P P$ and $D = \sum m_Q Q$ are the expansions of the divisors into (finite) sums of points on $X_v(\mathbb{C})$.

If F is a rational function on X we define its Arakelov divisor as

$$(\widetilde{F}) = (F)_X + \sum_v X_v, \quad a_v = -\int \log |F| \, \mathrm{d}\mu_v.$$

Here $(F)_X$ is the usual divisor of the function F in the scheme X.

We can now define linear equivalence of divisors on $\widetilde{X}$:

$$\widetilde{C} \approx \widetilde{D}, \quad \text{if} \quad \widetilde{C} - \widetilde{D} = (\widetilde{F}).$$

The main property of the index of intersection is its invariance relative to linear equivalence

$$\widetilde{C} \cdot \widetilde{D} = \widetilde{C} \cdot (\widetilde{D} + (\widetilde{F})).$$

This makes it possible to define the index of intersection for any two Arakelov divisors by the usual method of algebraic geometry.

Among the classes of divisors relative to linear equivalence there is, just as in ordinary geometry, a canonical class. The divisors in that class are constructed from a rational differential form ω of degree 1 on X (more precisely, it is a section of the relative cotangent bundle of X over B). We set

$$(\widetilde{\omega}) = (\omega)_X + \sum_v a_v X_v, \quad a_0 = -\int_{X_v} \log \left| \frac{\omega \wedge \widetilde{\omega}}{\mathrm{d}\mu_v} \right| \mathrm{d}\mu_v,$$

where $(\omega)_X$ is the divisor of the form ω in the scheme X.

The adjunction formula for the divisor $\widetilde{C} = C$, which represents a section of C on an arithmetical surface (that is, a surface having degree 1 over the base B), has the form

$$\widetilde{C} \cdot (\widetilde{\omega}) + \widetilde{C} \cdot \widetilde{C} = 0.$$

In ordinary algebraic geometry the adjunction formula for a curve C on a surface X is the following

$$C \cdot (\Omega_X) + C \cdot C = 2g - 2,$$

where Ω_X is the class of differential forms of degree 2 on X and g is the genus of the (nonsingular) curve C. If the surface X is fibered over the curve B and C is a section of this fiber bundle, then $2g - 2 = C \cdot f^*(\Omega_B)$ (here Ω_B is the canonical class of the curve B), and the adjunction formula has the form

$$C \cdot \big((\Omega_X) - f^*(\Omega_B)\big) + C \cdot C = C \cdot \big((\Omega_{X/B})\big) + C \cdot C = 0,$$

where $(\Omega_{X/B})$ is the class of divisors corresponding to the relative cotangent bundle of the surface X over B.

It is this equality that carries over to the arithmetical surfaces of Arakelov. For a more detailed exposition of Arakelov's theory and its subsequent development for varieties of any dimension see [18], [19], [28], and [45].

In our brief exposition we have examined only one twig on the enormous tree of the analogy between numbers and functions. Some idea of the tree as a whole can be gained from the following list:

- class-field theory (a parallel description of Abelian extensions of numerical and function fields);
- the zeta- and L-functions of schemes of dimension 1 (the problem of meromorphic continuation and the proof of the functional equation);
- the theory of height of rational points in Diophantine geometry;
- the Arakelov theory of arithmetical varieties;
- the classification of semi-simple algebraic groups over local fields;
- the theory of Bruhat–Tits buildings and symmetric spaces;
- arithmetical subgroups of algebraic groups, in particular the theory of reduction;
- the Langlands program of describing representations of Galois groups of local and global fields;
- the analogy between explicit formulas in number theory and the Lefschetz trace formula (A. Weil, C. Deninger, A. Connes).

This list is surely incomplete, [13] and the whole story is still far from over.

Bibliography

[1] Algebra, Mathematical Logic, Number Theory, Topology (survey articles on the occasion of the fiftieth anniversary of the Steklov Mathematical Institute of the USSR Academy of Sciences). *Proc. Steklov Inst. Math.*, 1984, **168**.

[2] Algebraic Surfaces. *Proc. Steklov Inst. Math.*, 1965, **75**, 281 pp.

[3] S. Yu. Arakelov. Intersection theory of divisors on an arithmetic surface. *Math. USSR, Izv.*, 1974, **8**, 1167–1180.

[4] S. Yu. Arakelov. Theory of intersections on an arithmetic surface. In: *Proceedings of the International Congress of Mathematicians* (Vancouver, 1974), V. 1. Montreal: Canad. Math. Congress, 1975, 405–408.

[5] Z. I. Borevich, I. R. Shafarevich. *Number Theory* (transl. N. Greenleaf). New York – London: Academic Press, 1966 (Pure Appl. Math., **20**).

[13] We mentioned above the influence of number theory on physics. They say that, after Faltings' talk at the Congress in Berkeley (1986), Witten bought all books on number theory in nearby shops. Later, the p-adic analogs of quantum mechanics and certain models of field theory were constructed. (See the survey [50].)

[6] J. B. Bost. Fibrés déterminants, déterminants régularisés et mesures sur les espaces de modules des courbes complexes. In: Séminaire Bourbaki, février 1987, exposé 676. *Astérisque*, **152–153**. Paris: Soc. Math. France, 1987, 113–149.

[7] H. Brückner. Eine explizite Formel zum Reziprozitätsgesetz für Primzahlexponenten p. In: *Algebraische Zahlentheorie* (Oberwolfach, 1964). Mannheim: Bibliographisches Institut, 1967, 31–39.

[8] H. Brückner. Explizites Reziprozitätsgesetz und Anwendungen. *Vorles. Fachber. Math. Univ. Essen*, 1979, Heft 2.

[9] H. Cartan. *Séminaire École Normale Supérieure*, 1950/51, 1951/52, 1953/54, 1957/58. Secrétariat mathématiques, Paris.

[10] J. W. S. Cassels, A. Frölich. *Algebraic Number Theory*. London: Academic Press, 1967.

[11] N. G. Chebotarev. *Theory of Algebraic Functions*. Moscow–Leningrad: Gostekhizdat, 1948 (Russian).

[12] P. Deligne. Intersections sur les surfaces régulières. In: *Groupes de monodromie en géométrie algébrique*. II (SGA 7 II, 1967–1969). Berlin – New York: Springer, 1973, exposé X, 1–38 (Lecture Notes in Math., **340**).

[13] P. Deligne. Le déterminant de la cohomologie. In: *Current Trends in Arithmetical Algebraic Geometry* (Arcata, CA, 1985). Providence, RI: Amer. Math. Soc., 1987, 93–177 (Contemp. Math, **67**).

[14] P. Deligne. Quelques idées maîtresses de l'œuvre de A. Grothendieck. Matériaux pour l'histoire des matématiques au XXième siècle. Actes de colloque à la mémoire de Jean Dieudonné (Nice, 1996). Paris: Soc. Math. France, 1998, 11–20 (Séminaires et Congrès, **3**).

[15] S. P. Demushkin et al. Igor' Rostislavovich Shafarevich (on his sixtieth birthday). *Russ. Math. Surveys*, 1984, **39**(1), 189–200.

[16] F. Enriques. *Le superficie algebriche*. Bologna: Nicola Zanichelli, 1949.

[17] D. K. Faddeev. On Hilbert's ninth problem. In: *The Hilbert Problems*. Moscow: Nauka, 1969, 131–140 (Russian).

[18] G. Faltings. Calculus on arithmetic surfaces. *Ann. Math.*, 1984, **119**, 387–424.

[19] G. Faltings. *Lectures on the Arithmetic Riemann–Roch Theorem*. Princeton, NJ, 1992 (Ann. Math. Studies, **127**).

[20] A. Grothendieck. The cohomology theory of abstract algebraic varieties. In: *Proceedings of the International Congress of Mathematicians* (Edinburgh, 1958). Cambridge University Press, 1960, 103–118.

[21] K. Hensel. *Theorie der algebraischen Zahlen*. Leipzig: B. G. Teubner, 1908.

[22] D. Hilbert. Über die Theorie der relativquadratischer Zahlkörper. *Jahresber. Deutsch. Math.-Verein.*, 1899, **6**; *Gesammelte Abhandlungen* [23], Bd. 1, 364–369.

[23] D. Hilbert. *Gesammelte Abhandlungen*, Bd. 1–3. Berlin: Springer, 1932–1935; New York: Chelsea, 1965 (reprint); Russian translation: D. Hilbert. *Selected Works*, V. 1–2. Moscow: Factorial, 1998.

[24] D. Hilbert. Mathematische Probleme. In: *Gesammelte Abhandlungen* [23], Bd. 2, 401–436.

[25] W. V. D. Hodge. *The Theory and Applications of Harmonic Integrals*. Cambridge University Press, 1941.

[26] A. I. Kostrikin et al. Vasilii Alekseevich Iskovskikh (on his 60th birthday). *Russ. Math. Surveys*, 1999, **54**(4), 863–868.

[27] S. Lang. *Fundamentals of Diophantine Geometry*. New York: Springer, 1983.

[28] S. Lang. *Introduction to Arakelov Theory*. New York: Springer, 1988.

[29] A. I. Lapin. The theory of the Shafarevich symbol. *Izv. Akad. Nauk SSSR, Ser. Mat.*, 1953, **17**, 31–50 (Russian).

[30] A. I. Lapin. On the theory of the Shafarevich symbol. *Izv. Akad. Nauk SSSR, Ser. Mat.*, 1954, **18**, 145–158 (Russian).

[31] A. I. Lapin. The general reciprocity law and a new foundation of class-field theory. *Izv. Akad. Nauk SSSR, Ser. Mat.*, 1954, **18**, 335–378 (Russian).

[32] S. Lichtenbaum. Curves over discrete valuation rings. *Amer. J. Math.*, 1968, **15**, 380–405.

[33] A. Néron. Quasi-fonctions et hauteurs sur les variétés abéliennes. *Ann. Math.*, 1965, **82**, 249–331.

[34] I. Newton. *The Mathematical Works*, Vol. I, II. Assembled with an introduction by D. T. Whiteside. New York – London: Johnson Reprint Corp., 1964; 1967.

[35] O. A. Oleinik. On Hilbert's sixteenth problem. In: *The Hilbert Problems*. Moscow: Nauka, 1969, 182–195.

[36] A. N. Parshin. Modular correspondences, heights, and isogeny of Abelian varieties. *Trudy Matem. Inst. im. Steklova*, 1973, **132**, 211–236 (Russian).

[37] A. N. Parshin. On the application of ramified coverings in the theory of Diophantine equations. *Math. USSR, Sb.*, 1990, **66**(1), 249–264.

[38] *Proceedings of the International Congress of Mathematicians* (Cambridge, MA, 1950), V. 1. Providence, RI: Amer. Math. Soc., 1952, p. 122.

[39] J.-P. Serre. Faisceaux algébriques cohérents. *Ann. Math.*, 1955, **61**, 197–278.

[40] I. R. Shafarevich. The general reciprocity law. *Matem. Sb.*, 1950, **26**, 113–146 (Russian).

[41] I. R. Shafarevich. Algebraic number fields. In: *Proceedings of the International Congress of Mathematicians* (Stockholm, 1962). Djursholm: Institut Mittag-Leffler, 1963, 163–176 (Russian).

[42] I. R. Shafarevich. *Lectures on Minimal Models and Birational Transformations of Two-Dimensional Schemes*. Bombay: Tata Institute of Fundamental Research, 1966.

[43] C. L. Siegel. *Analytic Functions of Several Complex Variables*. Lectures delivered at the Institute for Advanced Study, 1948–1949.

[44] C. L. Siegel. *Automorphic Functions of Several Complex Variables*. Moscow: Inostrannaya Literatura, 1954 (Russian translation).

[45] C. Soulé (with D. Abramovich, J.-F. Burnol, and J. Kramer). *Lectures on Arakelov Geometry*. Cambridge University Press, 1992.

[46] C. Soulé. Hermitian vector bundles on arithmetic varieties. *Proc. Symp. Pure Math.*, 1997, **62**, Part 1, 383–419.

[47] C. Soulé. On the field with one element. Talk given at the Arbeitstagung, Bonn, June 1999. Preprint IHES/M/99/55.

[48] C. Soulé, S. Bloch, H. Gillet. Non-Archimedean Arakelov theory. *J. Alg. Geom.*, 1995, **4**, 427–485.

[49] L. Szpiro, ed. Séminaire sur les pinceaux arithmétiques: la conjecture de Mordell. *Astérisque*, **127**. Paris: Soc. Math. France, 1985.

[50] V. S. Vladimirov, I. V. Volovich, E. I. Zelenov. *p-Adic Analysis and Mathematical Physics*. River Edge, NJ: World Scientific, 1994.

[51] S. V. Vostokov. Explicit form of the law of reciprocity. *Math. USSR, Izv.*, 1978, **13**, 557–588.

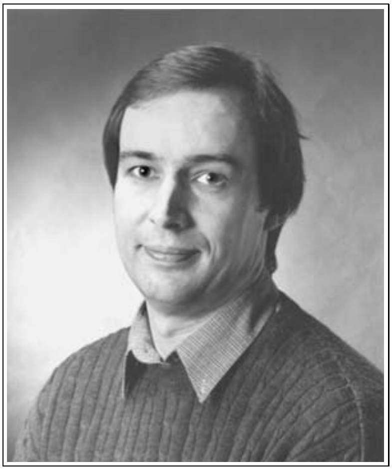
[52] A. Weil. Number-theory and algebraic geometry. In: *Proceedings of the International Congress of Mathematicians* (Cambridge, MA, 1950), Vol. 2. Providence, RI: Amer. Math. Soc., 1952, 90–110; *André Weil. Œuvres Scientifiques/Collected Papers*, Vol. 1 (1926–1951). Heidelberg–Berlin: Springer, 1980 (corrected second printing), 442–452.

[53] A. Weil. *Introduction à l'étude des variétés kählériennes*. Paris: Hermann, 1958.

[54] Yu. G. Zarkhin, A. N. Parshin. Finiteness problems in Diophantine geometry. Appendix to the Russian translation of: S. Lang. *Fundamentals of Diophantine Geometry*. Moscow: Mir, 1986, 369–448 (Russian).

[55] S. Zdravkovska, P. Duren, eds. *The Golden Years of Moscow Mathematics*. Providence, RI: Amer. Math. Soc., 1993.

[56] S. Zhang. Admissible pairing on a curve. *Invent. Math.*, 1993, **112**(1), 171–193.



A. A. Razborov

The $\mathbf{P} \stackrel{?}{=} \mathbf{NP}$-Problem: A View from the 1990s

Translated by R. Cooke

1. Introduction

The $\mathbf{P} \stackrel{?}{=} \mathbf{NP}$-problem (or, as it is sometimes informally called,[1] the *brute-force search* problem) was first stated in 1971–1972; that is, about 30 years ago. Over this comparatively short period of time it has managed to become one of the central open problems of modern mathematics, comparable in significance with the Riemann and Poincaré conjectures (see [Sma98]).

The present brief essay is devoted mainly to the history of the brute-force search problem — more precisely, to the development of various kinds of ideas leading in the end to its precise statement. For the reader who is interested in the influence that the $\mathbf{P} \stackrel{?}{=} \mathbf{NP}$-problem has had on the subsequent development of mathematics and information theory, and in the history of the numerous attempts to solve it (including papers of the present author), we note the survey article [Sip92]. Curious historical remarks on relevant research in the USSR during the period preceding the precise statement of the brute-force search problem can also be found in [Tra84] and [Sli99].

---

[1] For the etymology of this term see Section 3.3.

2. Pre-prehistory: The Theory of Algorithms and Computable Functions

Without any doubt, the most important concept at the base of the $\mathbf{P} \overset{?}{=} \mathbf{NP}$-problem (as one of the most important concepts developed in the twentieth century) is the fundamental definition of an *algorithm* in the strict mathematical sense of the word. Not having space here to recall the history of this discovery, we confine ourselves to a rapid elucidation of the state of the theory of algorithms (or, as it is sometimes called, the *theory of computable functions*) in the early 1960s, when our history actually began.

By that time the theory of algorithms and computable functions had turned into a fully formed, articulated and rapidly developing discipline. One of the cornerstones of the theory was the so-called *Church's thesis*, which asserted that all "reasonable" computing devices are equivalent. This thesis makes it possible to fix once and for all one particular type of computing device (which for historical reasons is usually taken to be a *Turing machine*). In accordance with Church's thesis,[2] results proved for Turing machines are also valid for other computing models and hence they can be interpreted as the results of *algorithms in general*. Among them, in particular, are the brilliant results on the algorithmic solvability or unsolvability of many natural (algorithmic) problems of algebra and logic. And although there remain many very important problems for which the question of algorithmic solvability is still open, on the whole the powerful general methods created in the late 1960s and the results obtained up to that time can be regarded as a comparatively successful classification of algorithmic problems in accordance with their (algorithmic) solvability (see, for example, the survey [Rab77]).

3. Prehistory: Three Components

Looking back on the period of approximately ten years from the early 1960s to the early 1970s that immediately preceded the statement of the brute-force search problem in articles of S. A. Cook [Coo71b], R. M. Karp [Kar72] and L. A. Levin [Lev73], one can distinguish (although somewhat provisionally) three main areas of research, which finally merged into one in the abovementioned papers:

- the abstract theory of computational complexity;
- algorithms for specific combinatorial problems;

[2] See also the article by Matiyasevich in the present collection.

- the concept of brute-force search and its elimination.

Until the papers of Cook, Karp, and Levin, these areas had very little in common, and so we shall discuss them separately.

3.1. The Abstract Theory of Computational Complexity

The development of the theory of computational complexity really began with the appearance of the first computers. Specialists naturally began to be interested not only in the question whether an algorithm *exists* for one algorithmic problem or another, but also in the extent to which the algorithms constructed are *effective*; that is, can be implemented from the practical point of view. It was in the 1960s that the basis of the general theory of effective algorithms was laid.

At the very beginning of this period, the central concept of a *signaling function*[3] $T_M(x)$ crystallized out. This function is the number of units of time (that is, processing time) that a Turing machine (or any other computing device — see Section 2) spends processing the input data, represented as a binary word x. In general form it is quite difficult to study this function due to its "looseness": for example, the overwhelming majority of problems can be interpreted only by using a small number of inputs x of a rather special type, and the set of these "comprehensible" inputs is unique to each problem. For this reason, and others, it is feasible to make a rational comparison of the signalers $T_M(x)$ and $T_{M'}(x)$ for two distinct algorithms M and M' only in exceptional cases.

The next very important concept on which the modern theory of computational complexity is based is the concept of *worst-case complexity*, which is defined by the following simple formula:

$$t_M(n) \stackrel{\text{def}}{=} \max_{|x| \leqslant n} T_M(x),$$

where $|x|$ is the bit length of the binary word x. In other words, worst-case complexity $t_M(n)$ *guarantees* that the computation will terminate within that time on *every* input word x, provided the bit length of the latter does not exceed n. It turns out that on the level of the functions $t_M(n)$ a completely successful construction of a rational mathematical theory for comparing the complexity of different algorithms is possible.

The concept of worst-case complexity seems to have been known in the USSR from the very beginning of the period in question (see, for example, [Yano59] or the footnote on p. 36 of [Tra64]). Its spread to the West was slightly

[3] In this brief essay we are interested only in functions that signal over time.

retarded by the circumstance that in the English language literature of the first half of the 1960s the so-called *machines with input* were encountered much more frequently (which in turn had arisen as a generalization of the extremely popular finite automata of the period), for which there does not exist any natural analog of worst-case complexity. The complexity aspects of computation on ordinary[4] Turing machines seem to have been considered for the first time in the English literature in [Hen65]; in that same paper there appeared the modern definition of worst-case complexity given above.

Now suppose we have an algorithmic problem written as the problem of computing a mapping $f\colon \{0,1\}^* \to \{0,1\}^*$ of the set of finite binary words into itself. If for every function f that is computable by some algorithm there existed an algorithm M that computes it and is optimal from the point of view of the asymptotic behavior of the worst-case complexity $t_M(n)$, then the further construction of complexity theory could be greatly simplified (namely, by declaring $t_M(n)$ for such an optimal algorithm M to be the complexity of the function f itself). The *speed-up theorem* proved in [Blu67] demonstrated convincingly that these hopes cannot be realized: there exist computable functions f for which every algorithm that computes them can be accelerated arbitrarily (for example, by a factor of "logarithm").

There is an alternative to this naive approach that is central in the theory of computational complexity, namely the concept of a *complexity class*. In general form, complexity classes are defined as follows. A set of algorithms of a particular form (which, as we shall see below, can be quite varied and not necessarily correspond to "realistic" computations) satisfying some set of complexity restrictions is fixed. Then we assign to the same complexity class all algorithmic problems for which *there exists at least one algorithm* in the given set. In this form the concept of a complexity class occurs in the early work [Rit63], but in that article only restrictions on the memory used by algorithms of a rather special form are considered.

The most important classes for the construction of complexity theory, namely

$$\mathrm{TIME}\big(t(n)\big) \stackrel{\text{def}}{=} \big\{ f \mid \exists M \; \big(M \text{ computes } f \;\&\; \forall n \; \big(t_M(n) \leqslant t(n)\big)\big)\big\},$$

first appeared in the ground-breaking paper [HS65] in the context of the machines with input mentioned above. The *hierarchy theorem*, which is the cornerstone of the modern theory of computational complexity, asserts that if $t_2(n)$ is even "slightly larger" than $t_1(n)$, then the class $\mathrm{TIME}\big(t_2(n)\big)$ contains a function

[4] That is, machines that calculate functions of finite words and output finite words; nowadays this model is the generally accepted one.

f that $\mathrm{TIME}\big(t_1(n)\big)$ does not contain. In other words, the hierarchy of complexity classes $\mathrm{TIME}\big(t(n)\big)$ is a proper hierarchy — for example, all the inclusions in the chain

$$\mathrm{TIME}(n) \subsetneqq \mathrm{TIME}(n^2) \subsetneqq \mathrm{TIME}(n^3) \subsetneqq \cdots \subsetneqq \mathrm{TIME}(2^n)$$

are strict. This theorem was proved in [HS65] for machines with input, and later extended (and slightly strengthened in the process) to the case of ordinary machines in [HS66] (see also [Yano59] and the discussion in Subsection 3.3 below).

Finally, the formulation of the complexity class **P** of problems solvable in polynomial time:

$$\mathbf{P} \stackrel{\text{def}}{=} \bigcup_{k \geqslant 0} \mathrm{TIME}(n^k),$$

and the recognition of its importance, became another remarkable discovery of complexity theory, made before the **NP**-era set in. [5] This class was first defined explicitly by A. Cobham in the paper [Cob64] (which unfortunately remained somewhat in the shade in its time, at least among specialists in complexity theory) and appears in the survey of M. Rabin [Rab66] on automata theory (see also the discussion of the papers of J. Edmonds in Subsection 3.2 below). Moving slightly ahead, we should note that at present the class **P** is regarded as the generally accepted theoretical approximation to the class of problems that are amenable to solution on existing computers. (An expanded discussion of this question can be found in the plenary talk of Cook at the International Congress of Mathematicians in 1990 in Kyoto [Coo90].) However, the direct interpretation of this thesis became possible only after the abstract theory of computational complexity merged with the theory of algorithms for specific combinatorial problems, signaling the onset of the **NP**-era (see Subsection 4.2 below). In the early papers [Cob64], [Rab66], the suggestion that the classes **P** "approximately coincide" with the class of "actually solvable problems" is still stated very cautiously and with a large number of reservations.

Such, in very general terms, was the state of computational complexity theory before the papers of Cook, Karp, and Levin. While the theoretical basis of the classification of algorithmic problems in accordance with their intrinsic complexity was essentially laid down, a severe lack of interesting problems, to

[5] We follow [Tra84] in giving this name to the period that began after the work of Cook, Karp, and Levin. While distinctly recognizing a certain pathos in this word, we would still like to point out that these papers brought about a radical change of guidelines and ideology in several very important related disciplines. That is why the use of this term here seems completely justified to me.

which the theory could be applied, began to be felt. As an illustration, we conclude this subsection with a characteristic quotation from one of the last papers [Coo71a] immediately preceding the onset of the **NP**-era.

> A final long-standing problem in the field of computational complexity is to prove that some interesting set (or function) must take a long time to compute on a reasonable general computer model. More specifically, no one can find any set not in Cobham's class $\mathscr{L}_*$, except artificial examples through use of diagonal arguments.

3.2. Algorithms for Combinatorial Problems

As it happens, a large number of interesting problems, to which complexity theory might be applied potentially, were considered simultaneously and essentially independently in the related area of construction of specific algorithms for specific combinatorial problems (usually optimization) on graphs, matroids, flow problems, problems of minimizing Boolean functions, and so forth. The most important distinguishing characteristic of such problems was that the existence of *some* algorithm was obvious for the majority of them, so that these problems were of no interest for the classical theory of computable functions considered above (Section 2). This area was also called into existence by the appearance of the first electronic computers (which stimulated interest precisely in *effective* algorithms). In the early stages of its development, people developing combinatorial algorithms empirically groped for many important concepts that later laid at the foundation of the general articulated theory.

A fully articulated interpretation of the difference between a "mechanical procedure" (= algorithm) and a "mechanical procedure short enough be practical" can be traced, for example, in the early paper [Qui52], which is devoted to the minimization of Boolean functions. In the example given in the review [Nel55] of this article, one can see clearly what lively (and quite personal in the present case) discussions can be elicited, in the absence of the precise guidelines provided by complexity theory, by the question whether some algorithm is "practical" or "purely mechanical."

This state of affairs, in which specialists in solving combinatorial problems were satisfied with intuitive pictures as to what constitutes a "practical" algorithm, lasted until the papers of J. Edmonds [Edm65b], [Edm65a]. However, the second section of [Edm65b], titled "Digression," was completely devoted to a discussion of the concept of a polynomial algorithm and (in the same cautious form as in the papers [Cob64], [Rab66] mentioned above) it contains the thesis that algorithms that execute in polynomial time can be identified with "practical" algorithms, but with certain reservations.

In any case, the overwhelming majority of problems that researchers encountered at the time did not yield to solution using "practical" algorithms in any reasonable sense of the word. In attempts to understand, if only on the intuitive level, which of these problems really are more difficult than the others, another central concept, that of *reducibility*, was formulated and later became the basis of a general theory. Suppose we have a pair of algorithmic problems $f_1, f_2\colon \{0,1\}^* \to \{0,1\}^*$ and a function g that *reduces* f_1 *to* f_2; that is, such that $\forall x\,\big(f_1(x) = f_2\big(g(x)\big)\big)$. In this way, if the reducing function g is easily computable (for example, g is in class $\mathbf{P}$, in which case the reducibility is called *polynomial*), the problem f_1 is demonstrably not more complex than the problem f_2: every effective algorithm for computing f_2 can be transformed in an obvious way into an equally effective algorithm for computing f_1.

The general concept of (polynomial) reducibility, which is one of the cornerstones of modern complexity theory, was first proposed in connection with the statement of the $\mathbf{P} \stackrel{?}{=} \mathbf{NP}$-problem in the papers of Cook, Karp, and Levin. In the period of time under discussion, only the first particular examples of reducibility were beginning to accumulate in the literature (see, for example, [Dan60], [Edm62], [Gim65]).

To summarize what has been said above, before the rise of the theory of **NP**-completeness in the early 1970s, the area of combinatorial algorithm construction saw many effective algorithms proposed for several important combinatorial problems. Also, in parallel with abstract complexity theory, the thesis was cautiously advanced that polynomial algorithms were practical and practical algorithms were polynomial. A significantly greater portion of the problems being studied did not yield to effective solution, and for these problems particular results on reducing them to one another began to appear. However, the absence of rigorous complexity conceptions of the "brute-force search" problem and polynomial reducibility made it impossible to advance in the construction of a classification of "presumably complex" combinatorial problems beyond some isolated examples.

3.3. The Concept of Brute-Force Search and its Elimination

The third basic direction of research in the prehistoric period of the $\mathbf{P} \stackrel{?}{=} \mathbf{NP}$-problem consists of research carried out mainly in the USSR, and specially focused on understanding the phenomenon of brute-force search (incidentally, the term "brute-force search problem" goes back precisely to this period, although it was understood very informally at the time). The school of S. V. Yablonskii (in whose organization and work A. A. Lyapunov also took part in the early stages) studied primarily the “cybernetic” problems of constructing complex Boolean functions, minimal circuits of different types, the so-called disjunctive normal forms of Boolean functions, and others. In the course of this activity a very important concept, according to which the difficulty of algorithmic problems is determined precisely by the “quantity” of brute-force searches of versions embedded in them, was quite distinctly worked out.

The present author began his career much later than the period in question and has encountered serious difficulties at this point in the preparation of this essay. Many ideas and even results obtained in the USSR at this time were communicated only by word of mouth, and were never published. One can form a reliable judgment as to how remarkable some of these were from one of the few written sources of that epoch that have come down to us — an abstract of a talk given by G. S. Tseitin in 1956 at two sessions of the mathematical logic seminar at Moscow University [Yano59]. According to this source, the concept of worst-case complexity and the hierarchy theorem were already known in the USSR at that time, at least in the context of normal Markov algorithms.

However, in the present essay preference is given to papers and ideas whose continuous and systematic development has led to the rise of the theory of computational complexity (and, in particular, the theory of **NP**-completeness) in its modern form. Unfortunately, the role of brilliant but isolated, unstated, and unpublished ideas in this context is very limited: as a rule, they live on only as long as they remain interesting to their immediate creators. For that reason, the author has tried to overcome the temptation to conduct a systematic investigation of the question as to exactly which part of modern complexity theory is native to the USSR, and to limit himself to the facts available in the literature.

An important milestone in this period was marked by the papers of Yablonskii [Yabl59a], [Yabl59b], in which it was proved rigorously that there is no effective algorithm in the rather special class of so-called regular algorithms for computing a complex Boolean function. In view of the significant disproportion between the value of these results and the interpretation ascribed to them, these papers received very mixed reviews (see [Tra84]). On the one hand, Yablonskii himself regarded the brute-force search problem as closed once his papers had been published, and made free use of the administrative perquisites available to him to “convince” the scholarly community of that. On the other hand, *de facto*, these papers played an important positive role in attracting the attention of other researchers to this topic.

Soviet articles of this period were characterized by an interest in particular classes of algorithms defined in accordance with the specifics of the problem under consideration. Unfortunately, due to insufficient appreciation of the importance of the general unifying concepts of computability theory and complexity theory (which manifested itself, among other places, in the neglect of the creation of a systematic theory on the textual level, as noted above) the brute-force search problem in its modern form could not be stated until the paper of Levin [Lev73], although all the other necessary components seem to have been recognized much earlier.

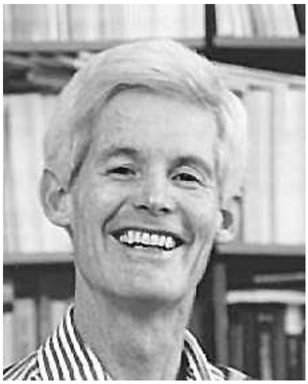
4. History: Three Sources

In the preceding section we attempted to convey the ominous feeling that came over specialists in several neighboring "paracomputer" fields in the late 1960s. It was clear that something very serious was brewing.

The "thunder rolled" in Cook's paper [Coo71b], and this thunder immediately turned into a storm in the article of R. M. Karp [Kar72]. (For obvious historical reasons, the paper [Lev73], in which many results of [Coo71b] and [Kar72] were obtained independently, stood somewhat apart.) In discussing these ground-breaking papers, we shall also try to touch briefly on the fruits that the ideas embedded in them have provided to the present.

4.1. Cook: The Complexity of Theorem-Proving Procedures

The complexity class **NP**, which consists of those problems that can be solved in polynomial time on a *nondeterministic* Turing machine, was first studied in the paper [Coo71b]. At present, this class is regarded as the strict mathematical equivalent of the informal concept of the "brute-force search" problem.



Stephen A. Cook

A *nondeterministic Turing machine* is a machine that is specially adapted for recognizing languages, that is, for solving algorithmic problems whose answer has the form YES/NO. In the course of its work the nondeterministic Turing machine may write "?" in some cell, after which its further work "divides nondeterministically" into two branches, according as the "?" is replaced by 0 or 1. As a result, a computation by a nondeterministic Turing machine is a binary tree generally of exponential size. The computation is regarded as successful for an input x (that is, $f(x) = \text{YES}$) if the answer YES is obtained along at least one branch and unsuccessful ($f(x) = \text{NO}$) otherwise.

It should be noted that, at the modern level of scientific development, nondeterministic Turing machines are a purely imaginary computational model, and no way is known to model them effectively using an analog model that actually exists at present. The most promising research in this area seems to come from the attempts at modeling nondeterministic branching using molecules of DNA (see, for example, the survey [BF98]). However, even these seem to be quite far from their logical completion.

Nondeterministic Turing machines were known in the literature even before the article [Coo71b]. Nevertheless, it was in this article that they first acquired permanent residence as full-fledged objects of investigation, and not simply a curious pathology. The very important precedent of considering a complexity class corresponding to a fictitious computational model that does not exist in the real world was thereby set. Such an approach turned out to be wonderfully fruitful for the subsequent development of complexity theory, and many of the most important achievements of later years were obtained in the context of just this scheme. To give at least a general idea of precisely what kind of achievements we are talking about, it suffices to mention *interactive proofs* (for which A. Wigderson received the Nevanlinna Prize for 1994), *probabilistically checked proofs*, and *quantum computation* (for which P. Shor received the Nevanlinna Prize for 1998). We shall return to these models in Subsection 4.2 below.

Another very important concept studied in [Coo71b] is the concept of *polynomial reducibility*. How this concept matured in the womb of the theory of combinatorial algorithms over the preceding period was told in Subsection 3.2. As for today, the modern theory of computational complexity would simply be unthinkable without the general concept of reducibility. Literally every branch of this discipline is held together by at least one concept of reducibility specific to that branch. It is the concept of reducibility that converts the theory of computational complexity from a disconnected collection of empirical facts into a rigorous and elegant mathematical theory.

As a matter of fact, there is nothing profound or amazing about the concept of reducibility itself. It is a standard working tool in the theory of computable functions. (The general methods of proving unsolvability noted in Section 2 for the most part amount precisely to the construction of suitable reducibilities.) From the mathematical point of view, reducibilities are nothing but morphisms in some obvious categories, naturally adapted for studying polynomial computations. The only reason that the concept of reducibility was able to completely change the face of the entire discipline is the empirical fact that *the number of reducibilities of some important problems to others is enormous* and, what is even more important, *there exist standard methods of constructing reducibilities* based on the consideration of imaginary computing models and the resulting complexity classes (see, for example, [GJ79], Chapters 3 and 7). Again, it was in the paper [Coo71b] that the first important step was taken in this direction: it was shown that *every* problem of class **NP** can be polynomially reduced to the perfectly definite problem of FEASIBILITY (of propositional formulas) of this class by using some general method based on the analysis of a suitable nondeterministic computation. In this way, FEASIBILITY is the "most complex" problem in the class **NP**. The now generally accepted name for such a problem is **NP**-*complete*. Such a classification scheme turned out to be wonderfully fruitful: in the overwhelming majority of cases the natural algorithmic problems turn out to be complete in some natural complexity class.

Finally, [Coo71b] contains the first example of the proof of **NP**-completeness for the *purely combinatorial* problem of ISOMORPHISM TO A SUBGRAPH. This last result served as the prelude to the next important milestone in the development of the subject that we are interested in, namely the paper of Karp [Kar72], to the discussion of which we now turn.

4.2. Karp: Reducibility of Combinatorial Problems

From my point of view, the most amazing circumstance connected with the paper [Kar72] is that practically all its terminology (and even its notation!), as well as all the concepts proposed in it, turned out to be so successful that they have taken deep root in complexity theory and are still used today, essentially without alteration. In this sense, the paper [Kar72] became the first convincing realization of the ideas presented in [Coo71b], in the form of an articulated theory.

Richard M. Karp

Karp introduced the notation **P** and **NP**. He also proposed his own version of polynomial reducibility (now known as *Karp reducibility*). Both this reducibility and the notation $\propto$ that he introduced for it soon became generally used. The abovementioned statement, that the concept of a polynomial algorithm is a satisfactory approximation to the concept of a "practical" algorithm, was made in the paper [Kar72] in a completely assured and definite form. The study of the class **P** of problems solvable by using polynomial algorithms began in the same paper.

It was in Karp's paper that the term **NP**-*complete problem* was introduced for the first time, the $\mathbf{P} \stackrel{?}{=} \mathbf{NP}$-problem was first posed, and the reason for the importance of complete problems was noted explicitly: "*It is obvious that either* *all complete languages belong to* **P**, *or none belongs to* **P**. *The former is the case if and only if* **P** = **NP**."

Passing from the methodological value of Karp's paper to the specific results proved in it, I should first mention in this connection the now generally adopted term "Karp's list of 21 complete problems." We have already said that the main reason for the success of the modern theory of reducibility of computations (and, in particular, the scheme of investigations and the well-worn brute-force search problem) consists of the empirical fact that there exist very many different reducibilities between natural algorithmic problems. As noted above in Subsection 3.2, only particular examples of reducibility were encountered in the previous literature on combinatorial algorithms (and there was no general definition). In contrast, Cook's article [Coo71b] is strictly mathematical. However, it is mostly the problem of FEASIBILITY that is considered in it, and this problem is of limited interest for those developing practical algorithms. Starting with the paper [Kar72] these areas were fated to merge. In that paper the **NP**-completeness of "a *large number* (21, to be exact) *of classical difficult computational problems arising in areas such as mathematical programming, graph theory, combinatorics, computational logic, and switching theory*" was proved. It was thereby shown convincingly that the abstract concepts of the theory of computational complexity (and, in particular, the concepts proposed in [Coo71b]) apply remarkably well to completely practical and comprehensible things, and that this striving for mathematical rigor and elegance may get along beautifully with the intuitive interpretation that comes from practical considerations, and which specialists in combinatorial algorithms would like to preserve in any case. Within a few years the list of **NP**-complete problems numbered in the thousands; at present the test for **NP**-completeness is obligatory for any brute-force search problem in which the earliest attempts to construct an effective algorithm have not led to success.

Essentially, Karp's paper laid the groundwork of the methodology that is the basis of the modern theory of computational complexity and is, at the same time, its most characteristic distinguishing feature in comparison with other areas of mathematics. In an amazing way, the most abstract, fictitious models of "computations" turn out to be *directly* responsible for the intrinsic complexity of an enormous number of specific practical problems. Among the most brilliant recent achievements in this area we may note the following:

- the application of the theory of interactive proof and probabilistic proof checking to the classification of optimization problems (see, for example, the survey [Aro94]);

- the application of quantum computing to the classification of problems lying at the basis of modern practical cryptography (such as FACTORIZATION and DISCRETE LOGARITHM [Sho97]).

4.3. Levin: Universal Brute-Force Search Problems

L. A. Levin

The paper [Lev73] contains (independently of Cook and Karp) in fully explicit form the concept of a *polynomial algorithm* — "an algorithm whose running time is comparable to the length of the input," in the terminology of [Lev73]), problems of class **NP** (a "quasi-brute-force search problem"), and **NP**-complete problem ("universal brute-force search problem"). In addition, [Lev73] contains a list of six natural **NP**-complete problems, including FEASIBILITY and ISOMORPHISM TO A SUBGRAPH, and the assertion (without proof) that they are **NP**-complete. Thus, in Levin's paper (in the extremely laconic form characteristic of the author) many very important discoveries of both [Coo71b] and [Kar72] were made independently.

In addition, [Lev73] contains an original definition of reducibility now known as *Levin reducibility*, whose potential advantages (in comparison with the more standard reducibilities of Cook and Karp) have only recently begun to be recognized (see, for example, [Aro95]). Another important result proved in [Lev73] is the theorem that there exists an optimal (in some sense) algorithm for every problem of class **NP**: the expected analog of Blum's speed-up theorem [Blu67] for brute-force search problems does not actually hold.

5. Conclusion

One of the central open problems of modern mathematics — the $\mathbf{P} \stackrel{?}{=} \mathbf{NP}$-problem — is amazingly multi-faceted, and, from my point of view, the rapid rise in its popularity over the last few decades is due precisely to that fact. On the one hand, it has an extremely simple, clearly expressible and quite elegant rigorous mathematical statement. On the other hand, the problem has a direct relation to a large number of completely realistic problems that arise in practically every area of human activity in which the methods of mathematics and information theory are used. Finally, the particular charm of the $\mathbf{P} \stackrel{?}{=} \mathbf{NP}$-problem comes from the circumstance that this is essentially a rigorous mathematical statement of the ancient philosophical question whether it is possible to eliminate, or at least decrease the amount of, brute force used (in searches).

In the present essay we have attempted to tell the history of this amazing discovery and to explain one of the reasons for its multi-facetedness by tracing the way in which several areas of research in different (but related) fields, that had previously been studied by different groups in various countries, merged into one in the $\mathbf{P} \stackrel{?}{=} \mathbf{NP}$-problem. Unfortunately, we could only touch briefly on the effect that the brute-force search problem has had, and continues to have, on the state of the modern theory of computational complexity.

The attempts to obtain the solution of the brute-force search problem, which have been unremitting since 1971, are outside the scope of this article. Although, as is known, no such solution had been obtained at the time the article was written, the particular results that have been proved in this area, along with the concepts and methods developed in the process of obtaining them, have already had an effect on complexity theory, fully comparable with the effect of the $\mathbf{P} \stackrel{?}{=} \mathbf{NP}$-problem itself (see, for example, [Sip92]).

It is difficult to say at present whether pivotally new ideas and approaches, which seem to be necessary for a definitive solution of the brute-force search problem, will be found in the foreseeable future. However, in any case, either this problem itself or the profound concepts and ideas, both those already called to life by it and those which without a doubt will follow in abundance from its hypothetical solution, is fated to become one of the most brilliant reference points in the mathematics of the twenty-first century.

6. Acknowledgments

I am deeply grateful to M. N. Vyalyi, L. A. Levin, and A. O. Slisenko for a number of useful remarks that they made after looking over a preliminary version of the text. The writing of this article would hardly have been possible without the patience and understanding that my wife Irene showed during the time when I was working on it. In addition, I am grateful to her for a number of specific stylistic corrections.

Bibliography

[Aro94] S. Arora. Probabilistic checking of proofs and hardness of approximation problems. *Electron. Colloq. Comp. Complexity*, 1994, **1**. Available via http://www.eccc.uni-trier.de/eccc-local/ECCC-Books/sanjeev_book_readme.htm

[Aro95] S. Arora. Reductions, codes, PCPs and inapproximability. In: *36th Annual IEEE Symposium on Foundations of Computer Science* (Milwaukee, WI, 1995). Los Alamitos, CA: IEEE Comp. Soc. Press, 1995, 404–413.

[BF98] R. Beigel, Bin Fu. Solving intractable problems with DNA computing. In: *Thirteenth Annual IEEE Conference on Computational Complexity* (Buffalo, NY, 1998). Los Alamitos, CA: IEEE Comp. Soc. Press, 1998, 154–168.

[Blu67] M. Blum. A machine-independent theory of the complexity of recursive functions. *J. Assoc. Comp. Machin.*, 1967, **14**, 322–336.

[Cob64] A. Cobham. The intrinsic computational difficulty of functions. In: *Proceedings of the 1964 International Congress for Logic, Methodology, and the Philosophy of Science*. Amsterdam: North-Holland, 1964, 24–30.

[Coo71a] S. A. Cook. Characterizations of pushdown machines in terms of time-bounded computers. *J. Assoc. Comp. Machin.*, 1971, **18**(1), 4–18.

[Coo71b] S. A. Cook. The complexity of theorem proving procedures. In: *Proceedings of the 3rd Annual ACM Symposium on the Theory of Computing*. 1971, 151–158.

[Coo90] S. A. Cook. Computational complexity of higher type functions. In: *Proceedings of the International Congress of Mathematicians*, Vol. 1, 2 (Kyoto, 1990). Tokyo: Math. Soc. Japan, 1991, 55–69.

[Dan60] G. B. Dantzig. On the significance of solving linear programming problems with some integer variables. *Econometrics*, 1960, **28**(1), 31–44.

[Edm62] J. Edmonds. Covers and packings in a family of sets. *Bull. Amer. Math. Soc.*, 1962, **68**, 494–499.

[Edm65a] J. Edmonds. Minimum partition of a matroid into independent sets. *J. Res. Nat. Bureau Stand.* (B), 1965, **69**, 67–72.

[Edm65b] J. Edmonds. Paths, trees, and flowers. *Canad. J. Math.*, 1965, **17**, 449–467.

[Gim65] J. F. Gimpel. A method of producing a Boolean function having an arbitrarily prescribed implicant table. *IEEE Trans. Comp.*, 1965, **14**, 485–488.

[GJ79] M. R. Garey, D. S. Johnson. *Computers and Intractability. A Guide to the Theory of* **NP**-*Completeness*. San Francisco, CA: W. H. Freeman, 1979.

[Hen65] F. C. Hennie. One-tape, off-line Turing machine computations. *Information and Control*, 1965, **8**(6), 553–578.

[HS65] J. Hartmanis, R. E. Stearns. On the computational complexity of algorithms. *Trans. Amer. Math. Soc.*, 1965, **117**, 285–306.

[HS66] F. C. Hennie, R. E. Stearns. Two-tape simulation of multitape Turing machines. *J. Assoc. Comp. Machin.*, 1966, **13**(4), 533–546.

[Kar72] R. M. Karp. Reducibility among combinatorial problems. In: *Complexity of Computer Computations* (Yorktown Heights, NY, 1972). New York: Plenum Press, 1972, 85–103.

[Lev73] L. A. Levin. Universal brute-force search problems. *Problemy Peredachi Informatsii*, 1973, **9**(3), 115–116 (Russian).

[Nel55] R. J. Nelson. Review of [Qui52]. *J. Symbolic Logic*, 1955, **20**, 105–108.

[Qui52] W. V. Quine. The problem of simplifying truth functions. *Amer. Math. Monthly*, 1952, **59**, 521–531.

[Rab66] M. Rabin. Mathematical theory of automata. In: *Proceedings of the 19th ACM Symposium in Applied Mathematics*. Providence, RI: Amer. Math. Soc., 1967, 153–175.

[Rab77] M. Rabin. Decidable theories. In: *Handbook of Mathematical Logic* (ed. J. Barwise). Amsterdam: North-Holland, 1977, Chapter C.3.

[Rit63] R. W. Ritchie. Classes of predictably computable functions. *Trans. Amer. Math. Soc.*, 1963, **106**, 139–173.

[Sho97] P. Shor. Polynomial-time algorithms for prime factorization and discrete logarithms on a quantum computer. *SIAM J. Computing*, 1997, **26**(5), 1484–1509.

[Sip92] M. Sipser. The history of the **P** versus **NP** problem. In: *Proceedings of the 24th ACM Symposium on the Theory of Computing*. 1992, 603–618.

[Sli99] A. Slissenko. Leningrad/St. Petersburg (1961–1998): From logic to complexity and further. In: *People and Ideas in Theoretical Computer Science* (ed. C. S. Calude). Singapore: Springer, 1999, 274–313.

[Sma98] S. Smale. Mathematical problems for the next century. *Math. Intelligencer*, 1998, **20**(2), 7–15.

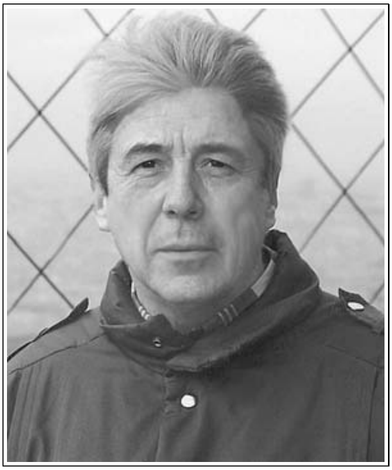
[Tra64] B. A. Trakhtenbrot. Turing computations with logarithmic retardation. *Algebra i Logika*, 1964, **3**(4) 33–48 (Russian).

[Tra84] B. A. Trakhtenbrot. A survey of Russian approaches to *perebor* (brute-force search) algorithms. *Ann. Hist. Computing*, 1984, **6**(4), 384–400.

[Yabl59a] S. V. Yablonskii. On the impossibility of eliminating the brute-force search of all functions in P_2 when solving certain problems of circuit theory. *Dokl. Akad. Nauk SSSR*, 1959, **124**(1), 44–47 (Russian).

[Yabl59b] S. V. Yablonskii. On the algorithmic difficulty of the design of minimal contact circuits. In: *Problems of Cybernetics* (ed. A. A. Lyapunov), No. 2. Moscow: Fizmatgiz, 1959, 75–121 (Russian).

[Yano59] S. A. Yanovskaya. Mathematical logic and the foundations of mathematics. In: *Mathematics in the USSR after 40 Years. 1917–1957*, Vol. 1. Moscow: Fizmatgiz, 1959, 13–120 (pp. 44–45: papers by G. S. Tseitin) (Russian).



L. P. Shil'nikov

Homoclinic Trajectories: From Poincaré to the Present

Translated by R. Cooke

In 1885, King Oscar II of Sweden decided to announce an international competition for the best mathematical research on an important scientific problem. The prize was to be awarded on the King's 60th birthday — 21 January 1889. The organization of the competition was entrusted to the editors of the Swedish mathematical journal *Acta Mathematica*. The jury was made up of three men: Mittag-Leffler, who was the editor of the journal, Weierstrass, and Hermite, the most authoritative European mathematicians. The jury proposed four topics for the competition. The first topic was a problem in celestial mechanics (the other three were purely mathematical), and it was Weierstrass who proposed it, strange as that may seem at first sight. The question involved the possibility of representing the solutions of the n-body problem, in the absence of collisions, using series in some known functions of time which converge uniformly on the entire real line. To this, the following was added: "But if the proposed problem cannot be solved within the allotted time, the prize may be awarded for a work in which another problem of mechanics is studied and completely solved in the indicated manner."

A total of 11 papers were submitted from different countries to the competition. For the sake of objectivity, and also in the tradition of the time, the papers were submitted anonymously, with identifying slogans. Two were declared the best: Poincaré's paper "On the three-body problem and the equations of dynamics" [1] and a paper by Appel "On the integrals of functions with multipliers and their application in the expansion of Abelian functions in trigonometric series." Somewhat later both were published in Vol. 13 of *Acta Mathematica* (1890), together with Hermite's report on Appel's article. As for Weierstrass' report on Poincaré's paper which was sent to Mittag-Leffler, contemporaries never saw it in print. [1] In that report, in particular, it was noted that "This paper, it is true, cannot be regarded as a solution of the problem posed for the competition, but it is so significant that I am convinced that its publication will mark the beginning of a new era in the history of celestial mechanics." We shall not give a detailed analysis of Weierstrass' review, in which he notes many merits of Poincaré's work. We shall exhibit only the portion of the review involving doubly asymptotic solutions — the main object of study in the paper:

> ... even when bodies numbering more than 2 and mutually attracting according to Newton's law or any other law move so that the distance between any two of them always remains within finite bounds, there may still exist forms of motion that we had hardly guessed up to now, and for them we do not know any suitable analytic expression (valid from $t=-\infty$ to $t=+\infty$); all that can be considered established is that they cannot be represented by trigonometric series.

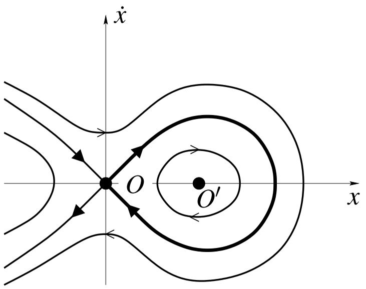


Fig. 1

What are these motions? To start with, let us consider the equation

$$\ddot{x}-x+x^2=0.$$

It is integrable, and its phase portrait has the form shown in Fig. 1. At the origin there is a saddle-point equilibrium position, while the point $O'(1,0)$ is a center. One of the separatrices of the saddle, emanating from $O(0,0)$, returns to that same point as $t\to\infty$, thereby forming a loop. For that reason a separatrix loop is a doubly asymptotic motion. All this was well known to Weierstrass, since he was essentially the founder [2] of the geometric method of constructing the phase portraits of equations of the

[1] The main reason was that the German mathematical community was very dissatisfied that the prize had been awarded to a French mathematician. A more detailed description of these events can be found in the remarkable commentary of I. B. Pogrebysskii [2].

[2] See L. I. Mandel'shtam, *Collected Works*, Vol. IV: *Lectures on Oscillations*, 1955 (Russian).

form

$$\ddot{x} + f(x) = 0.$$

Analogously one can construct doubly asymptotic solutions to saddle periodic motions. To visualize this, consider the system

$$\begin{aligned} \ddot{x} - x + x^2 &= 0, \\ \dot{\theta} &= 1, \end{aligned} \tag{1}$$

where the variable θ is periodic. The phase space of such a system is $\mathbb{R}^2 \times S^1$, where S^1 is the circle. Since we identify $\theta = 0$ and $\theta = 2\pi$, the study of such a system reduces to the study of the mapping $T : \theta = 0 \to \theta = 2\pi$ using trajectories of the system. The phase portrait of this mapping is the same as in Fig. 1, the only difference being that now $O(0,0)$ is a saddle fixed point with multipliers $e^{2\pi}$ and $e^{-2\pi}$ which has stable one-dimensional manifold W^s and an unstable one-dimensional manifold W^u, whose half-spaces coincide. However, in the case of a mapping the set of doubly asymptotic trajectories to $O(0,0)$ will now have cardinality of the continuum.

The same situation may also occur for an integrable system with two degrees of freedom, when at some level of the first integral there is a saddle periodic motion for which the stable and unstable manifolds coincide either completely or halfway. Naturally, such a possibility for the behavior of the asymptotic trajectories in integrable Hamiltonian systems was well known, even before Poincaré's paper. But Poincaré showed that in the nonintegrable cases the stable and unstable manifolds of saddle periodic motions may intersect without coinciding.

That is the situation that occurs in the study of the equation

$$\ddot{x} - x + x^2 = \mu A \sin t,$$

which can also be represented as the system

$$\begin{aligned} \ddot{x} - x + x^2 &= \mu A \sin\theta, \\ \dot{\theta} &= 1, \end{aligned}$$

which is a small perturbation of the system (1) for $0 < \mu \ll 1$. The mapping $T_\mu : \theta = 0 \to \theta = 2\pi$ still has a saddle fixed point O_μ tending to $O(0,0)$ as $\mu \to 0$. In their turn, W^s_μ and W^u_μ will be near to W^s_0 and W^u_0 (on any compact piece). But now they will intersect. The phase portrait for $\mu \neq 0$ is illustrated in Fig. 2.

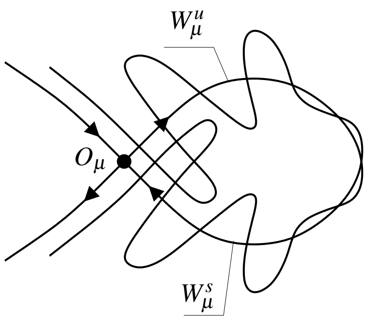


Fig. 2

The possibility of realizing such a behavior of trajectories in the three-body problem formed one of the sections of Poincaré's memoir. Later, in the third volume of his *New Methods of Celestial Mechanics* (1899) Poincaré exclaimed, "If one attempts to imagine the figure formed by these two curves and their infinitely many intersections, each of which corresponds to a doubly asymptotic solution, these intersections form something like a lattice or fabric or a net with infinitely tight loops. None of these loops can intersect itself, but it must wind around itself in a very complicated fashion in order to intersect all the other loops of the net infinitely many times. One is struck by the complexity of this figure, which I shall not even attempt to draw. Nothing gives us a better idea of the complicated nature of the three-body problem and the problems of dynamics in general, in which there is no unique integral and in which the Bohlin series diverge." Poincaré now gave such doubly asymptotic motions the name *homoclinic*. For obvious reasons, he proposed to name trajectories that are asymptotic to two different periodic motions *heteroclinic*. The genie was now out of the bottle. In the second half of the twentieth century practically all specialists in the qualitative theory of differential equations and nonlinear dynamics were to speak in the language of these concepts. As a whole, Poincaré's *New Methods of Celestial Mechanics*, which was a fuller exposition of the prize memoir, became the defining object that determined the development of the qualitative and ergodic theory for many years in the century that followed. This includes the perturbation method for finding periodic motions in systems close to integrable, the theory of integral invariants, the theory of trajectories which are stable in the sense of Poisson, theorems on reversibility, asymptotic series, and much else.

As for our basic subject — homoclinic orbits — Poincaré formally has only one general result involving them: If a two-dimensional mapping has a homoclinic orbit intersected (transversally) by the stable and unstable manifolds of a saddle fixed point, then there is also a countable set of homoclinic orbits. Poincaré never returned to the study of systems and mappings with homoclinic orbits. The question naturally arises as to why. To a certain degree the answer is that Poincaré, who started from problems of dynamics, assigned particular value to periodic motions, especially stable ones. Thus, in the first volume of the *New Methods of Celestial Mechanics* (1891) he wrote the following about such solutions: "... they are the only breach through which we can penetrate into the domain that was once considered inaccessible." Moreover, in connection with his exposition of the perturbation method, he stated the conjecture that in a nonintegrable analytic Hamiltonian system the stable periodic motions are everywhere dense on compact level surfaces of the Hamiltonian. For further understanding, the following circumstance is very important.

A year before the publication of the third volume of *New Methods of Celestial Mechanics*, that is, in 1898, the article of Hadamard, "On geodesics on surfaces of negative curvature" [3] appeared in print. The peculiarity of the geodesics in this case is that they are all unstable. This follows immediately from the fact that the equation that describes the mutual divergence of geodesics can be written in linear approximation as

$$\frac{\mathrm{d}^2 y}{\mathrm{d}s^2} + ky = 0,$$

where k is the curvature of the surface. Since $k < 0$, all geodesics have a saddle character. All two-dimensional orientable compact surfaces, except the sphere and torus, admit a metric such that the curvature is negative and constant. It follows immediately from the instability of geodesics on such surfaces that their behavior must have a complicated, tangled character. Hadamard ended his article with a question: "Does anything similar occur in the problems of dynamics, and, in particular, in celestial mechanics? If so, then the entire statement of the question of stability of planetary systems is in need of radical revision."

In this connection Poincaré offered a curious opinion, which he discussed in the article "On geodesic curves on convex surfaces" published in 1905 (see [4]). He noted that Hadamard's article was extremely interesting, but he believed that "the trajectories of the three-body problem are comparable not with geodesic curves on surfaces of negative curvature, but rather with geodesics on convex surfaces." The article of Hadamard is indeed remarkable, due to the fact that in it the method of symbolic description was used for the first time to analyze a geodesic flow. In particular, it followed from the article that the closed geodesics are everywhere dense, have homoclinic orbits and, moreover, in every neighborhood of a periodic orbit and any homoclinic trajectory of it there lies a countable set of periodic orbits. Naturally, Poincaré could not fail to notice this. But the fact that in a neighborhood of such a structure all periodic orbits are unstable was probably the reason why Poincaré was forced to think about its rather unimportant role for the problems of nonlinear dynamics.[3] Moreover, as it seems to the present author, Poincaré must have known that even in the general case (Hamiltonian, of course) the single-round periodic motions near homoclinic trajectories are of saddle type.

The subsequent development of Poincaré's ideas on the study of homoclinic structures is connected with the name of Birkhoff. Here one must first of all

[3] We note that by that time it was quite well known that the trajectories of an ideal gas have an unstable character. Boltzmann essentially used this fact to explain (granted, with insufficient rigor) the nonreversibility of the laws of macroscopic behavior. The reader is probably well aware of the polemic that arose in this connection.

note his 1935 article [5], better known as his "Papal memoir" since it was presented to a competition in honor of Pope Pius XII. In this article Birkhoff proves that for a two-dimensional area-preserving analytic diffeomorphism T having a saddle fixed point O with a homoclinic orbit Γ intersected transversally by the stable and unstable manifolds of O, in every neighborhood of the closure of a homoclinic orbit there is a countable set of single-round periodic motions of all periods, from some point on. The basic idea of the proof is not difficult to reproduce if we assume that in a neighborhood of the saddle the mapping T admits a first integral $H(x,y)$, whose existence was proved by Moser in 1956 (see [6]). Let $H(x,y)=0$ correspond to the local stable manifold (W^s) and the local unstable manifold (W^u). Then for small C the neighborhood of the saddle stratifies into invariant curves $H(x,y)=C$ in the shape of hyperbolas. Naturally, integrating them forward and backward using T and T^{-1}, we can extend these invariant curves along W^s and W^u to a larger domain. Since W^s and W^u intersect, the nearby invariant curves will have points of self-intersection, as shown in Fig. 3.

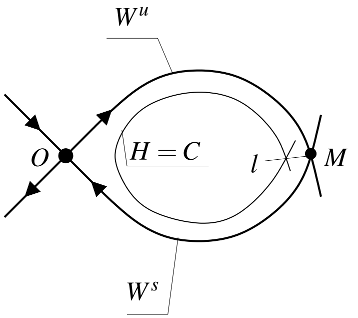


Fig. 3

The geometric locus of the points of intersection of the invariant curves will be a curve l emanating from the homoclinic point M. The trajectory of an arbitrary point P on l must remain on the invariant curve determined by the corresponding value of C. For that reason, if such a trajectory returns to l after one circuit, it must return to the same point P — that is, this trajectory is periodic. It is obvious that the number of iterations of the point $P_C \in l$ needed for its image once again to hit a neighborhood of M will tend to infinity as $C \to 0$. It now follows immediately from continuity considerations that there exists a sequence $\{C_n\}_{n=n_0}^{\infty}$, where $C_n \to 0$ as $n \to \infty$, such that $P_{C_n} = T^n P_{C_n}$. This means that in any neighborhood of a homoclinic point M there is a countable set of periodic points.

In the same memoir Birkhoff expressed the important idea of the possibility of a complete description of all trajectories in a neighborhood of a homoclinic orbit in the language of symbolic dynamics. In doing so, he emphasized the necessity of using an infinite set of symbols, referring to an analogy with the case of geodesic flow on surfaces of negative curvature.

One must note in this connection the large contribution of Birkhoff's school, in particular Morse and Hedlund, to the rise of symbolic dynamics as one of the important branches of the theory of dynamical systems. In doing so, we note that, even so, the main object of the application of symbolic dynamics for them was still only geodesic flows.

In explicit form, the problems connected with the study of nonconservative systems were raised by Andronov. One should note especially that in posing these problems he started from the problems of the theory of nonlinear oscillations, which at the time were associated mainly with the demands of theoretical radiotechnics. It quickly became clear that for those problems which are amenable to modeling using two-dimensional systems, there essentially already existed ready-made mathematical machinery in the form of Poincaré's theory of limit cycles and Lyapunov's theory of stability. This enabled Andronov at the very early stages of his work to draw the important conclusion that an adequate model of self-induced oscillations was provided by stable limit cycles. The next decisive step in that direction was taken in the paper "Structurally stable systems" by Andronov and Pontryagin [7]. This was actually the first paper that made it possible to speak of the theory of smooth dynamical systems as a full-fledged representative among the ranks of mathematical disciplines, since in this paper the object of study was formulated with complete clarity, an equivalence relation was introduced, a complete invariant was proposed, and so forth. True, in this paper the specific subject was only two-dimensional planar systems, but the concept of a structurally stable system fully preserved its meaning for the multi-dimensional case as well. For that reason, the question naturally arose as to the construction of a theory of structurally stable systems in general. Thus, for example, in the preface to the well-known book *Theory of Oscillations* by Andronov, Vitt,[4] and Khaikin it is stated that the authors propose to develop the multi-dimensional theory as well, including the case of distributed systems. As E. A. Leontovich later told the present author, "We were planning to study multi-dimensional systems." Of course, by that they meant primarily questions of structural stability and basic bifurcations. However, these plans were not fated to be realized. To a large extent that is because Andronov switched to studying nonlinear problems of automatic control. Moreover, Andronov got his principal collaborators N. N. Bautin and A. G. Maier[5] involved in this area. Nevertheless, somewhat later Maier made an in-depth study of the problem of structural stability of multi-dimensional systems. However, in this he was not successful.

[4] Because of the sad circumstances of the time, Vitt's name was taken off the list of authors in the first edition of the book.

[5] After the paper on the bifurcation of limit cycles of the separatrix loop of a saddle, Leontovich concentrated on work on books dealing with the qualitative theory and the theory of bifurcations of planar dynamical systems.

Moreover, a general opinion arose that systems with homoclinic orbits are structurally unstable.[6] But systems with a simple dynamics, which were later to be known as Morse–Smale systems, were believed to be structurally stable. On this level it is interesting that Smale adhered to this same point of view when he wrote the paper "Morse inequalities for dynamical systems" [8].

We remark, however, that the unsuccessful, but very substantial, analysis of systems with homoclinic orbits nevertheless led Maier to the solution of Birkhoff's problem on the ordinal number of central trajectories of dynamical systems, first on compact manifolds [9], and later on $\mathbb{R}^3$ [10]. Here the basic object in Maier's construction was a geodesic flow on a surface of negative curvature. Most likely, Andronov and Maier would have been well aware that such geodesic flows may be structurally stable. But Maier died in 1951 and Andronov died a year later.

The question of structural stability of two-dimensional diffeomorphisms and three-dimensional flows came to us via the address made by E. A. Leontovich at the Third Mathematical Congress in 1956 [11]:

> It should not be thought that the concept of structural stability carries over trivially to both these cases. Not to mention that in the case of a nonautonomous second-order system (depending periodically on time) this question is closely connected with the question of singular and regular trajectories, in which there is no clarity. Here there are many theoretical difficulties. Similar difficulties also exist in the case of an autonomous dynamical system in three-dimensional space. I cannot dwell on these in any detail. I can only say that the root of these difficulties is connected with a homoclinic point of a transformation of the plane into itself.

In the passage just quoted, the topic is essentially the now well-known problem of Andronov: Can structurally stable diffeomorphisms of the two-dimensional sphere have a countable set of periodic points?

In summarizing these little-known events, we shall limit ourselves to the following general remark. If it is not possible to prove the transversality of the intersection of stable and unstable manifolds of saddle periodic motions, then there is a high probability that homoclinic tangencies are occurring in the system under consideration (tangency of W^s and W^u). It is quite obvious that if homoclinic tangency occurs, one can always exhibit arbitrarily small

[6] In the early 1970s N. F. Otrokov, one of the participants in Andronov's seminar, expressed this as follows, in a conversation with the present author: "But we know that such systems are not structurally stable."

increments to the system such that new homoclinic tangencies will appear in the perturbed system, and so forth. Moreover, this is true for general one-parameter perturbations. Thus, in this sense, homoclinic tangencies behave in a way that they cannot be removed. Without a doubt, Poincaré had already noticed such a picture in constructing his geometric proof of the proposition that the number of homoclinic trajectories is infinite. Indeed, in a one-parameter family of two-dimensional diffeomorphisms T_μ containing a diffeomorphism generated by an integrable two-dimensional system with the separatrix loop of a saddle, the single-round homoclinic trajectories are structurally stable, even when $\mu = 0$ (Fig. 4), and there always exist arbitrarily small values of μ at which the diffeomorphism will have a double-round homoclinic orbit at whose points W^s and W^u are tangent (Fig. 5).

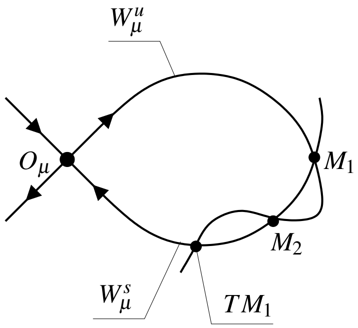


Fig. 4

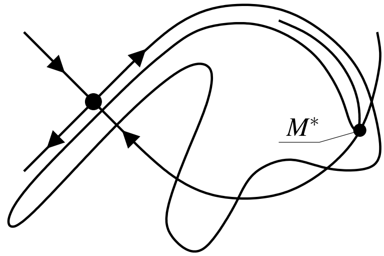


Fig. 5

In the case of nonconservative systems, for example, for the equation

$$\ddot{x} + \mu h\dot{x} - x + x^2 = \mu A \sin t,$$

where $\mu \ll 1$, even the single-round homoclinic orbits may be structurally unstable (that is, correspond to tangency of the manifolds) with a suitable connection between the parameters h and A (Fig. 6). Here, as h increases the homoclinic orbits disappear, while as h decreases two transversal orbits arise from the initial tangency. Once again, under certain relations between h and A there will exist structurally unstable double-round orbits, as well as homoclinic orbits making more than two rounds. For that reason, the desire naturally arises to assume that systems with homoclinic curves are structurally unstable.

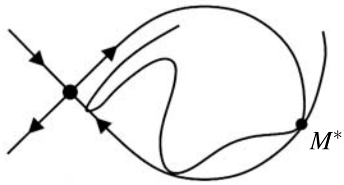


Fig. 6

In the 1950s the main source of information for the Soviet reader on homoclinic orbits and the complicated dynamic structures connected with them was the book *Qualitative Theory of Differential Equations* by Nemytskii and Stepanov [12] originally published in 1949. It presented the theorem of Birkhoff mentioned above and a number of other propositions of fundamental character on the structure of a nonwandering set in a neighborhood of a transversal homoclinic point. Since the discussion of this material preserved in full the idiosyncratic style of Birkhoff, it was very difficult to absorb. When the present author resorted to the original source, that is, the "Papal memoir," it became clear that the propositions in the book of Nemytskii and Stepanov on homoclinic orbits were nothing but statements based on the Birkhoff conjecture discussed above. On the whole, this was the reason why I called the problem of describing the structure of the set of trajectories lying entirely in a small neighborhood of a homoclinic orbit the Poincaré–Birkhoff problem.

Interest in homoclinic trajectories, and also in the intersections of the stable and unstable manifolds of saddle periodic motions, increased noticeably in physics circles in the late 1950s. The reason was that the structure of magnetic fields in ring-shaped particle accelerators of "Tokamak" type is generally not integrable. Taking account of small perturbations shows that, due to the appearance of homoclinic orbits, a charged particle may slip through the crack (formed by the stable and unstable manifolds of a saddle periodic trajectory) to the inner wall of the Tokamak. In a paper by Mel'nikov [13], an estimate is given for the splitting of the manifolds; this result is now called Mel'nikov's formula.

Progress began to occur in the 1960s. At the Kiev conference on nonlinear oscillations in 1961 Smale [14] gave an example of a diffeomorphism of the plane (the "Smale horseshoe") that behaves on a nonwandering set like a Bernoulli scheme of two symbols. As a corollary, both periodic and homoclinic points are dense in a nonwandering set. Although the proof that such a diffeomorphism is structurally stable (whose main peculiarity was (piecewise) linearity on a nonwandering set) was not given by Smale, the fact itself raised no doubts. Very soon afterward, the structural stability of the so-called U-systems (Anosov systems, in current terminology) was proved explicitly by Anosov. Among these systems, in particular, are geodesic flows on compact manifolds of negative curvature, and also hyperbolic diffeomorphisms of a torus.

Smale made the idea of the horseshoe [16] the basis of his theorem on the complex behavior of a trajectory in a small neighborhood of a transversal homoclinic point of a multi-dimensional diffeomorphism. Taking as the initial strip Π a neighborhood of a fixed point O containing a compact piece of a stable manifold together with a certain homoclinic point M, we find that for some integer m the m-th iteration T^m of the diffeomorphism acts on Π like a horseshoe (see Fig. 7). From this it followed that T^m has an invariant set in Π on which T^m is conjugate to a Bernoulli scheme of two symbols.

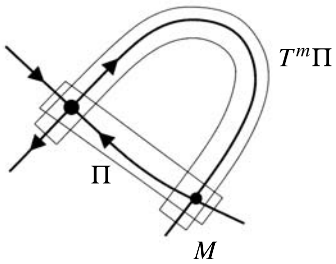


Fig. 7

Smale obtained this proposition under the assumption that T is reducible to linear form in a neighborhood of a hyperbolic point O. But that meant that the theorem was inapplicable in the case of Hamiltonian systems and symplectic mappings.[7] Moreover, and this is of fundamental importance, the "horseshoe" method did not give a complete description of all the trajectories in a neighborhood of the closure of a homoclinic trajectory, and as a consequence did not solve the Poincaré–Birkhoff problem. The present author [17] succeeded in solving this problem to the extent necessary in 1967. However, this result was preceded by the discovery of another problem with a complex dynamics.

I began studying multi-dimensional systems in the late 1950s. The first task on the agenda was the generalization of the global bifurcations of Andronov and Leontovich from the two-dimensional case to the multi-dimensional case. In theory, the solution of these problems for the case when only one periodic motion is generated, this motion being stable, might have been achieved in the framework of the methods known at the time. After studying these cases, I immediately turned to the following problem: Suppose that a three-dimensional system is given, having an equilibrium state O of saddle-focus type, that is, a state for which two of the roots of the characteristic equation are complex and lie in the left half-plane, say $\rho \pm i\omega$, where $\rho < 0$, $\omega \neq 0$, and a third root is real and positive, say $\lambda > 0$. Further suppose that one of the trajectories Γ emanating from O again tends to O as $t \to +\infty$ — that is, forms a homoclinic loop (Fig. 8).

The case when the saddle value

$$\sigma = \rho + \lambda$$

is less than zero led to the problem of the generation of a cycle, which had already been solved in the general multi-dimensional formulation. However, the case $\sigma > 0$ required special consideration. For both $\sigma < 0$ and $\sigma > 0$, the image of the area Π on the secant S under the action of the Poincaré mapping T over trajectories near Γ is a region of spiral type (Fig. 9). Here Π can be partitioned into a countable set of strips s_k, $k = k_0, k_0 + 1, \ldots, \infty$, the image of each of which

[7] As later research showed, the case of a two-dimensional symplectic mapping is an exception.

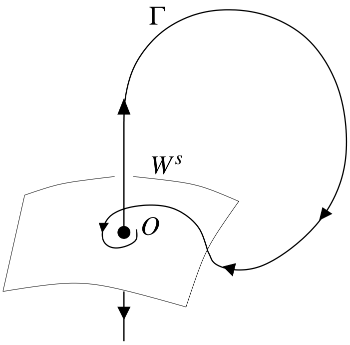


Fig. 8

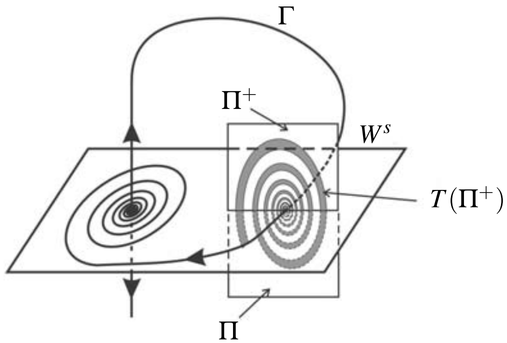
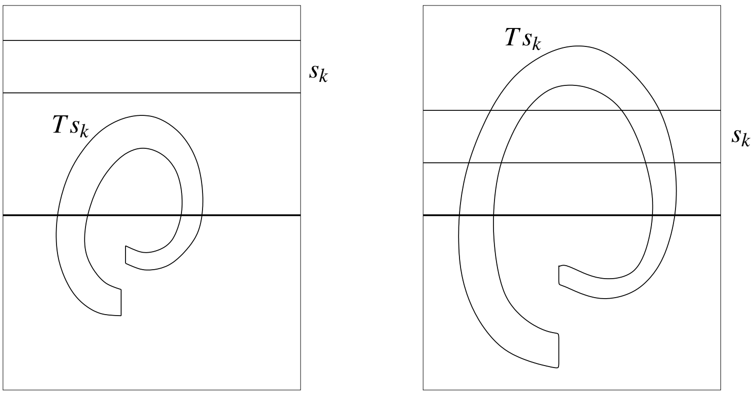


Fig. 9



$\sigma < 0$ $\sigma > 0$

Fig. 10

is one turn of the spiral, Ts_k. In the case $\sigma < 0$ we have $s_k \cap Ts_k = \varnothing$, while for $\sigma > 0$ the mapping T acts on s_k like a "Smale horseshoe" (Fig. 10). Therefore, in the case $\sigma > 0$ the Poincaré mapping has a countable set of "Smale horseshoes," and consequently also a countable set of saddle-type periodic motions [18].

Naturally, the first person I told about this was E. A. Leontovich. Her reaction was curious, as she said later, "I felt like saying that this could not be."

Immediately after this there came an understanding of dynamics in the case of a transversal homoclinic Poincaré orbit as well, and it was most convenient, at least for the author, to consider a system in the form of a flow. Whereas usually in this case the secant is chosen transversal to the periodic motion, here the secant S was chosen transversal to a stable manifold in a neighborhood of a homoclinic point. In this case, one can also construct the Poincaré mapping T and its domain of definition is a countable set of pairwise disjoint strips σ_k, $k = k_0, k_0+1, \ldots$ Here $T\sigma_k$ intersects all strips for every k (Fig. 11). In essence, this is the picture that gives a complete description identical to what Birkhoff had proposed in his "Papal memoir." However, in contrast to the case of a homoclinic loop of a saddle-focus, which is structurally unstable, the transversal homoclinic structure of Poincaré is structurally stable and consequently may admit a symbolic description with a finite number of symbols. To understand this, let us examine the possible encodings of the trajectories in a neighborhood of a homoclinic trajectory of a smooth flow.

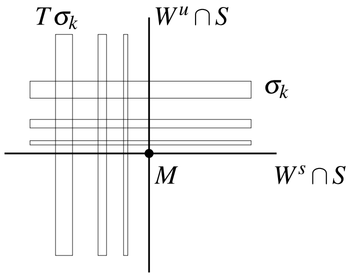


Fig. 11

Suppose the system has a periodic motion L of saddle type, that is, the multipliers do not lie on the unit circle, some of them lying inside it and the rest outside. Then L will have a stable manifold W^s and an unstable manifold W^u. Let us assume that they have a common trajectory Γ different from L (Fig. 12).

Consider a small neighborhood U of the union $L \cup \Gamma$. It will be a solid torus with a handle attached (Fig. 12). We shall encode every trajectory lying entirely in U as follows: To one of its complete circuits in the solid torus we assign the symbol 0, and to a passage through the handle, the symbol $\hat{1}$. Under this coding the infinite sequence of zeros

$$(\ldots, 0, 0, 0, \ldots)$$

will be assigned to the periodic motion L and the sequence

$$(\ldots, 0, \hat{1}, 0, \ldots)$$

to the homoclinic trajectory Γ.

Thus, to every trajectory of U there will correspond a sequence

$$(\ldots, i_{-k}, \ldots, i_0, i_1, \ldots),$$

where the symbol i_m assumes either the value 0 or the value $\hat{1}$. Here the symbol $\hat{1}$ must necessarily be followed by a rather long segment of zeros. The minimum number $\bar{n}$ of zero symbols following a $\hat{1}$ depends on the choice of neighborhood: The smaller its diameter, the larger $\bar{n}$. One can then carry out the

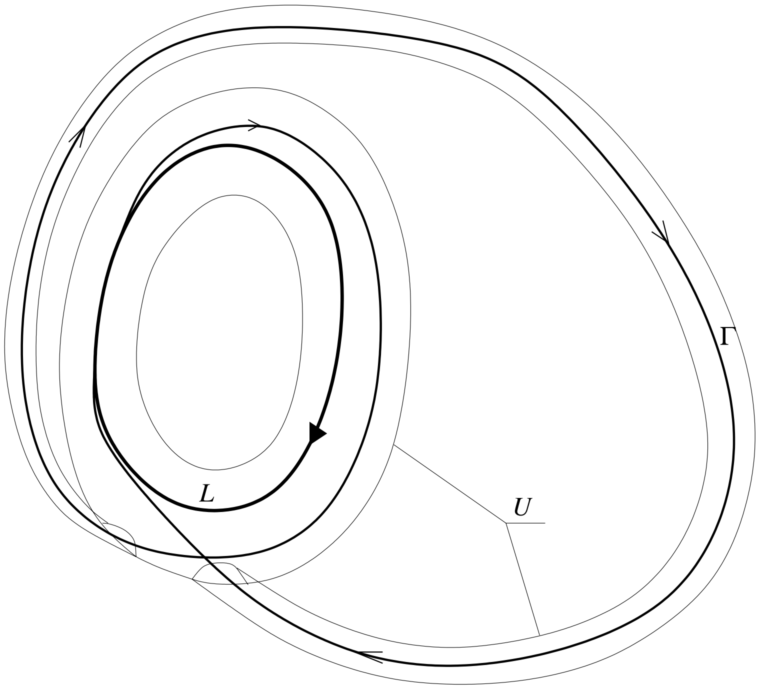


Fig. 12

following re-encoding: Set

$$1 = \{\hat{1}, \underbrace{0, \dots, 0}_{\overline{n}}\}.$$

Then the trajectories will be coded by sequences

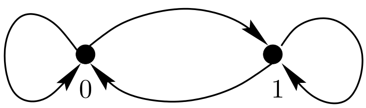


Fig. 13

$$(\dots, i_{-k}, \dots, i_0, i_1, \dots),$$

in which either 0 or 1 may follow a 1. In other words, the encoders come from the Bernoulli scheme of two symbols, whose graph is shown in Fig. 13.

Since the symbol $\hat{1}$ is necessarily followed by zeros, to a trajectory lying in U and not asymptotic to L one can assign a doubly infinite sequence

$$(\dots, n_{-k}, \dots, n_0, n_1, \dots, n_k, \dots)$$

where n_k denotes the number of zeros following the next $\hat{1}$; to a trajectory asymptotic to L in one direction (say, as $t \to -\infty$) there corresponds a singly infinite sequence

$$(n_0, n_1, \ldots, n_k, \ldots),$$

and to a trajectory homoclinic to L there corresponds a finite sequence

$$(n_0, n_1, \ldots, n_k)$$

(and the number of passages through the handle is $k+1$). Here $n_k \geqslant \overline{n}$ everywhere, where $\overline{n}$ is an integer depending on the diameter of U. Actually, this is the codification that appears if one uses the numbers of the strips σ_n as symbols.

The assertion that for each such symbolic sequence in U there exists a unique saddle trajectory with a prescribed coding is the essence of the problem. The present author succeeded in showing this under the assumption that W^s and W^u intersect transversally in a homoclinic trajectory Γ.

Strictly speaking, in the case of flows it is necessary to use the concept of a suspension. We shall not pay any attention to this point. We note only that in the restriction to the set of all trajectories lying entirely inside a special small neighborhood of the set $L \cup \gamma$ this flow is topologically equivalent to a suspension over a Bernoulli scheme of two symbols – independently of the dimension of the system. In the case of a diffeomorphism, however, that is not the case. In a neighborhood of the trajectory Γ the symbolic description is given by the Markov chain shown in Fig. 14, where the meaning of $\overline{n}$ is the same as above and p is the number of iterations needed for the image of the homoclinic point M lying in a small neighborhood of the fixed point O to hit this neighborhood again. However, two chains with different values of $\overline{n}+p$ will not be conjugate since they have different topological entropies. This means that in the case of a diffeomorphism the answer depends on the choice of neighborhood.[8]

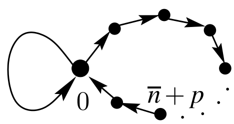


Fig. 14

The consideration of problems connected with the study of the behavior of multi-dimensional systems in the neighborhood of homoclinic trajectories required the invention of a new technique. One of its elements was the construction of local mappings in a neighborhood of periodic motions and equilibrium states in the so-called entwined form, whose essence reduced to solving not a

[8] Naturally the suspensions over such Markov chains are equivalent (see [19]).

Cauchy problem, but a special boundary-value problem.[9] Effective criteria were also given for the existence of nonperiodic trajectories (in terms of theorems on saddles and stable fixed points of operators on a countable product of Banach spaces).

In the 1960s this technique enabled the author to solve not only these problems but also the analog of the Poincaré–Birkhoff problem for the case of a homoclinic manifold of a saddle invariant torus [20], and also to explain a fundamentally new bifurcation problem — the generation of a nontrivial hyperbolic set from the bouquet of homoclinic loops of a structurally unstable equilibrium state of saddle-saddle type [21]. Subsequently the technique of entwined maps was effectively used to solve the Poincaré–Birkhoff problem for mappings in a Banach space [22] (including, in particular, the case when the unstable manifold of a saddle fixed point has infinite dimension), and also for nonautonomous systems with an arbitrary nonperiodic time dependence [23].

The natural evolution of research now led to the need to study homoclinic tangencies. The systematic study of this problem was begun by N. K. Gavrilov and the author [24] in the early 1970s. As the object of study we took three-dimensional flows having a saddle periodic motion L whose stable and unstable manifolds had quadratic contact along some homoclinic trajectory Γ. Let λ and γ be multipliers of L and $|\lambda| < 1$, $|\gamma| > 1$. Assume that the saddle value $\sigma = |\lambda\gamma| \neq 1$; here, without loss of generality, we can assume that $|\sigma| < 1$. Let U be a small neighborhood of the closure $\Gamma \cup L$ of the homoclinic trajectory and N be the set of all trajectories lying entirely in U. Depending on the signs of the multipliers and the signs of certain coefficients that characterize the way in which the stable and unstable manifolds meet Γ, the systems with homoclinic trajectories were assigned to one of three classes. In the process it was established that

1) for systems of the first class the set N is trivial: $N = \{L, \Gamma\}$;

2) for systems of the second class N is a nontrivial subset that admits a complete description in the language of symbolic dynamics of three symbols;

3) for systems of the third class N still contains nontrivial hyperbolic subsets, but the latter do not generally exhaust the set N, and there is an everywhere-dense structural instability on the bifurcation manifolds of a system of the third class.

[9] The inconvenience of constructing the mapping in direct form by solving a Cauchy problem is that the derivatives with respect to the initial conditions tend to infinity in this case as the number of iterations increases, while for a mapping in entwined form all derivatives are uniformly bounded.

Specifically, as follows from [24], in any one-parameter family of systems in which the original homoclinic tangency of the third class does not split and the quantity

$$\theta = -\frac{\ln|\lambda|}{\ln|\gamma|}$$

varies monotonically, systems having structurally unstable periodic motions are dense. Later, it was shown that in such one-parameter families systems with a countable number of stable periodic motions for $\sigma < 1$ are dense and so also are systems with secondary homoclinic tangencies [25]. (If $\sigma > 1$, systems with a countable number of unstable periodic motions are dense.)

The reason is that for systems of the third class the structure of N depends crucially on the quantity θ. For example, if $\lambda > 0$ and $\gamma > 0$, then the third class contains tangencies for which the stable and unstable manifolds behave as shown in Fig. 15. On a two-dimensional section S of L, the manifold W^u is tangent to W^s near the point $O = S \cap L$ from above, and W^s is tangent to W^u near O from the left. Just as in the case of a structurally stable homoclinic, for each trajectory of N (except O and Γ) one can construct an encoding: namely, a sequence of integers

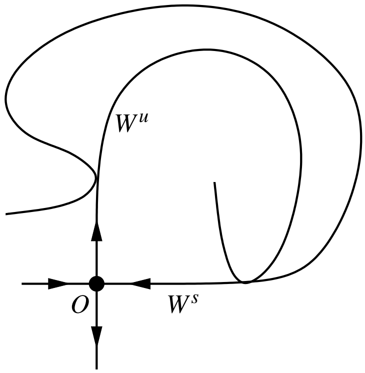


Fig. 15

$$(\ldots, n_{-k}, \ldots, n_0, n_1, \ldots, n_k, \ldots),$$

that is infinite for trajectories not lying in W^s and W^u and finite (from one end or both) for trajectories that are asymptotic to L. It was shown in [24] that for every sequence of sufficiently large integers n_k for which $n_{k+1} < n_k\theta'$ for all k there is a continuum of trajectories in U with the given encoding; and conversely if $n_{k+1} > n_k\theta''$ for at least one k, then there are no trajectories in U with this encoding. Here θ' and θ'' are certain numbers such that $1 < \theta' < \theta < \theta''$, and θ' and θ'' can be made arbitrarily close to θ at the cost of decreasing the size of the neighborhood U. A more precise description was obtained in [25], but it follows immediately from this result that for an arbitrarily small change in θ the structure of the set N varies continuously.

Later, it was shown explicitly that θ is an *invariant of* Ω*-equivalence* (that is, topological equivalence on the set of nonwandering points) for systems of the third class [26]–[28]. In other words, systems with different values of θ cannot be Ω-equivalent and consequently *arbitrarily small changes of* θ *must lead to bifurcations in a nonwandering set.*

Systems with quadratic homoclinic tangencies form bifurcation surfaces of codimension 1. Therefore, of course, one must first examine what happens when the tangency of invariant manifolds splits. Let μ be the bifurcation parameter responsible for the splitting of a separatrix and X_μ a family of systems in which μ varies monotonically. Thus X_μ intersects transversally the surface of the system with homoclinic tangency for $\mu = 0$. The following fact is of fundamental importance here: *In every transversal one-parameter family X_μ there is a sequence of intervals accumulating at $\mu = 0$ in which the parameter values corresponding to quadratic homoclinic tangencies are dense* (*and X_μ is transversal to each of the corresponding bifurcation surfaces*).

This remarkable result was proved by S. Newhouse for nonconservative two-dimensional diffeomorphisms [10] in [31]. Roughly speaking, it means that although each individual homoclinic tangency can be removed by small motions of the system, they nevertheless do not enable one to get rid of homoclinic tangencies altogether.

Sheldon Newhouse

Domains of everywhere dense structural instability in the space of C^r-smooth dynamical systems ($r \geqslant 2$) in which systems with homoclinic tangencies are dense are called *Newhouse domains* (the intervals of parameter values mentioned above along which the transversal family X_μ intersects a Newhouse domain are called *Newhouse intervals*).

The best known result [32] on the dynamics of two-dimensional mappings in Newhouse domains is that if the saddle value $\sigma = |\lambda\gamma|$ is less than 1, then in these domains, *systems having infinitely many stable* [11] *periodic motions are dense*. This assertion follows almost immediately from the density of the parameter values corresponding to homoclinic tangencies, and the earlier result [25] that in the case $\sigma < 1$ the transversal family contains a sequence (accumulating at $\mu = 0$) of intervals of values of μ corresponding to a stable periodic motion.

[10] It was extended to the multi-dimensional case in [29]; under the condition that the unstable manifold of a saddle periodic trajectory is one-dimensional, the multi-dimensional case was also done in [30].

[11] If $|\sigma| > 1$, we have infinitely many totally unstable periodic motions. For the multi-dimensional case the common property of systems in Newhouse domains is the coexistence (in infinite number) of periodic motions with stable manifolds of different dimensions, that is, with a different number of positive/negative Lyapunov exponents. (See [33] and [34], where criteria are also given for the existence of an infinite set of stable motions in the multi-dimensional case, a special case of which was studied in [32].)

But the streak of amazing phenomena in systems with homoclinic tangencies did not end there, as was shown by another series of papers of S. V. Gonchenko, D. V. Turaev, and the present author. As we have already noted, it was known that in systems with homoclinic tangency of the third class one can obtain another trajectory of homoclinic tangency by an arbitrarily small change in θ. (The original homoclinic tangency does not disappear in this process.) It turned out that this fact has a far-reaching consequences. To be specific, by an application of localized small smooth increments one can establish that, *in the set of systems with homoclinic tangencies of the third class, all systems which have infinitely many saddle-type periodic trajectories, with homoclinic tangencies also of the third class, are dense.* [12] This means that such systems have *infinitely many independent continuous invariants* (*moduli*) *of* Ω*-equivalence*. (For each tangency of the third class taken individually the corresponding quantity θ is such an invariant; we do not assert that the totality of all these quantities is a complete invariant — other invariants are also possible, for example, the quantity τ of [27].)

The construction of a homoclinic tangency with an infinite set of trajectories served as the basic element in the proof of the following proposition [35]–[37]:

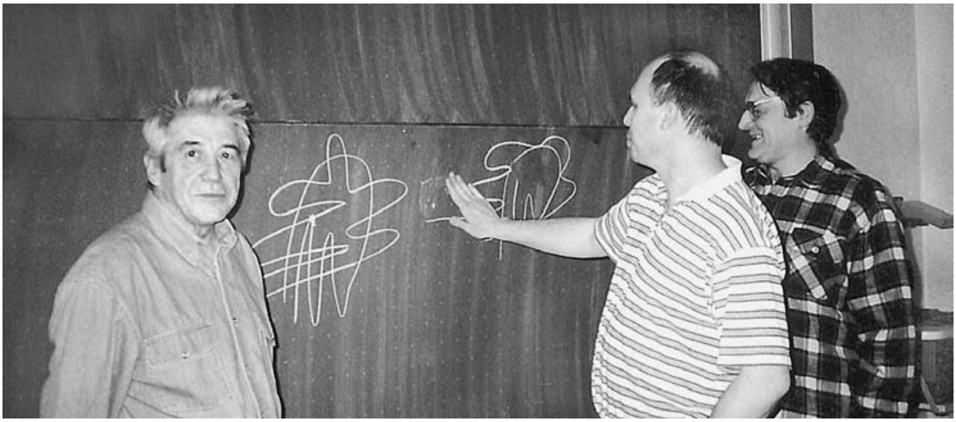
In the set of systems with quadratic homoclinic tangency of the third class, systems having homoclinic tangency of every arbitrarily large order and systems having infinitely many structurally unstable periodic trajectories of every order of degeneracy are dense.

The latter objects are periodic trajectories with a single multiplier equal to 1 or -1, and an arbitrarily large number of zero Lyapunov quantities — the successive coefficients of the nonlinear terms in the normal form of the Poincaré mapping on a central manifold. Thus, in the case of one multiplier equal to 1, for a periodic trajectory of degeneracy order k the Poincaré mapping on a central manifold has the form

$$\bar{x} = x + l_{k+1}x^{k+1} + o(x^{k+1}).$$

The construction of a complete bifurcation diagram for such a periodic motion requires exactly k parameters. Since we can obtain structurally unstable periodic trajectories of every degeneracy order k, this means that for systems with homoclinic tangencies a complete description of the dynamics in the framework of any finite-parameter family is *theoretically impossible*.

[12] Here, and throughout this article, we mean density in the C^r-topology, for an arbitrary finite r. If we consider C^∞-systems, density in the C^r-topology for an arbitrary finite r means, by definition, density in the C^∞-topology.



L. P. Shil'nikov, D. V. Turaev, and S. V. Gonchenko

This discouraging result is all the more important because the systems of the third class are dense in Newhouse domains, that is, we have entire domains in the space of smooth dynamical systems for which a complete description of the dynamics (in particular, a complete description of the bifurcations of periodic motions) is unattainable.

We generalized the results discussed here to the multi-dimensional case almost immediately [33], [34]; an analogous classification of systems with quadratic tangency was given, the basic invariants (moduli) of Ω-equivalence were exhibited (and in the case of complex leading multipliers $\lambda e^{i\varphi}$, $\gamma e^{i\psi}$ they turned out also to be the arguments φ and ψ), the results of Newhouse were generalized, and the density of multi-dimensional systems with infinitely degenerate periodic and homoclinic trajectories in Newhouse domains was established. Thus, the general conclusion of the theoretical impossibility of a complete description is valid for the multi-dimensional case as well.

All this is relevant to the study of specific dynamical models, since homoclinic tangencies, and consequently also Newhouse domains are being discovered in practically all known families of systems with a complex dynamics, from the small periodic perturbations of integrable systems discussed above to such popular models as the Hénon mapping and the Chua circuit, and also in the transition to chaos through destruction of quasi-periodic motions and after period doubling.

Popular objects among the systems with the complex dynamics that occur in very many cases, are the systems with homoclinic loop of saddle-focus type, discussed above. Such systems in general also form bifurcation sets of codimension 1 and are to a certain degree analogous to systems of the third class

[38]–[40]. Thus, in these bifurcation sets systems having homoclinic tangency are also dense. Consequently, nearby systems will have Newhouse domains, and degeneracies of arbitrarily high order will also occur.

Moreover, as was recently established by Turaev and the author, there can also exist strange attractors (we called them *wild*) containing a saddle-focus. Naturally, for this reason the attractor has a spiral character. All the trajectories in wild spiral attractors are unstable, as one expects for genuine strange attractors. Moreover this property is preserved also for small smooth perturbations. The fact that wild spiral attractors contain equilibrium states relates them to attractors of Lorenz type. For example, both types of attractors are structurally unstable: either they have homoclinic loops of an equilibrium state, or that can be arranged by small perturbations of the system. However, whereas attractors of Lorenz type have dimension 2, the wild spirals have dimension 3. Wild spiral attractors have been constructed in one-parameter families of systems on $\mathbb{R}^n$, where $n \geqslant 4$. When this is done, the domain of variation of the parameters contains Newhouse intervals, with all the consequences that entails [41]. Nevertheless, such attractors are completely natural objects of nonlinear dynamics.

In his report on Poincaré's competition paper, Weierstrass wrote that the results obtained in it destroy many illusions about the dynamics of Hamiltonian systems. This was the essential reason why qualitative methods became the basic methods of nonlinear dynamics. We now see that it is also necessary to get rid of the illusion that it is possible to give a complete qualitative analysis of dynamic systems. In both the one case and the other, the crisis was caused by Poincaré's homoclinic orbits.

Bibliography

[1] H. Poincaré. Sur le problème des trois corps et les équations de la dynamique. *Acta Mathematica*, 1890, **13**, 1–271.

[2] H. Poincaré. *Les méthodes nouvelles de la mécanique céleste*. Tt. 1, 2, 3. Paris: Gauthier-Villars, 1892, 1892, 1899. English translation: *New Methods of Celestial Mechanics* (ed. D. Goroff). Woodbury, NY: American Institute of Physics, 1993.

[3] J. Hadamard. Les surfaces à courbures opposées et leur lignes géodésiques. *J. Math., Sér. 5*, 1898, **4**.

[4] H. Poincaré. Sur les lignes géodésiques des surfaces convexes. *Trans. Amer. Math. Soc.*, 1905, **6**, 237–274.

[5] G. D. Birkhoff. Nouvelles recherches sur les systèmes dynamiques. *Mem. Pont. Acad. Sci. Nov. Lyncaei*, 1935, **53**, 85–216.

[6] J. Moser. The analytic invariants of an area-preserving mapping near a hyperbolic fixed point. *Commun. Pure Appl. Math.*, 1956, **9**, 673–695.

[7] A. A. Andronov, L. S. Pontryagin. Structurally stable systems. *Dokl. Akad. Nauk SSSR*, 1937, **14**, 247–251 (Russian).

[8] S. Smale. Morse inequalities for a dynamical system. *Bull. Amer. Math. Soc.*, 1960, **66**, 43–49.

[9] A. G. Maier. On the ordinal number of central trajectories. *Dokl. Akad. Nauk SSSR*, 1948, **59**(8), 1393–1396 (Russian).

[10] A. G. Maier. On the central trajectories in Birkhoff's problem. *Matem. Sb.*, 1950, **26**(2), 265–290.

[11] E. A. Leontovich. Some mathematical papers of the Gor'kii school of A. A. Andronov. In: *Proceedings of the Third Mathematical Congress*, Vol. 3. Moscow: USSR Academy of Sciences, 1958 (Russian).

[12] V. V. Nemytskii, V. V. Stepanov. *Qualitative Theory of Differential Equations*. Princeton University Press, 1960.

[13] V. K. Mel'nikov. On the stability of a center under time-periodic perturbations. *Trudy Mosk. Matem. Obshch.*, 1963, **12**, 3–52 (Russian).

[14] S. Smale. A structurally stable differentiable homeomorphism with an infinite number of periodic points. In: *Proceedings of the International Symposium on Nonlinear Oscillations*, Vol. 2. Kiev, 1963, 365–366.

[15] D. V. Anosov. Geodesic flows on closed Riemann manifolds with negative curvature. *Proc. Steklov Inst. Math.*, 1967, **90**, 235 pp.

[16] S. Smale. Diffeomorphisms with many periodic points. In: *Differential and Combinatorial Topology*. Princeton University Press, 1965, 63–80 (Princeton Math. Ser., **24**).

[17] L. P. Shil'nikov. On a problem of Poincaré–Birkhoff. *Math. USSR, Sb.*, 1967, **3**, 353–371.

[18] L. P. Shil'nikov. A case of the existence of a countable number of periodic motions. *Sov. Math. Dokl.*, 1965, **6**, 163–166.

[19] V. S. Afraimovich, L. P. Shil'nikov. On the singularity sets of Morse–Smale systems. *Trudy Mosk. Matem. Obshch.*, 1973, **28**, 181–214 (Russian).

[20] L. P. Shil'nikov. Structure of a neighborhood of a homoclinic tube of an invariant torus. *Sov. Math. Dokl.*, 1968, **9**, 624–628.

[21] L. P. Shil'nikov. On a new type of bifurcation of multidimensional dynamical systems. *Sov. Math. Dokl.*, 1969, **10**, 1368–1371.

[22] L. M. Lerman, L. P. Shil'nikov. Homoclinic structures in infinite-dimensional systems. *Sib. Math. J.*, 1988, **29**(3), 408–417.

[23] L. M. Lerman, L. P. Shil'nikov. Homoclinic structures in nonautonomous systems: Nonautonomous chaos. *Chaos*, 1992, **2**, 447–454.

[24] N. K. Gavrilov, L. P. Shil'nikov. On three-dimensional dynamical systems close to systems with a structurally unstable homoclinic curve, I; II. *Math. USSR, Sb.*, 1972, **17**, 467–485; 1973, **19**, 139–156.

[25] S. V. Gonchenko. Nontrivial sets with structurally unstable homoclinic curve. In: *Methods of the Qualitative Theory of Differential Equations. An Intercollegiate Collection* (ed. E. A. Leontovich). Gor'kii State University, 1984, 89–102 (Russian).

[26] S. V. Gonchenko. Moduli of systems with structurally unstable homoclinic trajectories (the case of diffeomorphisms and vector fields). In: *Methods of the Qualitative Theory of Differential Equations. An Intercollegiate Collection* (ed. L. P. Shil'nikov). Gor'kii State University, 1989, 34–49 (Russian).

[27] S. V. Gonchenko, L. P. Shil'nikov. Invariants of Ω-conjugacy of diffeomorphisms with a structurally unstable homoclinic trajectory. *Ukrain. Math. J.*, 1990, **42**(2), 134–140.

[28] S. V. Gonchenko, L. P. Shil'nikov. Moduli of systems with a structurally unstable homoclinic Poincaré curve. *Russ. Acad. Sci. Izv. Math.*, 1992, **41**(3), 417–445.

[29] S. V. Gonchenko, D. V. Turaev, L. P. Shil'nikov. On the existence of Newhouse domains in a neighborhood of systems with a structurally unstable homoclinic Poincaré curve (multidimensional case). *Russ. Acad. Sci. Dokl. Math.*, 1993, **47**(2), 268–273.

[30] J. Palis, M. Viana. High dimension diffeomorphisms displaying infinitely many periodic attractors. *Ann. Math.*, 1994, **140**, 207–250.

[31] S. Newhouse. The abundance of wild hyperbolic sets and non-smooth stable sets for diffeomorphisms. *Publ. Math. IHES*, 1979, **50**, 101–151.

[32] S. Newhouse. Diffeomorphisms with infinitely many sinks. *Topology*, 1974, **13**, 9–18.

[33] S. V. Gonchenko, D. V. Turaev, L. P. Shil'nikov. Dynamical phenomena in multidimensional systems with a structurally unstable homoclinic Poincaré curve. *Russ. Acad. Sci. Dokl. Math.*, 1993, **47**(3), 410–415.

[34] S. V. Gonchenko, L. P. Shilnikov, D. V. Turaev. Dynamical phenomena in systems with structurally unstable Poincaré homoclinic orbits. *Chaos*, 1996, **6**(1), 23–52.

[35] S. V. Gonchenko, D. V. Turaev, L. P. Shil'nikov. Models with a structurally unstable homoclinic Poincaré curve. *Sov. Math. Dokl.*, 1992, **44**(2), 422–426.

[36] S. V. Gonchenko, L. P. Shil'nikov, D. V. Turaev. On models with nonrough Poincaré homoclinic curves. *Physica D*, 1993, **62**(1–4), 1–14.

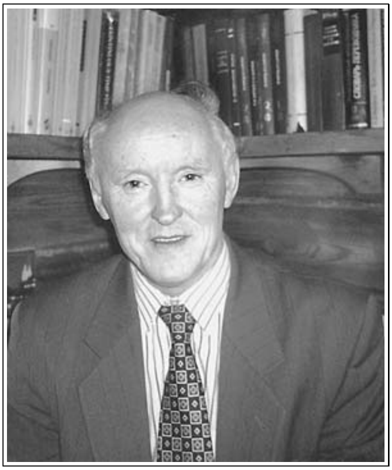
[37] S. V. Gonchenko, D. V. Turaev, L. P. Shil'nikov. Homoclinic tangencies of arbitrary order in Newhouse domains. In: *Dynamical Systems. Proceedings of the Conference Dedicated to the 90th Birthday of L. S. Pontryagin*. Moscow: VINITI, 1999, 69–128 (*Sovr. Probl. Prilozh.*, **67**) (Russian).

[38] L. P. Shil'nikov. On the question of the structure of an expanded neighborhood of a structurally stable equilibrium state of saddle-focus type. *Matem. Sb.*, 1970, **81**, 92–103 (Russian).

[39] I. M. Ovsyannikov, L. P. Shil'nikov. On systems with homoclinic loops of saddle-focus type. *Matem. Sb.*, 1986, **130**, 552–570 (Russian).

[40] I. M. Ovsyannikov, L. P. Shil'nikov. Systems with a homoclinic curve of a multidimensional saddle-focus, and spiral chaos. *Math. USSR, Sb.*, 1992, **73**(2), 415–443.

[41] D. V. Turaev, L. P. Shil'nikov. An example of a wild strange attractor. *Sb. Math.*, 1998, **189**(1–2), 291–314.



A. N. Shiryaev

From "Disorder" to Nonlinear Filtering and Martingale Theory

Translated by R. Cooke

At any given time there is only a thin layer
separating what is trivial from what is impossibly difficult.
It is in that layer that mathematical discoveries are made . . .
Diary entry of A. N. Kolmogorov
14 September 1943

1. The "Disorder" Problem – Prehistory

1.1. Sometime near the end of 1958 Andrei Nikolaevich Kolmogorov held a conversation with Yurii Borisovich Kobzarev (a member of the Academy of Sciences and the founder of the Soviet school of radiolocation) concerning a number of theoretical questions involving methods of detecting signals entering a locator amid strong noise. One of the questions that interested Kobzarev involved correctly posing the problem of rapid detection of the reflected signal arriving from a "target" appearing at a "random" time, not known in advance.

At that time the topic of *separating signals from a mixture with noise* was both very popular and quite well studied. However, the original assumptions had, as a rule, the following defect – it was originally assumed that there are at most two hypotheses.

- H_∞ — only noise N occurs the whole time, that is, the signal appears at time $\theta = \infty$;
- H_0 — from the very beginning, that is, at $\theta = 0$, a mixture $S+N$ of signal S and noise N arrives.

In other words, the observed process $X = (X_t)_{t \geqslant 0}$ has the following structure at all $t \geqslant 0$:

$$X_t = \begin{cases} N_t & \text{under the hypothesis } H_\infty, \\ N_t + S_t & \text{under the hypothesis } H_0. \end{cases} \tag{1}$$

From the statistical point of view, the question as to which hypothesis is to be preferred on the basis of the observations $X = (X_t)_{t \leqslant T}$ is solved, as a rule, from the values of the *likelihood ratio*

$$L_T = \frac{\mathrm{d}P_0}{\mathrm{d}P_\infty}(X,T), \tag{2}$$

defined as the Radon–Nikodým derivative of the probability distribution $P_0 = \mathrm{Law}(X_t; t \leqslant T \big| H_0)$ with respect to the probability distribution $P_\infty = \mathrm{Law}(X_t; t \leqslant T \big| H_\infty)$.

Thus, for example, if it is required to minimize the sum $\alpha + \beta$ of the errors of first and second kind (α is the probability of adopting the hypothesis H_∞ when hypothesis H_0 holds and β the probabilty of adopting the hypothesis H_0 assuming that H_∞ is actually true), then by the *Neyman–Pearson test*, the optimal method is to adopt the hypothesis H_0 when $L_T \geqslant K$ and H_∞ if $L_T < K$. (Here K is a certain constant determined from the distributions P_0 and P_∞.)

The *Wald sequential method*, which minimizes the average duration of observations *simultaneously* for both hypotheses H_0 and H_∞ under given errors of first and second kind, has also attained widespread usage. (This optimality property of the Wald method, which is based on observing the process L_T, $T \geqslant 0$, holds at least for homogeneous processes with independent increments relative to the probability distributions of the process X under each of the hypotheses H_0 and H_∞.)

These optimality properties of both the Neyman–Pearson method and the Wald method in the problem of distinguishing the two hypotheses carried over automatically to the case when the "target" may appear at a certain time θ different from 0 or ∞. (The time $\theta = 0$ means, as stated above, the presence of a "target" from the very beginning of the observation, and the value $\theta = \infty$ is interpreted as the complete absence of a target.)

It is this circumstance that caused the uneasiness of Kobzarev, who wished to have a precise *mathematical statement of the problem of most rapid dectection* *of a randomly appearing “target”* and the realization of the sense in which the traditional methods of detection, based on the Neyman–Pearson and Wald methods, are close to optimal.

1.2. At that time (from September 1957) I was already working in the Department of Probability at the Mathematical Institute of the Academy of Sciences, directed by Andrei Nikolaevich Kolmogorov, where Viktor Leonov and I were enthusiastically studying the nonlinear analysis of random processes and, in particular, the formulas connecting moments and semi-invariants, higher-order spectral characteristics, and limit theorems for stationary processes under conditions imposed on the behavior of the semi-invariants.

When he invited me to his home in Komarovka, Andrei Nikolaevich said that one ought to study thoroughly the problems of distinguishing signals from a mixture with noise, studying as a preliminary the literature on methods of detection and radio location. He also planned a number of meetings with radio engineers connected with Yu. B. Kobzarev in order to gain an understanding of what was realistically expected of us.

Many meetings and consultations were held, in which, unfortunately, the topic was mostly the technical side of the matter, the spectral characteristics of signals and noise and the need to take all this into account somehow. However, we never reached any precise mathematical statements, having gotten tangled up in new requirements that arose of taking account, for example, of the fact that after an “alarm” was sounded and the “target” appeared, the observation process did not terminate, but rather began anew. In other words, it was somehow assumed that realistic problems of rapid detection are not single-stage (as in the Neyman–Pearson and Wald schemes) but multi-stage.

As a result of all this preliminary but unquestionably useful work, it became clear that it was impossible to take account of all the stated desiderata primarily involving the model that describes the nature of the noise and signal; and that, in a certain sense, one must concentrate on the more “difficult” model from the point of view of detection with a description of the statistical properties of the noise and signal and also the statistics of the appearance of the “target.”

We had many conversations with Andrei Nikolaevich, in which he emphasized that success could be attained only by turning to simple, “crude,” but representative models that “epitomized” in some sense the realistic models we had heard of in conversations with radio engineers; and then the formulation of the actual problem of rapid detection began to be sketched. The course of our reasoning here was as follows.

We would first study the *single-stage* problem of detecting a “target” that appeared at a certain time $\theta \in [0,\infty]$.

What is θ? Is it simply an *unknown parameter*, or is it a *random variable* with some probability distribution?

Here we proceeded along the route of assuming that θ is a *random* variable with an *exponential* distribution, $P(\theta > t) = e^{-\lambda t}$, where $\lambda > 0$ is some known constant to start with.

At first glance the very assumption of the "randomness" of the time of appearance of the "target," and even more the assumption of the exponential nature of the probability distribution of θ may seem highly artificial. However, the following circumstances justify these assumptions.

First, if we use the notation $\theta_t = I(t \geqslant \theta)$, we obtain a well-studied *homogeneous Markov process* with a single switch $0 \to 1$ at time θ.

Second,

$$P(\theta > t+s | \theta > s) = P(\theta > t) = e^{-\lambda t}.$$

In other words, if it is known that the "signal" has not appeared up to time s, the statistics of its appearance will be the same as at the time when the observation began, and therefore everything virtually starts over again.

Finally, if $\lambda \to 0$ (and hence $E\theta \to \infty$), the conditional probability is

$$P\big(\theta \in (a,b) \big| \theta \in (A,B)\big) \to \frac{|b-a|}{|B-A|},$$

where $A < a < b < B$. In other words, the *limiting conditional distribution* is conditionally uniform, that is, if we know that the "signal" may appear during the interval (A,B), then within that interval it appears uniformly.

Such an assumption seems completely natural if nothing is known about the time of appearance of the "signal," while at the same time one can foresee that the conditionally uniform nature of the distribution of that time is the most "difficult" case for detection.

Subsequently, the time of appearance of the "target" in our work (see, for example, [4], [6]) came to be called the time of appearance of the *disorder*, partly because in public print it was necessary somehow to "disguise" a certain applied direction of these radio-location problems. (Thus, the paper [4] was titled "On the detection of disorder in a manufacturing process.")

The term "disorder" (*razladka* in Russian) caught on and is now used in many papers devoted to sequential methods of detecting randomly appearing signals. The Western term *change point*, which has also become firmly entrenched in the statistical literature, usually refers to the circle of problems in which the decision as to the time of change of probability characteristics is adopted a posteriori, retrospectively, that is, taking account of the entire mass of available data at once (without the right to accept new data, as occurs in sequential methods).

Besides the solution of the problem of the probabilistic-statistical nature of the *time* of appearance of the "disorder," it was also necessary to pay attention to at least the idealized probablistic-statistical nature of the incoming data up to the time of "disorder" (that is, when there was only "noise") and after that time (interpreted as the state "signal + noise").

In the early stages we limited ourselves to a model comprehensible to radio engineers, which, using their language, can be stated as follows:

- the noise is modeled as "white Gaussian noise with mean value zero,"
- signal + noise is described as "white Gaussian noise, but with a nonzero drift."

If we use the terminology, which was adopted in the radiotechnical literature of the 1950s and 1960s, we can say that the incoming signal $\dot{X}$ at the entrance to the locator has the form

$$\dot{X}_t = \begin{cases} \delta_t, & t < \theta, \\ \delta_t + r, & t \geqslant \theta, \end{cases} \tag{3}$$

where δ_t is "white Gaussian noise with zero mean" and r is some nonzero constant.

Of course, it is necessary to give a rigorous mathematical meaning to this notation, and that is achieved by passing to the integral form in (3). To be specific, we shall assume that the process being observed (that is, the process at the entrance to the locator) $X = (X_t)_{t \geqslant 0}$ has the following structure:

$$X_t = r \int_0^t \theta_s \, \mathrm{d}s + \sigma B_t, \tag{4}$$

where $\theta_s = I(s > \theta)$ and $B = (B_t)_{t \geqslant 0}$ is a standard Brownian motion (Wiener process).

It is customary to write relation (4) in differential form as follows:

$$\mathrm{d}X_t = rI(t > \theta)\, \mathrm{d}t + \sigma \, \mathrm{d}B_t, \quad X_0 = 0. \tag{5}$$

It was this model, together with the assumption of exponential distribution of the time of appearance of the "disorder" that was adopted in our first papers on rapid detection ([1]–[4]).

We must now state the problem of detecting the time of appearance of the "disorder" itself taking account of the desiderata that the radio engineers had stated on the descriptive level.

Since the fact of the appearance of the "disorder" was to be incorporated as the raising of an "alarm" signal, it was natural to introduce into consideration a certain (random) time — call it τ — which was to be identified with the time when an "alarm" was declared. Naturally the decision, whether it is worthwhile to raise an alarm at time τ, was to be based only on the information available up to that time. We thus arrive at the situation in which the time of the "alarm" τ was to be a *stopping time* (Markov time), that is, such that for each $t \geqslant 0$ the event is

$$\{\tau \geqslant t\} \in \mathscr{F}_t^X,$$

where $\mathscr{F}_t^X$ is the σ-algebra of events generated by the values X_s, $s \leqslant t$, of the observed process X.

1.3. Having the times θ and τ, we consider the following two events: $\{\tau < \theta\}$ and $\{\tau \geqslant \theta\}$. The first event $\{\tau < \theta\}$ is connected with a false alarm, and if the probability $P(\tau < \theta)$ is interpreted as the *probability of a false alarm*, it would be desirable that this quantity be small.

With the second event $\{\tau \geqslant \theta\}$, in which the alarm signal is given "correctly," that is, after a "disorder" appears, it is natural to connect the *mean time of delay* $E(\tau-\theta)^+$ $(= E_{\max}(\tau-\theta,0))$ or the *conditional average time of delay* $E(\tau-\theta \,|\, \tau \geqslant \theta)$. (It is clear that $E(\tau-\theta)^+ = E(\tau-\theta \,|\, \tau \geqslant \theta)P(\tau \geqslant \theta)$.)

One would like to make both the quantity $E(\tau-\theta \,|\, \tau \geqslant \theta)$ or $E(\tau-\theta)^+$ and the quantity $P(\tau < \theta)$ small *simultaneously* by choosing a suitable stopping time τ. It is clear, however, that it is not possible to achieve the *simultaneous* minimization of these detection characteristics. For that reason the following (*conditional-extremal*) version of the problem of rapid detection has been proposed.

Version A. Let α be a constant, $\alpha \in (0,1)$, and $\mathfrak{M}_\alpha$ the class of stopping times τ for which the probability of a false alarm satisfies the inequality $P(\tau < \theta) \leqslant \alpha$. It is required to find the quantity

$$\mathbb{A}(\alpha) = \inf_{\tau \in \mathfrak{M}_\alpha} E(\tau-\theta \,|\, \tau \geqslant \theta) \tag{6}$$

and the (optimal) time τ_α^* for which $E(\tau_\alpha^*-\theta \,|\, \tau_\alpha^* \geqslant \theta) = \mathbb{A}(\alpha)$.

Using Lagrange multipliers, one can obtain the solution of this problem from the solution of the following "weighted" problem:

Version B. Find, for each $c > 0$, the quantity

$$\mathbb{B}(c) = \inf_{\tau \in \mathfrak{M}} \{P(\tau < \theta) + cE(\tau - \theta \,|\, \tau \geqslant \theta)P(\tau \geqslant \theta)\} \,, \tag{7}$$

where $\mathfrak{M}$ is the class of all finite stopping times; also find the optimal stopping time $\tau^*(c)$ at which the value $\mathbb{B}(c)$ is attained.

In Section 3 below we shall consider other versions of the "disorder" problem. At this point we turn to the solution of this problem in the stated versions.

2. The "Disorder" Problem — its Solution in Versions A and B

2.1. We begin by making more precise the nature of the distribution of the time of appearance of the "disorder" θ. It is reasonable to assume that

$$P(\theta = 0) = \pi \quad \text{with} \quad \pi \in [0,1].$$

In other words, we shall admit the possibility of the presence of a "disorder" at the initial observation time $t = 0$. Under this assumption the *exponential* nature of the appearance of the "disorder" can be stated as follows:

$$P(\theta > t \,|\, \theta > 0) = \mathrm{e}^{-\lambda t}. \tag{8}$$

In the solution of rapid-detection problems in Versions A and B the key role is played by the *a posteriori probability* (constructed from the results of the observations X_s, $s \leqslant t$)

$$\pi_t = P(\theta \leqslant t \,|\, \mathscr{F}_t^X), \tag{9}$$

which is the probability that the "disorder" appeared in the interval $[0,t]$.

Indeed, it is not difficult to see that for every stopping time τ

$$P(\tau < \theta) = E_\pi(1 - \pi_\tau) \tag{10}$$

and

$$E(\tau - \theta)^+ = E\int_0^\infty I(\theta \leqslant s \leqslant \tau)\,\mathrm{d}s = E\int_0^\tau P\big(\theta \leqslant s \,|\, \mathscr{F}_s^X\big)\,\mathrm{d}s = E_\pi \int_0^\tau \pi_s\,\mathrm{d}s, \tag{11}$$

where E_π is the average with respect to the measure P_π — the probability distribution of the process X under the assumption that $\pi_0 = \pi$.

Thus, from (7), (10), and (11) we find that

$$\mathbb{B}_\pi(c) = \inf_{t\in\mathfrak{M}} E_\pi\left\{(1-\pi_\tau) + c\int_0^\tau \pi_s\,\mathrm{d}s\right\}, \tag{12}$$

where the index π on $\mathbb{B}(c)$ reminds us that $\pi_0 = P(\theta = 0) = \pi$.

We see by (12) that the solution of the problem of rapid detection in Version B has been reduced to solving the *problem of optimal stopping for a process with a posteriori probability* $(\pi_t)_{t\geqslant 0}$. The subsequent steps in the solution of this problem involved primarily clarifying the structure of the process $(\pi_t)_{t\geqslant 0}$. With this purpose, it is convenient to introduce the process $\varphi_t = \pi_t/(1-\pi_t)$ and first study its structure, then pass to the process π_t.

Let $P_\theta = \mathrm{Law}(X|\theta)$ be the probability distribution of the process $X = (X_t)_{t\geqslant 0}$ under the assumption that the parameter θ in the model (5) is simply some numerical parameter from $[0,\infty]$. We shall use the notation (see (2))

$$L_t = \frac{\mathrm{d}P_0}{\mathrm{d}P_\infty}(X,t) \tag{13}$$

for the Radon–Nikodým derivative of the measure $P_0\big|\mathscr{F}_t^X$ with respect to the measure $P_\infty|\mathscr{F}_t^X$, where, as usual, $P_\theta|\mathscr{F}_t^X$ is the restriction of the measure P_θ to the σ-algebra $\mathscr{F}_t^X$.

Setting

$$P(\cdot) = \pi P_0(\cdot) + (1-\pi)\int_0^\infty \lambda e^{-\lambda\theta} P_\theta(\cdot)\,\mathrm{d}\theta,$$

we define similarly the derivative $\dfrac{\mathrm{d}P_\theta}{\mathrm{d}P}(X,t)$ with respect to the measure $P = P(\cdot)$.

By Bayes' formula we find that

$$\pi_t = \pi\frac{\mathrm{d}P_0}{\mathrm{d}P}(X,t) + (1-\pi)\int_0^t \frac{\mathrm{d}P_\theta}{\mathrm{d}P}\lambda e^{-\lambda\theta}\,\mathrm{d}\theta$$

and

$$1-\pi_t = (1-\pi)e^{-\lambda t}\frac{\mathrm{d}P_t}{\mathrm{d}P}(X,t) = (1-\pi)e^{-\lambda t}\frac{\mathrm{d}P_\infty}{\mathrm{d}P}(X,t).$$

Therefore

$$\varphi_t = \frac{\pi_t}{1-\pi_t} = \varphi_0 e^{\lambda t} L_t + \lambda e^{\lambda t}\int_0^t \frac{L_t}{L_\theta} e^{-\lambda\theta}\,\mathrm{d}\theta = e^{\lambda t} L_t\left(\varphi_0 + \lambda\int_0^t \frac{e^{-\lambda\theta}}{L_\theta}\,\mathrm{d}\theta\right). \tag{14}$$

For the model (5) the likelihood ratio is

$$L_t = \exp\left(\frac{r}{\sigma^2}X_t - \frac{r^2}{2\sigma^2}t\right). \tag{15}$$

Hence by the *Itô formula* ([11]–[13]) we find that

$$\mathrm{d}L_t = \frac{r}{\sigma^2} L_t \,\mathrm{d}X_t. \tag{16}$$

Taking account of this relation and again applying the Itô formula, we find by (14) that

$$\mathrm{d}\varphi_t = \lambda(1+\varphi_t)\,\mathrm{d}t + \frac{r}{\sigma^2}\varphi_t\,\mathrm{d}X_t, \quad \varphi_0 = \frac{\pi}{1-\pi}. \tag{17}$$

From this relation and the fact that $\pi_t = \varphi_t/(1+\varphi_t)$, and again applying the Itô formula, we obtain

$$\mathrm{d}\pi_t = \left(\lambda - \frac{r^2}{\sigma^2}\pi_t^2\right)(1-\pi_t)\,\mathrm{d}t + \frac{r}{\sigma^2}\pi_t(1-\pi_t)\,\mathrm{d}X_t, \quad \pi_0 = \pi. \tag{18}$$

Equations (17) and (18) were first published in 1961 in our paper [2] (and then, in more expanded form in [3] in 1963 — see also [7] and [8]).

Equation (18) must, of course, be understood in the sense that the corresponding integral relation holds:

$$\pi_t = \pi_0 + \int_0^t \left(\lambda - \frac{r^2}{\sigma^2}\pi_s^2\right)(1-\pi_s)\,\mathrm{d}s + \int_0^t \frac{r}{\sigma^2}\pi_s(1-\pi_s)\,\mathrm{d}X_s. \tag{19}$$

Here, however, a theoretical difficulty arises, involving the interpretation of the *stochastic integral with respect to* $\mathrm{d}X_s$. Up to the time when this equation (19) was obtained (1959–1960), stochastic integrals were defined (K. Itô) only for the case when X is a Brownian motion (Wiener process); and naturally it was necessary to give a precise interpretation of the stochastic integral in the case of the process (4) that we were considering. One of the ways of interpreting the integral $\int_0^t \pi_s(1-\pi_s)\,\mathrm{d}X_s$ would have been (in accordance with (5)) simply as the sum of two integrals

$$\int_0^t \pi_s(1-\pi_s) r I(s>\theta)\,\mathrm{d}s + \int_0^t \sigma\pi_s(1-\pi_s)\,\mathrm{d}B_s,$$

where this last integral is the Itô stochastic integral with respect to the Brownian motion $B = (B_s)_{s\geqslant 0}$.

However, such a method of interpretation did not clarify, say, such a natural question as whether the process $(\pi_t)_{t\geqslant 0}$ is a Markov process. As it turned out, such is indeed the case. One can see this and give Eq. (19) a precise meaning as follows.

Having the process $X = (X_t)_{t\geqslant 0}$ defined by the formula (4), we form the process

$$\overline{B}_t = \frac{1}{\sigma}\left(X_t - r\int_0^t \pi_s\,\mathrm{d}s\right) = \frac{r}{\sigma}\int_0^t (\theta_s - \pi_s)\,\mathrm{d}s + B_t. \tag{20}$$

It is quite remarkable that relative to the flow $(\mathscr{F}_t^X)_{t\geqslant 0}$ this process is a square-integrable martingale, that is, $E|\overline{B}_t|^2 < \infty$, $E(\overline{B}_t|\mathscr{F}_s^X) = \overline{B}_s$, and

$$E|\overline{B}_t - \overline{B}_s|^2 = t - s, \quad s \leqslant t.$$

Hence it follows by a well-known theorem of Lévy that the process $\overline{B} = (\overline{B}_t)_{t\geqslant 0}$ is a *Brownian motion.*

Since by (20) we have

$$\mathrm{d}X_t = r\pi_t\,\mathrm{d}t + \sigma\,\mathrm{d}\overline{B}_t, \quad X_0 = 0, \tag{21}$$

it follows that (18) takes the following form:

$$\mathrm{d}\pi_t = \lambda(1-\pi_t)\,\mathrm{d}t + \frac{r}{\sigma}\pi_t(1-\pi_t)\,\mathrm{d}\overline{B}_t, \quad \pi_0 = \pi. \tag{22}$$

There is now no difficulty in assigning a meaning to this equation — it is the usual *stochastic differential equation* of Itô, where the integration with respect to $\mathrm{d}\overline{B}_t$ is understood as stochastic integration with respect to the Brownian motion $\overline{B} = (\overline{B}_t)_{t\geqslant 0}$.

The process $\overline{B} = (\overline{B}_t)_{t\geqslant 0}$ is called a *renewal* process, since by virtue of (21) it “renews” the “information” $\mathscr{F}_t^X$ that was obtained from observation of the process X over the interval $[0,t]$. It is also interesting that in the scheme (4) that we are studying

$$\mathscr{F}_t^{\overline{B}} = \mathscr{F}_t^X, \quad t > 0.$$

In other words, the process $\overline{B}$ not only “renews the information,” it also holds the same information as the process X.

Thus, to give an exact meaning to all the equations we are considering which contain the differential $\mathrm{d}X_t$, we must first represent that differential in the form (21) and carry out all further operations with the differential $\mathrm{d}\overline{B}_t$. Thus, if we look at (15), we find that

$$L_t = \exp\left[\frac{r}{\sigma}\overline{B}_t + \frac{r^2}{\sigma^2}\int_0^t \left(\pi_s - \frac{1}{2}\right)\mathrm{d}s\right],$$

and hence (see (16)) by the classical formula of Itô we find

$$\mathrm{d}L_t = \frac{r^2}{\sigma^2}L_t\pi_t\,\mathrm{d}t + \frac{r}{\sigma}L_t\,\mathrm{d}\overline{B}_t.$$

2.2. From Eq. (22) we can immediately draw the following important conclusion — the process $(\pi_t)_{t\geqslant 0}$ (with respect to the flow $(\mathscr{F}_t^X)_{t\geqslant 0}$) is a *diffusion Markov process* with phase space $[0,1]$, local drift

$$a(\pi) = \lambda(1-\pi), \tag{23}$$

and local variance

$$b^2(\pi) = \left(\frac{r}{\sigma}\right)^2 \left(\pi(1-\pi)\right)^2. \tag{24}$$

It follows from this that the problem (12) in Version B is the problem of *optimal stopping of the diffusion process* $(\pi_t)_{t\geqslant 0}$ with the infinitesimal operator

$$\mathscr{A} = a(\pi)\frac{\mathrm{d}}{\mathrm{d}\pi} + \frac{1}{2}b^2(\pi)\frac{\mathrm{d}^2}{\mathrm{d}\pi^2}. \tag{25}$$

To simplify the notation we set (assuming the constant $c > 0$ fixed, but on the other hand emphasizing the dependence on $\pi_0 = \pi$)

$$\rho^*(\pi) = \mathbb{B}_\pi(c).$$

We also use the notation $\rho_0(\pi) = 1 - \pi$. This function $\rho_0(\pi)$ can be regarded as the *risk due to stopping immediately* ($\tau = 0$), and it is clear that $\rho^*(\pi) \leqslant \rho_0(\pi)$. The function $\rho^*(\pi)$, as one can easily show, is convex upward and decreases as π increases from 0 to 1 (see Fig. 1).

It becomes clear from these properties that there exists a value $A^* \in [0,1]$ such that the following relations hold:

$$C^* \equiv \{\pi : \rho^*(\pi) < \rho_0(\pi)\} = \{\pi : \pi < A^*\}$$

and

$$D^* \equiv \{\pi : \rho^*(\pi) = \rho_0(\pi)\} = \{\pi : \pi \geqslant A^*\}.$$

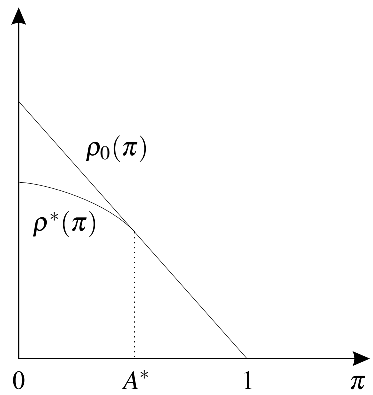


Fig. 1

It turns out that in the optimal stopping problem that we are considering, the optimal stopping time τ^* has a rather simple structure:

$$\tau^* = \inf\{t : \pi_t \in D^*\} \ (= \inf\{t : \pi_t \geqslant A^*\}). \tag{26}$$

In other words, the alarm signalling the presence of "disorder" should be given when the process π_t first reaches the domain D^*, which it is natural to call the *stopping region*. Then the region C^* is just the *region of continued observation*.

How are we to find the risk function $\rho^*(\pi)$ and the boundary point A^* that separates the stopping region from the region of continued observation? Here it helped to recognize that one could connect the so-called *Stefan problem* (a problem with moving boundary) with the optimal stopping problem under consideration. The solution of the Stefan problem yields both the unknown function $\rho^*(\pi)$ and the unknown boundary point A^*, which according to (26) determines the stopping time τ^* $(= \inf\{t : \pi_t \geqslant A^*\})$.

Thus we consider the following *Stefan problem*:

Find a function $\rho = \rho(\pi)$, $\pi \in [0,1]$ *and a boundary point A such that*

$$\rho(\pi) = 1 - \pi, \qquad \pi \geqslant A, \tag{27}$$

$$\mathscr{A}\rho(\pi) = -c\pi, \qquad \pi < A. \tag{28}$$

Condition (27) is completely understandable, since for $\pi \geqslant A$ the required risk must coincide with the risk from stopping immediately. But Eq. (28) is merely the backward Kolmogorov equation for the functional $E_\pi\big((1-\pi_t) + c\int_0^\tau \pi_s\,\mathrm{d}s\big)$, where $\tau = \inf\{t : \pi_t \geqslant A\}$ and $\pi < A$.

Let us write (28) in detailed form

$$\lambda(1-\pi)\rho'(\pi) + \frac{1}{2}\left(\frac{r}{\sigma}\right)^2 \pi^2(1-\pi)^2\rho''(\pi) = -c\pi. \tag{29}$$

The general solution of this equation contains two undetermined constants (C_1 and C_2), which, together with the unknown boundary A yields *three* unkown constants, even though up to now we have had only *one* condition on the boundary A:

$$\rho(A) = 1 - A. \tag{30}$$

To solve the problem we introduce two more conditions:

$$\rho'(A-) = -1 \tag{31}$$

and

$$\rho'(0+) = 0. \tag{32}$$

Condition (31) is called the *smooth-fit* condition and is introduced *ad hoc*. (In [14] the conditions are given under which smooth-fit holds of necessity for the risk function on the boundary of the stopping region.) As for (32), its naturalness is a consequence of the following ([9]).

We use the notation $r(\pi) = \rho'(\pi)$. It then follows from (29) that

$$r'(\pi) = -\frac{c\pi + \lambda(1-\pi)r(\pi)}{\frac{1}{2}(\frac{r}{\sigma})^2\pi^2(1-\pi)^2}. \tag{33}$$

If we assume that $r(0) \neq 0$ for the solution we require, then one can see by (33) that $r'(\pi) \sim 1/\pi^2$ in a neighborhood of zero, and hence $\rho(\pi) \sim \ln\pi$. But this contradicts the fact that the solutions we are interested in must satisfy $0 \leqslant \rho(\pi) \leqslant 1-\pi$ by the meaning of the problem.

But if $r(0) = 0$, then $r'(\pi) \sim 1/\pi$ in a neighborhood of zero, and Eq. (29) may have a solution that does not tend to $+\infty$ or $-\infty$ as $\pi \downarrow 0$. In fact, analysis of the integral curves of Eq. (29) exhibits the following behavior for them.

We fix some point a in the interval $[0,1]$, and we shall “release” the solution from the point $1-a$, that is, set $\rho(a) = 1-a$ with the additional condition that $\rho'(a) = -1$. Then one can observe the following.

If a is close to 1, the corresponding solutions “tend” to $-\infty$. But if a is close to zero, they “tend” to $+\infty$. And there is a *unique* value $a = a^*$ at which the solution is finite at the point $\pi = 0$ (see Fig. 2). Speaking in the language of the qualitative theory of differential equations, the solutions in question have a *separatrix* that, as it happens, is the solution that yields the solution of the optimal stopping problem in Version B.

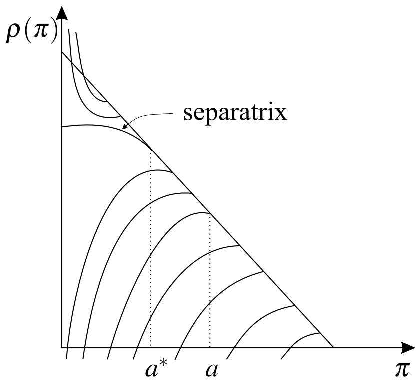


Fig. 2

A simple analysis ([2], [10]) shows that the solution of the Stefan problem (27), (28) with the additional conditions (31) and (32) has the following form:

$$\rho(\pi) = \begin{cases} (1-A) - \displaystyle\int_\pi^A y(x)\,\mathrm{d}x, & \pi \in [0,A], \\ 1-\pi, & \pi \in [A,1], \end{cases} \tag{34}$$

where

$$y(x) = -C\int_0^x \mathrm{e}^{-\Lambda[G(x)-G(u)]}\,\frac{\mathrm{d}u}{u(1-u)^2},$$

$$G(u) = \ln\frac{u}{1-u} - \frac{1}{u}, \quad \Lambda = \frac{\lambda}{r^2/(2\sigma^2)}, \quad C = \frac{c}{r^2/(2\sigma^2)}.$$

The constant A is found from the equation

$$C \int_0^A \mathrm{e}^{-\Lambda[G(A)-G(u)]} \frac{\mathrm{d}u}{u(1-u)^2} = 1, \tag{35}$$

which is Eq. (31).

2.3. We now claim that the solution $\big(\rho(\pi), A\big)$ found for the Stefan problem is *precisely* the pair $\big(\rho^*(\pi), A^*\big)$ that yields the solution of the optimal stopping problem

$$\rho^*(\pi) = \inf_{t \in \mathfrak{M}} E_\pi \left\{ (1-\pi_t) + c \int_0^\tau \pi_s \, \mathrm{d}s \right\}.$$

One can verify this by resorting to the so-called *verification conditions*:

a) for every $\tau \in \mathfrak{M}$

$$E_\pi \left\{ (1-\pi_t) + c \int_0^\tau \pi_s \, \mathrm{d}s \right\} \geqslant \rho(\pi) \, ,$$

where $\rho(\pi)$ is the solution defined by formula (34);

b) for the threshold A just found (from Eq. (35)) the corresponding time $\tau_A = \inf\{t : \pi_t \geqslant A\}$ is such that

$$E_\pi \left\{ (1-\pi_{\tau_A}) + c \int_0^{\tau_A} \pi_s \, \mathrm{d}s \right\} = \rho(\pi).$$

Both of these properties can easily be established by applying the Itô formula to the process $\rho(\pi_t)$. (For details see, for example, the 1976 monograph [10], Chapter IV, § 4.)

2.4. Having obtained the solution of the optimal stopping problem in Version B, one can now easily obtain the solution in Version A as well.

First of all, it is clear that the usual Lagrange method of solving constrained extremal problems by reducing them to the study of "weighted" functionals shows that in the class $\mathfrak{M}_\alpha$ the optimal stopping time τ_α^* *exists* and has the following form:

$$\tau_\alpha^* = \inf\{t : \pi_t \geqslant A_\alpha^*\}, \tag{36}$$

where A_α^* is a constant. But since

$$\alpha = P_\pi(\tau_\alpha^* \leqslant \theta) = E_\pi(1-\pi_{\tau_\alpha^*}),$$

we find that

$$\alpha = (1-A_\alpha^*) I(\pi \leqslant A_\alpha^*) + (1-\pi) I(\pi > A_\alpha^*). \tag{37}$$

Thus, considering the case when $P(\theta = 0) = \pi = 0$, we find that the optimal threshold satisfies

$$A^*_\alpha = 1 - \alpha. \tag{38}$$

Let $c = c(\alpha)$ be the value of the constant in (7) at which in Version B the threshold $A^* = A^*(c)$ is exactly equal to $A^* = 1 - \alpha$. (The existence of such a value $c = c_\alpha$ follows from the continuous dependence of $A^*(c)$ on c and the fact that the values $A^*(c)$ belong to the interval $[0,1]$ when c assumes values in $[0,\infty)$.) Then, on the one hand, for $\pi = 0$,

$$\begin{aligned}\rho^*(0) &= \inf_\tau \{P_0(\tau < \theta) + c_\alpha E_0(\tau - \theta)^+\}\\ &= P_0(\tau^*_\alpha < \theta) + c_\alpha E_0(\tau^*_\alpha - \theta \,|\, \tau^*_\alpha \geqslant \theta) P_0(\tau^*_\alpha \geqslant \theta)\\ &= \alpha + c_\alpha E_0(\tau_\alpha - \theta \,|\, \tau^*_\alpha \geqslant \theta)(1 - \alpha).\end{aligned} \tag{39}$$

On the other hand, by (34)

$$\rho^*(0) = (1 - A^*_\alpha) + \frac{c_\alpha}{\nu} \int_0^{A^*_\alpha} \left[\int_0^x \mathrm{e}^{-(\lambda/\nu)[G(x)-G(u)]} \frac{\mathrm{d}u}{u(1-u)^2} \right] \mathrm{d}x \tag{40}$$

with $\nu = r^2/(2\sigma^2)$. Since $1 - A^*_\alpha = \alpha$, we find by comparing (39) and (40) that in the class $\mathfrak{M}_\alpha$ the delay time

$$\mathbb{R}(\alpha;\lambda) = E_0(\tau^*_\alpha - \theta \,|\, \tau^*_\alpha \geqslant 0)$$

is determined by the formula

$$\mathbb{R}(\alpha;\lambda) = \frac{1}{\nu(1-\alpha)} \int_0^{1-\alpha} \left[\int_0^x \mathrm{e}^{-(\lambda/\nu)[G(x)-G(u)]} \frac{\mathrm{d}u}{u(1-u)^2} \right] \mathrm{d}x. \tag{41}$$

2.5. Let us analyze formula (41) under the assumption that $\lambda \to 0$, that is, let $E\theta \to \infty$. Then one can see that under this assumption it is natural to assume in addition that $\alpha \to 1$.

Let $\lambda \to 0$ and $\alpha \to 1$, but in such a way that $(1-\alpha)/\lambda \to T$, where T is some positive constant. Analysis of formula (41) shows that under such a limiting passage

$$\mathbb{R}(\alpha;\lambda) \to \mathbb{R}(T), \tag{42}$$

where

$$\mathbb{R}(T) = \frac{1}{\nu} \left\{ \mathrm{e}^b(-\mathrm{Ei}(-b)) - 1 + b \int_0^\infty \mathrm{e}^{-bu} \frac{\ln(1+u)}{u} \,\mathrm{d}u \right\}, \tag{43}$$

$-\mathrm{Ei}(-b) = \displaystyle\int_b^\infty \frac{\mathrm{e}^{-z}}{z}\,\mathrm{d}z$ is the *exponential integral function* and $b = \dfrac{1}{\nu T}$.

Assuming for simplicity that $\nu = 1$, we find

$$\mathbb{R}(T) = \begin{cases} \ln T - 1 - \mathbb{C} + O(1/T), & T \to \infty, \\ T/2 + O(T^2), & T \to 0, \end{cases} \tag{44}$$

where $\mathbb{C} = 0.577\ldots$ is Euler's constant.

Setting $\varphi_t = \pi_t/(1-\pi_t)$, we see that

$$\tau_\alpha^* = \inf\{t : \pi_t \geqslant 1-\alpha\} = \inf\left\{t : \frac{\varphi_t}{\lambda} \geqslant \frac{1-\alpha}{\alpha\lambda}\right\}.$$

Let $\pi_0 = 0$ and $\psi_t = \lim_{\lambda \downarrow 0} \varphi_t/\lambda$, where φ_t of course depends on λ (see (14)). Then, since

$$\mathrm{d}\varphi_t = \lambda(1+\varphi_t)\,\mathrm{d}t + \frac{r}{\sigma^2}\varphi_t\,\mathrm{d}X_t, \quad \varphi_0 = 0, \tag{45}$$

we find

$$\mathrm{d}\psi_t = \mathrm{d}t + \frac{r}{\sigma^2}\psi_t\,\mathrm{d}X_t, \tag{46}$$

or, in the integral form

$$\psi_t = t + \frac{r}{\sigma^2}\int_0^t \psi_s\,\mathrm{d}X_s. \tag{47}$$

If we assume that $\theta = \infty$, that is, that "disorder" does not arise, then $\mathrm{d}X_s = \sigma\,\mathrm{d}B_s$, and so ($P_\infty$-almost surely)

$$\psi_t = t + \frac{r}{\sigma}\int_0^t \psi_s\,\mathrm{d}B_s. \tag{48}$$

Under the limiting passages $\lambda \to 0$ and $\alpha \to 1$ such that $(1-\alpha)/\lambda \to T$ we find

$$\tau_\alpha^* = \inf\left\{t : \frac{\varphi_t}{\lambda} \geqslant \frac{1-\alpha}{\alpha\lambda}\right\} \to \tau^*(T) = \inf\{t : \psi_t \geqslant T\}. \tag{49}$$

Consequently, by (48)

$$\psi_{\tau^*(T)} = \tau^*(T) + \frac{r}{\sigma}\int_0^{\tau^*(T)} \psi_s\,\mathrm{d}B_s \tag{50}$$

and

$$T = E_\infty \psi_{\tau^*(T)} = E_\infty \tau^*(T).$$

In other words, the constant T has a very clear meaning — it is the *average time elapsed before the* (*false*) *alarm is raised* when the process $(\psi_t)_{t\geqslant 0}$ subject to Eq. (48) is observed.

It follows from formula (14) that the process $\psi_t = \lim_{\lambda\downarrow 0} \frac{\varphi_t}{\lambda}$ can be represented in the form

$$\psi_t = \int_0^t \frac{L_t}{L_\theta}\,\mathrm{d}\theta. \tag{51}$$

The statistic $\psi = (\psi_t)_{t\geqslant 0}$, which first appeared in [2] and [3], later came to be known in the literature as the *Shiryaev–Roberts* statistic. (S. Roberts studied the analog of this statistic for the case of discrete time in [15].) Many papers (see, for example, [23]) show that this statistic is often endowed with the properties of *optimality* or *almost optimality* under various tests applied to the delay time.

3. The Change-Point Problem — Versions C, D, and E

3.1. It was assumed in Versions A and B that θ is a *random* variable (with exponential distribution). In many respects it is precisely because a successful statement of the rapid detection problem was found (in the papers [1]–[3]) admitting an exact solution, that other (parametric) formulations were soon proposed (see the collection of articles [23]), in which it was assumed that θ is simply a parameter taking a value in $[0,\infty)$.

In this case, for example, the following formulations of rapid-detection problems are known, in which $\mathfrak{M}(T) = \{\tau : E_\infty\tau \geqslant T\}$.

Version C. Find the quantity

$$\mathbb{C}(T) = \inf_{\tau\in\mathfrak{M}(T)} \sup_\theta \operatorname*{ess\,sup}_\omega E_\theta\big((\tau-\theta)^+ \big| \mathscr{F}_\theta\big)(\omega). \tag{52}$$

Version D. Find the quantity

$$\mathbb{D}(T) = \inf_{\tau\in\mathfrak{M}(T)} \sup_\theta E_\theta\big(t-\theta \big| \tau \geqslant \theta\big). \tag{53}$$

Version E. Find the quantity

$$\mathbb{E}(T) = \inf_{\tau\in\mathfrak{M}(T)} \frac{1}{T}\int_0^\infty E_\theta(\tau-\theta)^+\,\mathrm{d}\theta. \tag{54}$$

Naturally, in each of these problems it is required to find also the optimal or asymptotically optimal (at least as $T\to\infty$) stopping times. (The criterion (52) was proposed in the paper of Lorden [17]).

It is appropriate to note at this point that the problem of rapid detection (in Versions A, B, C, D, E, and others) is considered in the statistical literature not only for the model (4) that we are interested in, but also for many others, mostly for the case of discrete time (see, for example, [15]–[21]). Here we note only the results obtained for Versions C, D, and E in the case of the model (4).

3.2. Together with the statistic

$$\psi_t = \int_0^t \frac{L_t}{L_\theta}\,\mathrm{d}\theta$$

we also introduce the very useful statistic

$$\gamma_t = \sup_{\theta \leqslant t} \frac{L_t}{L_\theta}. \tag{55}$$

In the author's paper [22] it was shown that this statistic is optimal in the problem of rapid detection in Version C: *for every* $T > 0$ *the time*

$$\tau^*(T) = \inf\{t \geqslant 0 : \gamma_t \geqslant C^*(T)\} \tag{56}$$

is optimal in the class $\mathfrak{M}(T)$. Here $C^*(T)$ is a root of the equation

$$C - 1 - \ln C = T, \tag{57}$$

The key point in the proof is the following inequality, which is valid for every stopping time $\tau \in \mathfrak{M}(T)$:

$$\sup_\theta \operatorname*{ess\,sup}_\omega E_\theta\big((\tau-\theta)^+ \,\big|\, \mathscr{F}_\theta\big)(\omega) \geqslant \frac{E_\infty \int_0^\tau \gamma_t\,\mathrm{d}t}{E_\infty \gamma_\tau}. \tag{58}$$

The magnitude of the delay

$$\mathbb{C}(T) = \inf_{\tau \in \mathfrak{M}(T)} \sup_\theta \operatorname*{ess\,sup}_\omega E_\theta\big((\tau-\theta)^+ \,\big|\, \mathscr{F}_\theta\big)(\omega)$$

is defined by the formula ($\nu = 1$):

$$\mathbb{C}(T) = \ln C^*(T) + \frac{1}{C^*(T)} - 1 \sim \begin{cases} \ln T - 1 + O(1/T), & T \to \infty, \\ T/2 + O(T^2), & T \to 0. \end{cases}$$

We remark that in the case of the model (4)

$$\gamma_t = \exp\Big(H_t - \min_{\theta \leqslant t} H_\theta\Big), \quad \text{where} \quad H_t = \frac{r}{\sigma^2} X_t - \frac{r^2}{2\sigma^2} t.$$

The processes $H_t - \min_{\theta \leqslant t} H_\theta$ and $\max_{\theta \leqslant t} H_\theta - H_t$ are called CUSUM (cumulative-sum) processes in the literature. Procedures for detecting changes in the probability charcteristics based on CUSUM processes were introduced in the papers of E. Page [24], [25], and have become widely known in practical work on the qualitative analysis of production processes (see, for example, the monograph [26]).

In Version D, both statistics $\gamma = (\gamma_t)_{t \geqslant 0}$ and $\psi = (\psi_t)_{t \geqslant 0}$ are asymptotically optimal (at $T \to \infty$, see [16], [27]). Moreover

$$\mathbb{D}(T) = \inf_{\tau \in \mathfrak{M}(T)} \sup_{\theta} E_\theta(\tau - \theta \mid \tau \geqslant \theta)$$

has (for $\nu = 1$) the following asymptotics:

$$\mathbb{D}(T) \sim \ln T, \quad T \to \infty.$$

In Version E, the optimal statistic (for every $T > 0$) is $\psi = (\psi_t)_{t \geqslant 0}$ and the corresponding quantity

$$\mathbb{E}(T) = \inf_{\tau \in \mathfrak{M}(T)} \frac{1}{T} \int_0^\infty E_\theta(\tau - \theta)^+ \, \mathrm{d}\theta$$

coincides exactly with the $\mathbb{R}(T)$ defined in (43) (for more details, see [3]). Thereby, according to (44) (with $\nu = 1$)

$$\mathbb{E}(T) = \ln T - 1 + \mathbb{C} + O(1/T), \quad T \to \infty,$$

where $\mathbb{C} = 0.577\ldots$ is Euler's constant.

4. The Change-Point Problem — Version F

4.1. In Section 1 we noted that in the course of our discussions with radio engineers the problem of most rapid detection of a randomly appearing target, when the observations were *multi-stage*, was put forth as almost the basic problem. The essence of the matter consists of the following.

Assume that some method of observation has been developed that ends (as happened in all the versions A, B, C, D, and E considered so far) in the raising of an alarm (say, at time τ_1). But the observations do not stop at that point. Everything essentially starts over again, and so on, many times.

Here is a way of posing the rapid detection problem. Let $\psi_t = \psi(t; X_s; s \leqslant t)$, $t \geqslant 0$, be some nonnegative functional (statistic) of the observations X_s, $s \leqslant t$, on the basis of whose values the decision is taken to raise an alarm signaling the presence of a target. Assume that this happens (at time τ_1) when the process ψ_t, $t \geqslant 0$, first exceeds some critical threshold a.

We shall assume that the detection procedure is so organized that the observations essentially begin over again, based on the functional

$$\psi_{\tau_1+t} = \psi(t; X_s - X_{\tau_1}, \tau_1 \leqslant s \leqslant \tau_1 + t).$$

When the process ψ_{τ_1+t} reaches the threshold a an alarm is sounded (at time $\tau_1 + \tau_2$) and so on.

Assume that for the method chosen to send the alarm signals

$$E_\infty \tau_i = T, \quad i \geqslant 1,$$

where the mathematical expectation is taken under the assumption that the target ("disorder") is absent, that is, $\theta = \infty$ and T is some constant which by its meaning is the *mean time between two false alarms*.

4.2. The question as to the nature of the statistic, that is the character of the time when the "target" ("disorder") appears is far from trivial. Based on practical common sense it is natural to assume: first, that the appearance of the target is preceded by a long period of observation; second, that a stationary regime of observation gets established (in the absence of a target); third, that the target appears against the background of this established stationary regime.

Under such assumptions the mean delay time (for the given observation procedure, say δ) in the detection of "disorder" will be determined by the formula

$$\mathbb{F}^\delta(T) = \int_0^a (E_0^\psi \tau_a) F^\delta(\mathrm{d}\psi),$$

where $F^\delta(\mathrm{d}\psi)$ is the one-dimensional stationary distribution of the observed process, $E_0^\psi \tau_a$ is the mathematical expectation of the time τ_a of emergence to level a of the process being observed under the assumption that the process was in the state ψ at the time when the disorder appeared.

We can now state the problem of rapid detection of a stationary regime as follows.

Version F. Suppose the observation procedure δ belongs to $\Delta(T)$, where $\Delta(T)$ is the class of procedures for which $E_\infty \tau_i = T$. It is required to find

$$\mathbb{F}(T) = \inf_{\delta \in \Delta(T)} \mathbb{F}^\delta(T)$$

and the optimal observation procedure δ^*.

The result obtained in [2] and [3] asserts that the optimal procedure δ^* consists of observation of the process $\psi = (\psi_t)_{t\geqslant 0}$ satisfying the stochastic differential equation (see (46))

$$\mathrm{d}\psi_t = \mathrm{d}t + \frac{r}{\sigma^2}\psi_t\,\mathrm{d}X_t, \quad \psi_0 = 0.$$

The first stopping time τ_1 is defined as the time when this process first attains the level $a = T$. Then the observations are renewed each time according to the same scheme as in the first stage.

It is interesting that the minimum possible mean delay time $\mathbb{F}(T) = \mathbb{E}(T)$, that is, coincides with the mean delay time in Version E.

5. Development of the Theory of Optimal Stopping Rules and the Theory of Nonlinear Filtering

5.1. The rapid-detection problem (7) stated in Version B was reduced, as shown in Section 2, to the problem of optimal stopping (12) of the diffusion Markov process $(\pi_t)_{t\geqslant 0}$. The fact that this is a diffusion Markov process follows immediately from the stochastic differential equation (22), which is one of the first equations of optimal nonlinear *filtering*.

Indeed, if $\theta_t = I(t \geqslant 0)$, then $\pi_t = E(\theta_t|\mathscr{F}_t^X)$, which is the optimal estimate of the value of θ_t in the mean-square sense using the observations X_s, $s \leqslant t$. In general the problem of constructing estimates $E(\theta_t|\mathscr{F}_t^X)$ for a pair $(\theta_t, X_t)_{t\geqslant 0}$ of random processes $(\theta_t)_{t\geqslant 0}$ and $(X_t)_{t\geqslant 0}$ forms the basic subject of the *theory of optimal* (*nonlinear*) *filtering*.

At practically the same time (the late 1950s and early 1960s) when Eqs. (17) and (18) were obtained, there appeared papers by Kalman and Bucy on optimal *linear* filtering for Gaussian processes subject to the stochastic equations

$$\begin{aligned}\mathrm{d}\theta_t &= a(t)\theta_t\,\mathrm{d}t + b(t)\,\mathrm{d}W_t',\\ \mathrm{d}X_t &= A(t)\theta_t\,\mathrm{d}t + B(t)\,\mathrm{d}W_t'',\end{aligned}$$

where (W', W'') is a pair of Wiener processes, and the conditional distribution $\mathrm{Law}(\theta_0|X_0)$ is Gaussian.

The works of R. L. Stratonovich [28], [29] on *conditional Markov processes* belong to the same time period. In these works the interpretation of the stochastic integrals that arose was different (as became clear later) from the interpretation in the generally accepted sense proposed by Itô.

All these works initiated a broad program of our research in the context of the special-topics seminar "Nonlinear filtering, controllable processes, and martingale theory," which ran for a long time in the Steklov Mathematical Institute with the active participation of undergraduates, graduate students and scholars of Moscow University and other Moscow organizations.

In 1974, the monograph *Statistics of Random Processes* [11] (by R. S. Liptser and A. N. Shiryaev) was published. Its subtitle, "Nonlinear Filtering and Related Questions," showed that it was the theory of optimal nonlinear (and linear) theory that formed the main subject of the book, in which many results, including those of participants in the special-topics seminar, were included.

5.2. The problem of rapid detection, which we studied in 1961 ([1], [2]), served as the starting point for many later papers on optimal stopping rules using Markov methods, stochastic differential equations, and the elements of martingale theory. In 1963 there appeared a paper of E. B. Dynkin [30] devoted to problems of optimal choice of the stopping time for a general Markov process, whose statement was the following.

Let $X = (X_t, \mathscr{F}_t, P_x)$ be a Markov process in the phase space (E, B) with probability distribution P_x corresponding to the initial state $x \in E$. Assume that by stopping observations at a finite Markov time τ we obtain a gain $g(X_\tau)$. Then the mean gain corresponding to the initial state $x \in E$ is the mathematical expectation $E_x g(X_\tau)$.

Set

$$S(x) = \sup_\tau E_x g(X_\tau).$$

We call the function $S(x)$ the *price*, and a time τ_ε such that

$$S(x) \leqslant E_x g(X_{\tau_\varepsilon}) + \varepsilon$$

for all $x \in E$ will be called *ε-optimal.*

The main questions posed in [30], and widely studied in the monographs [31], [10], are the following: *What is the structure of the function $S(x)$? How can it be found? When do there exist ε-optimal and optimal (that is, 0-optimal) times? What is their structure?*

In the case of discrete time, besides the "Markov" approach to the problem of optimal stopping just discussed, there exists also the so-called "martingale" approach, initiated in a paper of Snell and developed in papers of Chow, Robbins, and Siegmund (see their monograph [32]). Although in essence the two approaches are equivalent (every process can be made Markov by "enlarging" the state space), nevertheless in solving particular problems (especially for the case of continuous time) the "Markov" approach is preferable because of the presence of the powerful analytic machinery provided by the Kolmogorov equations for the functionals of a Markov process, as well as the machinery of stochastic differential equations, which have an intimate connection to the methods of the theory of partial differential equations.

6. Martingale Theory and Stochastic Calculus

6.1. It became clear at the very beginning (1959–1961) of the study of the disorder problem in Versions A and B, that the methods of martingale theory and stochastic calculus play a decisive role both in deriving the equations for the *a posteriori* probability $\pi_t = P(\theta \leqslant t | \mathscr{F}_t^X)$ and in the proof that the stopping time obtained $\tau^* = \inf\{t : \pi_t \geqslant A^*\}$ is optimal (applying the "verification conditions" based on the Itô formula).

For us — the participants in the abovementioned seminar on "Nonlinear filtering, controllable processes, and martingale theory" (the name of the seminar was sometimes changed depending on the stress placed on the problems under consideration) — the 1960s and later years were very productive. This coincided in time with the intensive development of the general theory of random processes, in which a significant contribution was made by the French probabilists — primarily P.-A. Meyer, C. Dellacherie, C. Doléans, J. Jacod, M. Yor — and the Japanese scholars K. Itô, S. Watanabe, and H. Kunita. In the Soviet Union at that time, the theory and applications were developed by I. V. Girsanov, I. I. Gikhman, B. I. Grigelionis, E. B. Dynkin, M. P. Ershov, Yu. M. Kabanov, N. V. Krylov, R. S. Liptser, B. L. Rozovskii, A. V. Skorokhod, A. N. Shiryaev, and their students.

6.2. In the Kolmogorov axioms for probability theory, the main object on which all studies are carried out is a *probability space* $(\Omega, \mathscr{F}, P)$. The great fundamental achievement of the modern general theory of random processes was the recognition that it is useful to endow this probability space with an additional structure — a flow (filtration) of sub-σ-algebras $(\mathscr{F}_t)_{t\geqslant 0}$ such that $\mathscr{F}_s \subseteq \mathscr{F}_t \subseteq \mathscr{F}$, $s \leqslant t$.

The *filtered probability space* so formed,

$$(\Omega, \mathscr{F}, (\mathscr{F}_t)_{t\geqslant 0}, P)$$

was the *stochastic basis* on which stochastic analysis for random processes $X = (X_t)_{t\geqslant 0}$ with (ordered) time parameter $t \in \mathbb{R}_+$ is constructed.

The introduction of filtered probability spaces turned out to be very successful; it was due to this additional *structure* $(\mathscr{F}_t)_{t\geqslant 0}$ that the usefulness and effectiveness of such concepts as stopping time (Markov time), martingale, local martingale, compatibility (adaptiveness) relative to the flow $(\mathscr{F}_t)_{t\geqslant 0}$, optionality, predictability, quadratic characteristic, the triplet of predictable characteristics, and others were fully revealed.

6.3. The key concept of stochastic calculus in the "general theory of random processes" became the concept of a *semi-martingale* — a stochastic process $X=(X_t)_{t\geqslant 0}$ that admits a representation in the form

$$X_t = X_0 + A_t + M_t, \quad t \geqslant 0, \tag{59}$$

where $A=(A_t)_{t\geqslant 0}$ is a process of bounded variation (a "signal" in the model (4)), and $M=(M_t)_{t\geqslant 0}$ is a local martingale ("noise" in the model (4)). All the processes under consideration are assumed to be compatible with the flow $(\mathscr{F}_t)_{t\geqslant 0}$ (that is, $\mathscr{F}_t$-measurable for each $t\geqslant 0$) and have trajectories that are right-continuous for $t\geqslant 0$ and with left-hand limits for each $t>0$.

For semi-martingales one can give the definition of a *stochastic integral*

$$(\varphi\cdot X)_t = \int_0^t \varphi_s\, \mathrm{d}X_s, \quad t>0$$

(for a large store of predictable processes $\varphi=(\varphi_x)_{x\geqslant 0}$) and derive *Itô's formula* for processes $F=\big(F(X_t)\big)_{t\geqslant 0}$ in the case of functions F in the class C^2.

In a certain sense, the class of semi-martingales is the *maximal* class of processes for which one can define a stochastic integral in the case of bounded predictable functions $\varphi=(\varphi_x)_{x\geqslant 0}$, with the natural requirement that it is possible to pass to the limit under the integral sign in the Lebesgue integral (for more details see [33], [35]).

The class of semi-martingales is quite large — it contains processes (sequences) with discrete time, martingales, submartingales, supermartingales, local martingales, many diffusion Markov processes, Itô processes, Lévy processes, and others. This class is invariant relative to a change of measure, change of time, and reduction of the flow of sub-σ-algebras $(\mathscr{F}_t)_{t\geqslant 0}$. All this triggered a wide range of papers connected with semi-martingales (see, for example, the monographs [12] and [13]).

Finally, impressive results have been obtained in recent years on the application of the general theory of semi-martingales in the stochastic mathematics of finance. Thus, one of the fundamental results is stated (with some provisos of a technical nature) as follows: *In a semi-martingale financial market, arbitrage is absent if and only if there exists an equivalent martingale measure.* The author's monograph [36] is devoted to a detailed exposition of the current state of the stochastic mathematics of finance.

Bibliography

[1] A. N. Shiryaev. The detection of spontaneous effects. *Sov. Math. Dokl.*, 1961, **2**, 740–743.

[2] A. N. Shiryaev. The problem of the most rapid detection of a disturbance in a stationary process. *Sov. Math. Dokl.*, 1961, **2**, 795–799.

[3] A. N. Shiryaev. On optimal methods in quickest detection problems. *Theory Probab. Appl.*, 1963, **8**(1), 22–46.

[4] A. N. Shiryaev. On the detection of disorder in a manufacturing process, I; II. *Theory Probab. Appl.*, 1963, **8**(3), 247–265; **8**(4), 402–413.

[5] A. N. Shiryaev. On the theory of decision functions and control of a process of observation based on incomplete information. In: *Transactions of the Third Prague Conference on Information Theory, Statistical Decision Functions, Random Processes* (Liblice, 1962). Prague: Czechoslovak Academy of Sciences, 1964, 657–681 (Russian); English translation: *Select. Transl. Math. Statist. Probab.*, 1966, **6**, 162–188.

[6] A. N. Shiryaev. Some exact formulas in a 'disorder' problem. *Theory Probab. Appl.*, 1965, **10**(2), 349–354.

[7] A. N. Shiryaev. Stochastic equations of non-linear filtering by jump-like Markov processes. *Problems Inform. Transmiss.*, 1966, **2**(3), 1–18.

[8] A. N. Shiryaev. Some new results in the theory of controlled random processes. In: *Transactions of the Fourth Prague Conference on Information Theory, Statistical Decision Functions, Random Processes* (Prague, 1965). Prague: Czechoslovak Academy of Sciences, 1967, 131–203 (Russian); English translation: *Select. Transl. Math. Statist. Probab.*, 1969, **8**, 49–130.

[9] A. N. Shiryaev. Two problems of sequential analysis. *Cybernetics*, 1967, **3**(2), 63–69.

[10] A. N. Shiryaev. *Statistical Sequential Analysis: Optimal Stopping Rules*. Moscow: Nauka, 1969 (Russian).

[11] R. S. Liptser, A. N. Shiryaev. *Statistics of Random Processes*, Vol. I, II, 2nd revised and expanded edition. Berlin: Springer, 2001.

[12] R. S. Liptser, A. N. Shiryaev. *Theory of Martingales*. Dordrecht: Kluwer, 1989 (Math. Appl., Sov. Ser., **49**).

[13] J. Jacod, A. N. Shiryaev. *Limit Theorems for Stochastic Processes*. Berlin: Springer, 1987 (Grundlehren Math. Wiss., **288**).

[14] B. I. Grigelionis, A. N. Shiryaev. On Stefan's problem and optimal stopping rules for Markov processes. *Theory Probab. Appl.*, 1966, **11**(4), 541–558.

[15] S. W. Roberts. A comparison of some control chart procedures. *Technometrics*, 1966, **8**, 411–430.

[16] M. Pollak, D. Siegmund. A diffusion process and its applications to detecting a change in the drift of Brownian motion. *Biometrika*, 1985, **72**(2), 267–280.

[17] G. Lorden. Procedures for reacting to a change in a distribution. *Ann. Math. Statist.*, 1971, **42**, 1879–1908.

[18] G. V. Moustakides. Optimal stopping times for detecting changes in distributions. *Ann. Math. Statist.*, 1986, **14**, 1379–1387.

[19] M. Pollak. Optimal detection of a change in distribution. *Ann. Statist.*, 1985, **13**, 206–227.

[20] B. Yakir. A note on optimal detection of a change in distribution. *Ann. Statist.*, 1997, **25**, 2117–2126.

[21] B. Yakir, A. M. Krieger, M. Pollak. Detecting a change in regression: first-order optimality. *Ann. Statist.*, 1999, **27**, 1896–1913.

[22] A. N. Shiryaev. Minimax optimality of the CUSUM method in the case of continuous time. *Uspekhi Mat. Nauk*, 1996, **51**(4), 173–174 (Russian).

[23] *Change-Point Problems* (South Hadley, MA, 1992) (eds. E. Carlstein, H.-G. Müller and D. Siegmund). Hayward, CA: Institute of Mathematical Statistics, 1994 (IMS Lecture Notes Monograph Ser., **23**).

[24] E. S. Page. Continuous inspection schemes. *Biometrika*, 1954, **41**, 100–115.

[25] E. S. Page. Control charts with warning lines. *Biometrika*, 1955, **42**, 243–257.

[26] D. M. Hawkins, D. H. Olwell. *Cumulative Sum Charts and Charting for Quality Improvement*. New York: Springer, 1998.

[27] A. N. Shiryaev. Quickest detection problems in the 'Technical Analysis' of the financial data. In: *Mathematical Finance — Bachelier Congress* (Paris, 2000). Berlin: Springer, 2002, 487–521.

[28] R. L. Stratonovich. Conditional Markov processes. *Teor. Veroyatn. Primen.*, 1960, **5**(1), 172–195 (Russian).

[29] R. L. Stratonovich. *Conditional Markov Processes and their Application to the Theory of Optimal Control*. New York: Elsevier, 1968.

[30] E. B. Dynkin. The optimum choice of the instance for stopping a Markov process. *Sov. Math. Dokl.*, 1963, **4**, 627–629.

[31] E. B. Dynkin, A. A. Yushkevich. *Markov Processes: Theorems and Problems*. New York: Plenum Press, 1969.

[32] Y. S. Chow, H. Robbins, D. Siegmund. *Great Expectations: The Theory of Optimal Stopping*. New York: Houghton Mifflin, 1971.

[33] J. Jacod. *Calcul stochastique et problèmes de martingales*. Berlin: Springer, 1979 (Lecture Notes in Math., **714**).

[34] N. V. Krylov. *Controlled Processes of Diffusion Type*. Moscow: Nauka, 1977 (Russian).

[35] P. A. Meyer. A short presentation of stochastic calculus. Appendix to: M. Émery. *Stochastic Calculus in Manifolds*. Berlin: Springer, 1989.

[36] A. N. Shiryaev. *Essentials of Stochastic Finance. Facts, Models, Theory*. River Edge, NJ: World Scientific, 1999; 2nd corrected Russian edition: *Fundamentals of Stochastic Mathematics of Finance*, Vol. I: *Facts, Models*; Vol. II: *Theory*. Moscow: PHASIS, 2004.

Ya. G. Sinai

How Mathematicians and Physicists Found Each Other in the Theory of Dynamical Systems and in Statistical Mechanics

Translated by R. Cooke

Historians of science have yet to explain the sudden convergence of mathematicians and physicists in the late twentieth century, with the mathematicians remaining mathematicians and the physicists — of course — remaining physicists, while each began to understand the other completely and to work on the same problems, but in different ways. In my generation, the flow of mathematicians into physics began quite early. As early as my third year at university in 1954, my classmate Tikhomirov brought the news that a special topics course of Tamm was starting in the Physics Department. This was at the beginning of the well-known period when the leading theoretical physicists Landau, Leontovich, Artsimovich, Tamm, and others were invited to teach at Moscow University. Several people, myself among them, rushed to the Physics Department. I remembered Tamm speaking unbelievably rapidly and in great excitement, but my knowledge of physics was totally insufficient for understanding the gist of what he was saying. For that reason I soon stopped going. I later took Leontovich's course in electrodynamics and began to attend Landau's lectures on quantum mechanics. I don't remember why, but I also didn't stay there long.

We had heard about the mathematicians of the older generation who studied physics. In our eyes, Bogolyubov was a pure physicist. The work of

Kolmogorov on turbulence was also well known to us. Kolmogorov himself told us, his students, nothing about it. Several years later I had an occasion to ask him about turbulence. He stated that he had arrived at the hypothesis of scale invariance by studying the results of particular experiments over several months. At the time, his apartment was filled with rolls of paper covered with the numbers he was studying. What struck me was that Kolmogorov spoke of these topics as pure physicist. He could work fluently with data on the equations of state of real gases, and such concepts as (thermodynamic) entropy and the laws of thermodynamics. Gel'fand also demonstrated a mastery of physics during the talks in his seminar. But there were also other examples. Khinchin, whom we knew as one of the leading lights of probability theory, wrote two small books on the foundations of classical and quantum statistical mechanics. Neither of them was inspiring, although they enjoyed a certain measure of popularity. I never had the occasion to talk directly with Khinchin. I once attended a talk by him on queueing servers in the seminar of the Department of Probability. His talk was exemplary in the precision of the statements and clarity of the proofs. Late in life Khinchin began to study information theory, even a bit earlier than Kolmogorov, and published a well-known article in the *Uspekhi Matematicheskikh Nauk* on the entropy of a distribution and McMillan's theorem. There were also stories about Dmitrii Evgen'evich Men'shov, who attended the physics seminars but never spoke a word at them. My recollection is that the opposite process, in which physicists took an interest in mathematical results and became frequent attendees at mathematics seminars, began somewhat later, after Kolmogorov's paper on the perturbation theory of integrable Hamiltonian systems, which laid the foundation of the famous KAM (Kolmogorov–Arnold–Moser) theory, and another paper on the concept of the entropy of a dynamical system. One physicist told me that KAM theory is so natural that he couldn't understand why physicists themselves didn't think of it.

In the autumn of 1957, Kolmogorov gave a special-topics course in the theory of dynamical systems and spent three lectures on the proof of his fundamental KAM-theorem. Subsequently, this theorem appeared on the examination in classical mechanics, which Meshalkin and I took and passed as graduate students. In a certain sense this theorem is uncharacteristic of Kolmogorov's work. It requires delicate analytic estimates and rather complicated inductive reasoning. Possibly, that is why Kolmogorov never published the complete text of his proof. This was done later by Arnold.

The following year Kolmogorov conducted a seminar on these topics. In this seminar the leaders of the Soviet program on controlled nuclear fusion — Artsimovich and Leontovich — spoke once on the problem of the existence of magnetic surfaces. The presenter was Artsimovich, even though he was the experimentalist and Leontovich the theorist. The problem was subsequently solved by Arnold, who has written previously about the history of his work on this topic.

As early as his address at the Amsterdam Congress (1954), and many times subsequently, Kolmogorov stressed that ergodic theory should be used to analyze the dynamics of particular dynamical systems. In the West, ergodic theory was understood in a narrower sense, as the area of mathematics in which one studies mainly the existence of various averages. For that reason, when the number of papers investigating classical dynamical systems grew sharply, the need arose for a new term, and the words "deterministic chaos" appeared. As far as I know, this phrase was coined by Chirikov, Zaslavskii, and Ford.

Kolmogorov first spoke about the entropy of a dynamical system in one of the lectures of his special-topics course. The theorem that he proved there would be stated in modern terms as follows: *Any two Bernoulli generators of an automorphism of a measure space have the same entropy.* But in the text that was written for publication, both the approach itself and the basic theorems looked different. In the first place, Kolmogorov introduced the concept of a quasi-regular system, or, as we now say, a K-system. (For a brief time the term *Kolmogorov system* was used, but Andrei Nikolaevich himself asked that it be replaced by the abbreviation K-system.)[1]

The concept of a K-system should be regarded as a generalization of the concept of a regular stationary process in probability theory. Let $(\Omega, \mathscr{F}, P)$ be a probability space and $\{S^t\}$ a group of measure-preserving automorphisms of this space, where $t \in \mathbb{Z}$ or $t \in \mathbb{R}^1$. Then $\{S^t\}$ is called a *K-system* if there exists a σ-subalgebra $\mathscr{F}_0 \subset \mathscr{F}$ such that

1) $\mathscr{F}_t = S^t \mathscr{F}_0 \supset \mathscr{F}_0$ for $t > 0$;

2) $\bigvee_t \mathscr{F}_t = \mathscr{F}$;

3) $\bigwedge_t \mathscr{F}_t = \mathscr{N}$, where $\mathscr{N}$ is the trivial σ-subalgebra consisting of the events having probability 1 or 0.

The subalgebra $\mathscr{F}_0$ is called a *K-subalgebra*. Kolmogorov defined entropy for K-systems as the conditional entropy $H(\mathscr{F}_1 \,|\, \mathscr{F}_0)$ and proved that $H(\mathscr{F}_1 \,|\, \mathscr{F}_0)$ is independent of the choice of the K-subalgebra $\mathscr{F}_0$.

It is now clear that the introduction of entropy was not accidental. Kolmogorov had studied information theory for several years. He called the attention of mathematicians to the importance of the work of Shannon, the creator of

[1] This is an example of Kolmogorov's modesty, since the Russian word *quasi-regular* is spelled with a *K*. — *Transl.*

information theory. At the all-union mathematical congress in 1961 Kolmogorov spoke about his joint work with Gel'fand and Yaglom on the general concept of information and its properties. Over a period of several years, Kolmogorov and Tikhomirov studied problems involving the dimension of function spaces and published a well-known survey of this topic in the *Uspekhi Matematicheskikh Nauk*. All this is bound up with entropy in one way or another.

After preparing the text of his article on the entropy of a dynamical system and submitting it to the *Doklady Akademii Nauk*, Kolmogorov departed for a six-month stay in Paris. At that time I was interested in the general fact that the measure-preserving groups of transformations generate the stationary stochastic processes of probability theory when generators are chosen. Starting from this connection, I proposed a definition of the entropy of a dynamical system that later gained general acceptance. In doing so, I needed a proposition about the entropy of a generator of a partition. My argument for it was for the most part like the corresponding proposition in Kolmogorov's lecture. Equality needs to be replaced by inequality in only one place. Nowadays, this looks routine, but at the time it represented a rather non-trivial step. Moreover, it was at first not at all clear how to use this new definition and what kind of connection it had with Kolmogorov's entropy. For that reason, I had no intention of publishing the definition.

In the spring of 1958 I met Rokhlin and told him about Kolmogorov's work and my own approach to entropy. Rokhlin was very interested and proposed computing the entropy of a group automorphism of the torus. We anticipated that it would be zero, since we believed at the time that entropy could be used to distinguish the dynamical systems that arise from probability, while for classical systems it should be zero.

I sketched numerous diagrams and attempted to obtain zero, but nothing worked out. At some point I showed my diagrams to Kolmogorov, and he immediately said that the entropy should be positive. After that, I obtained the correct answer very quickly. It now became reasonable to publish my definition and computation for an automorphism of the torus; the paper was written and soon published in the *Doklady Akademii Nauk.*

Entropy led to a new point of view on the problem of isomorphism of dynamical systems, and in particular isomorphism of Bernoulli automorphisms. It now began to look like a stationary coding problem. Meshalkin constructed the first beautiful and nontrivial examples of such coding. The final solution of the problem of isomorphism of Bernoulli automorphisms was obtained by the American mathematician Ornstein, the author of other remarkable results in this area.

Soon afterwards, Rokhlin noticed a gap in Kolmogorov's proof that $H(\mathscr{F}_1 | \mathscr{F}_0)$ is independent of the choice of the K-subalgebra $\mathscr{F}_0$ and gave an example of two K-subalgebras with a different value for the conditional entropy. Kolmogorov had to modify his definition slightly, but by that time my definition had become quite generally accepted.

Kolmogorov's paper on the entropy of a dynamical system had a marvelous outcome after it was realized that entropy can also be useful for classical dynamical systems (that is, smooth vector fields and diffeomorphisms); it then became clear that such systems as group automorphisms of the torus, geodesic flows on manifolds of negative curvature, and the like, have positive entropy. Chronologically, this actually coincided with the work of Smale and Anosov on structural stability and the topological classification of systems with hyperbolic behavior. (See the article of Anosov in the present collection.) I was more interested in such statistical properties of classical systems as ergodicity, mixing, and the central limit theorem. The first step in the analysis of such properties was to prove that the entropy is positive. The work of Oseledets and Pesin on the theory of Lyapunov exponents played a large role in the development of this entire area.

Physicists began to show an interest in all this activity quite early. In plasma physics there appeared numerous examples of nonlinear dynamical systems, the study of which required both KAM theory and entropy theory. For that reason constant contacts were established with the well-known specialists in this area — Zaslavskii and Chirikov. To a certain degree, contacts with them and their students continue to this day. Chirikov proposed an empirical test for the presence of stochastic properties in two-dimensional mappings, and Zaslavskii, in joint work with Sagdeev and Filonenko, studied the width of stochastic layers. (Incidentally, only in recent years have Lazutkin and Gelfreich succeeded in justifying the formula derived in their paper.) Later on, Zaslavskii and Chirikov, together with Ford and Casati, laid the foundations of the theory of quantum chaos. One of the central points in it is Shnirel'man's theorem on the uniform distribution of the squares of the eigenfunctions of quantum systems whose classical limit is ergodic.

Two circumstances promoted the spread of the ideas of the theory of dynamical systems with stochastic behavior. In 1970 Ruelle and Takens published a paper "On the nature of turbulence," in which they proposed the concept of a strange attractor. The Navier–Stokes equations played no part in it, but the concept itself attracted attention. Strange attractors began to be detected (mostly numerically) in many systems, their Hausdorff dimension was estimated as a measure of stochasticity, and so forth. On the other hand, at the same time the

1963 paper of Lorenz, in which the system later known as a Lorenz system appeared, gained great popularity. Lorenz himself, one of the leading specialists in hydrodynamics and especially in meteorology, introduced his system of three ordinary differential equations containing simple nonlinearities on the right-hand sides as an approximation to a hydrodynamical system from convection theory. He studied it numerically and detected stochasticity. We remark that this was actually done before the papers of Smale and Anosov, although Lorenz already knew about the stochasticity of one-dimensional expanding mappings and had constructed the corresponding mapping numerically in his own case. Lorenz's paper was published in a journal devoted to atmospheric physics and remained unknown to mathematicians for a long time. I heard about it from a talk of Martin at a conference on statistical physics in Budapest in 1976. Soon afterward appeared the work of Guckenheimer and Williams on the one hand, and the papers of Afraimovich, Bykov, and Shil'nikov on the other, in which mathematical models of an attractor in a Lorenz system were proposed. In my view, the Afraimovich–Bykov–Shil'nikov model is the more geometrical of the two. On the basis of this model Bunimovich and I proved that the Lorenz system is stochastic. Very recently Tucker has obtained a computer proof of stochasticity in the original system. The ideas of chaos elicited great enthusiasm in the physicists of Gor'kii (now Nizhnii Novgorod), especially Gaponov-Grekhov, and Rabinovich. Their surveys in the *Uspekhi Fizicheskikh Nauk* and other Academy publications have promoted the spread of the ideas of chaos theory. Pikovsky and Rabinovich have proposed the "Gor'kii attractor" model. Many will recall the remarkable winter "nonlinear wave" schools near Gor'kii, where mathematicians and physicists listened patiently to each other. The majority of Gor'kii mathematicians studying the theory of dynamical systems belonged to the school of Andronov, one of the closest students of Mandel'shtam. Shil'nikov, one of the most active and prominent participants in this group, has been a constant attendee at the Moscow seminars. His article on the theory of dynamical systems can be found in the present collection.

For more than 40 years the Department of Mechanics and Mathematics at Moscow University ran a seminar on ergodic theory. For the first two years this seminar was led by Rokhlin. After he moved to Leningrad, Alekseev and I began to lead the seminar. Alekseev continued to lead it up to his death in 1980. Many mathematicians took their first steps in science in this seminar: Gurevich, Oseledets, Stepin, Katok, Margulis, Ratner, Dinaburg, Yakobson, Bunimovich, Khanin, Chernov, Zhitomirskaya, and others.

In the mid-1960s activity in the mathematical problems of statistical physics in the Department of Mechanics and Mathematics at Moscow University began to develop in parallel with the theory of dynamical systems. The initiative came from Dobrushin and Minlos. In his early career Dobrushin was one of the most active probabilists. However, at some point he concluded that there were no more big problems in probability theory, and even said so in an address at one of the conferences on probability in Vilnius. He sought out fresh mathematical problems for himself, in addition to those of information theory, in which he was already engaged. So far as I can judge, it was Gel'fand who advised Minlos to study statistical physics. The result was the appearance of the well-known seminar of Dobrushin and Minlos on statistical physics, in which I also began to participate. Somewhat later Malyshev joined this seminar. The four of us came to be regarded as the official leadership of the seminar. We began with a paper of van Hove on the existence of free energy and general theorems on systems of correlation equations. At approximately the same time there appeared a book by Ruelle devoted to rigorous statistical mechanics and covering the topics being discussed in our seminar. We arrived very quickly at the problem of phase transitions, and Dobrushin produced his first famous paper, in which he proved the existence of a phase transition of the first kind in the Ising model. A little later it was realized that a result of this type had been obtained earlier in a slightly different form, and on the level of rigor accepted in physics, by the famous British physicist Peierls. Our research group was soon joined by Berezin, with whom I wrote a paper containing a generalization of Dobrushin's result to a larger class of models. After giving a talk in our seminar on Onsager's solution of the two-dimensional Ising model, Berezin advanced the idea that general two-dimensional ferromagnetic models can be integrated in the same sense as the Ising model, and for this purpose he attempted to apply the machinery of integration over Grassmann variables that he had developed. His article on these topics was published in the *Uspekhi Matematicheskikh Nauk* and is frequently cited in the literature to this day, although the ultimate purpose was not achieved. A new impetus to this area was recently given in a paper of Pinson and Spencer. As for phase transitions of the first kind, it was soon realized that the central role in the proofs of Dobrushin and Peierls was played by the concept of the ground state and the so-called Peierls' inequality. Pirogov and I succeeded in giving a more general form to the corresponding constructions, which led to our theory, now widely known among specialists in mathematical statistical physics.

The analysis of systems of correlation equations for correlation functions led to the general concept of a limiting Gibbs random field. This was first done in our seminar by Minlos for lattice systems at high temperature. Dobrushin then proposed a general definition of a Gibbs random field in terms of conditional distributions in bounded domains under given boundary conditions.

At approximately the same time, analogous definitions were given by Lanford and Ruelle, and the corresponding relations are known in the literature as the DLR equations. While in Paris, Bogolyubov learned of all this activity from Ruelle and recalled that as early as 1947 he had published a joint paper with his student Khatset, in which systems of correlation equations on the entire space also appeared. Later there appeared a paper of Bogolyugov, Petrina, and Khatset containing an updated exposition of the earlier paper of Bogolyubov and Khatset.

The general theory of Gibbs random fields was expounded by Dobrushin in three articles in *Functional Analysis*, which also contained new examples of phase transitions of the first kind. I remark in passing that the idea of a phase transition of the first kind in antiferromagnetic models was stated by Kolmogorov immediately after Dobrushin's presentation at a meeting of the Moscow Mathematical Society. From the general point of view, Gibbs random fields amount to the natural concept of a Markov field with two-dimensional time. Gibbs fields immediately began to occupy a central place in probability theory. Dobrushin himself devoted a great deal of time to developing the conditions for uniqueness of such a field. He is the creator of the general thesis that all natural random fields are Gibbsian in a sufficiently broad sense of the term. Dobrushin continued to reflect on this problem to the last years of his life.

It is difficult to say to what extent physicists have mastered the ideas of a Gibbs random field. Most likely, they have missed the relevant questions, regarding them as unimportant. Of course, phase transitions were known in physics in connection with the structure of the set of ground states, but Dobrushin's result on the existence of non-translationally-invariant states (the so-called *Dobrushin phases*) in three-dimensional lattice models was new even to physicists.

Minlos and I published two long articles (of 100 pages each) on condensation in the Ising model. A student of mine remarked to me that one shouldn't publish such long articles, since nobody will read them. He turned out to be wrong. In our article we had to choose a value for a rather small constant, which we took to be 1/333, writing that it was named in honor of Jürgen Moser. A little later there appeared an article by Gallavotti, in which he wrote that the Moser constant in his case was chosen equal to 1/33333. Definitive results in this area were obtained in papers by Dobrushin, Kotecky, and Shlosman on the Wulff droplet, that is, the exact shape of the volume occupied by the given phase.

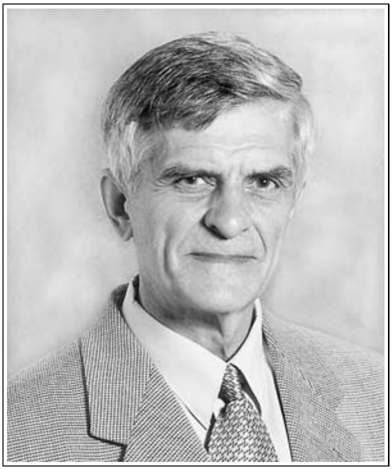
Many young mathematicians who have now become famous matured in our seminar on statistical physics: Sukhov, Shlosman, Blekher, Pirogov, Gertsik, Anshelevich, and others. The duration of the individual sessions of the seminar eventually reached four hours. Frequently, the leaders and participants would interrupt the speaker and give their own explanation of what was happening. Physicists spoke many times in the seminar, and a number of papers by participants in the seminar were devoted to the mathematical justification of their results.

For me the seminar in statistical physics was also important because the ideas of the Gibbs approach and the thermodynamical formalism found application in the theory of hyperbolic dynamical systems. (See my paper "Gibbs measures in ergodic theory.") In particular, the now well-known concept of an SRB-measure is permeated with statistical physics. The papers of Ruelle and Bowen played a large role in the development of this area.

A question which is now frequently discussed is: How should mathematicians study theoretical physics in general? It is not possible to give a single prescription on this score. Dobrushin once remarked that every mathematician invents his own physics. This, of course, is an extreme view. Many mathematicians make a distinction between theoretical and mathematical physics. By theoretical physics they understand mainly quantum field theory and general relativity. The majority of physicists working in these areas have an excellent mastery of topology, algebraic geometry, and complex analysis, and here the boundary between mathematicians and theoretical physicists is very arbitrary. The leading theoretical physicists in these areas are regular participants in mathematical congresses and conferences.

Mathematical physics is studied by mathematicians who obtain rigorous mathematical theorems and physicists who obtain rigorous results. The problems come from physics, and the choice of a problem requires knowledge of both physics and mathematical methods. The mathematicians recognize that the results they are obtaining are not new from the point of view of a physicist. Nevertheless, the relationship here resembles that between pop music and classical music. Pop music has many fans, but classical music lasts through the ages. Another hidden danger of mathematical physics is the level of achievement. Some years ago I proved a theorem in the theory of phase transitions. One Thursday, arriving at Landau's seminar in the Institute, I chanced upon a talk about a paper by a Western physicist, in which the same result was obtained, but without precise expansions, estimates, and the like. A complicated question arises: Was it necessary to prove that theorem? Since then, in this situation and others like it, I have always answered this question affirmatively for myself, for one simple reason: the pleasure of a well-proved theorem is the highest possible.

Still, there is a virtual boundary, highly individual, separating the problems where rigor is needed and those where it is superfluous. Each person chooses that boundary in his own way.



V. M. Tikhomirov

Approximation Theory in the Twentieth Century

Translated by R. Cooke

Introduction

One of the fundamental problems of approximation theory is the quantitative and qualitative estimation of the devices that transform infinite information into finite information, a problem that is inherent in the concepts of number, set, and mapping. In principle, approximation theory is meant to form the theoretical underpinning of computational mathematics.

As a separate chapter in analysis, approximation theory was born approximately 150 years ago. At its inception the research of Chebyshev, Weierstrass, Lebesgue, Landau, and Vallée Poussin played the leading role. In the twentieth century this theory was developed primarily in Russia and the former Soviet Union. A fundamental role in its rise was played by outstanding Russian/Soviet mathematicians – P. L. Chebyshev, S. N. Bernshtein, and A. N. Kolmogorov. They and their students formed several active schools in Saint Petersburg/Leningrad, Moscow, Sverdlovsk/Ekaterinburg, Ukraine (Khar'kov, Dnepropetrovsk, Kiev), and a number of other republics of the USSR.

One can distinguish three periods in approximation theory – the Chebyshev period, the Bernshtein period, and the Kolmogorov period. Among the ideas of Chebyshev and his successors were problems of *best approximation of* *particular elementary functions by fixed approximation methods* (mostly algebraic polynomials), and *exact solution of certain extremal problems of analysis theoretically connected with approximation problems*. The time frame of the Chebyshev period is the second half of the nineteenth century, but the task of finding exact solutions to various kinds of extremal problems imbued with approximative content was discussed actively in the twentieth century as well, right down to the present.

In the early twentieth century, Bernshtein began to develop a broad program of research on *the connection between the smoothness of a function and the rate of its approximation by algebraic and trigonometric polynomials*. In this program approximations by certain particular methods were studied, but the emphasis was now on approximation of collections of functions rather than individual functions, on functional spaces and subsets (function classes) contained in them. Bernshtein gave a name to this new branch of approximation theory: *constructive function theory*. This theory was being developed intensively still in the 1970s.

In the mid-1950s, the theory received a new impetus from Kolmogorov. He significantly enlarged the scope of approximation theory, posing problems involving *optimal means and methods of approximation, coding, and recovery of functions*. These problems were discussed actively right up to the 1990s. At present the search is on for new topics, but it is too early to speak of a new period.

Our purpose here is to discuss several selected fundamental themes of approximation theory (mostly developed in the twentieth century) from the modern point of view and to outline certain plans for the future.

The article consists of four sections. In the first three the subject will be the above three periods of approximation theory, and the fourth contains supplementary material and a summary.

1. Extremal Problems of Approximation Theory (The Circle of Chebyshev's Ideas)

Approximation theory began in memoirs of Chebyshev in 1854 and 1859 [8, pp. 23–51, 152–236].

In this section we shall mostly tell about the exact solutions of extremal problems connected with approximation theory. We shall state four types of problems that served as the source of numerous investigations. (The original problems were stated by Chebyshev.)

P. L. Chebyshev
(1821–1894)

Statements of the problems. 1) In his first memoir on approximation theory "The theory of mechanisms known as parallelograms" (1854) [8, pp. 23–51] Chebyshev posed the problem of *best uniform approximation of a continuous function defined on a closed interval using algebraic polynomials of a given degree.* Here is a statement of that problem in modern notation:

$$\|y(\cdot)-x(\cdot)\|_{C([a,b])} \to \min, \quad x(\cdot)\in\mathscr{P}_n, \qquad (1.1)$$

where $\mathscr{P}_n$ is the space of algebraic polynomials of degree not greater than n and $y(\cdot)$ is a given continuous function on $[a,b]$.

Chebyshev gave a double motivation for the problem (1.1): On the one hand, the connection of this problem with the construction of special mechanisms of the type of Watt's parallelogram; on the other hand, the needs of computational mathematics.

Particular results on exact solutions of the problem (1.1) were obtained by Chebyshev, Andrei Markov, Zolotarev, and others (see [11]).

In the 1930s problems involving the approximation of individual functions by fixed means of approximation were translated into geometric language and the language of functional analysis. Bernshtein, Kolmogorov, Nikol'skii, Stechkin, and many others devoted papers to the study of various aspects of this general problem (see the survey [10]).

2) In his article "On a problem posed by Mendeleev" (1889) [23, pp. 51–75] Markov called the attention of mathematicians to a new circle of extremal problems closely connected with approximation theory — *inequalities for the derivatives of polynomials.*

Here are two typical problems on extremal properties of polynomials:

$$x^{(k)}(\tau) \to \max, \quad \|x(\cdot)\|_{C([-1,1])} \leqslant \delta, \quad x(\cdot)\in\mathscr{P}_n. \qquad (1.2a)$$

$$\|x^{(k)}(\cdot)\|_{C([-1,1])} \to \max, \quad \|x(\cdot)\|_{C([-1,1])} \leqslant \delta, \quad x(\cdot)\in\mathscr{P}_n. \qquad (1.2b)$$

The problems (1.2a) and (1.2b) were posed for $k=1$ and $n=2$ by Mendeleev, who encountered them in solving the problem of optimal dilution of alcohol in the preparation of vodka (so that these problems also arose "in practice").

A. A. Markov
(1856–1922)

The problems (1.2a) and (1.2b) were studied for $k=1$ and any n by Andrei Andreevich Markov (in the abovementioned paper) and for arbitrary k and n by his brother Vladimir Andreevich Markov [24]. It should be noted, however, that Chebyshev had studied the problem (1.2a) with $k=0$, $\tau \in \mathbb{R}\setminus[-1,1]$ earlier (1883). We shall call this problem *Chebyshev's polynomial extrapolation problem*. (Extrapolation problems, in which a prediction of the behavior of a function outside the set, on which some information about it exists, remains a current problem in applications.) Extremal problems in spaces of polynomials were studied by Bernshtein's successors in Leningrad. Similar problems in spaces of splines were studied actively in Ukraine by the school of N. P. Korneichuk (see [16]–[17]).

3) In 1913 Edmund Landau proved the following sharp inequality:

$$\|\dot{x}(\cdot)\|_{C^b(\mathbb{R}_+)} \leqslant 2\|x(\cdot)\|^{1/2}_{C^b(\mathbb{R}_+)}\|\ddot{x}(\cdot)\|^{1/2}_{L_\infty(\mathbb{R}_+)}. \tag{1.3}$$

Here $C^b(\mathbb{R}_+)$ is the Banach space of bounded continuous functions on $\mathbb{R}_+$ with the sup-norm.

One of the most remarkable inequalities similar to (1.3) was proved by Kolmogorov [14, pp. 252–263], and for that reason inequalities of the form

$$\|x^{(k)}(\cdot)\|_{L_q(T)} \leqslant K\|x(\cdot)\|^{\alpha}_{L_p(T)}\|x^{(n)}(\cdot)\|^{\beta}_{L_r(T)} \tag{1.3$'$}$$

(where $0 \leqslant k < n$ are integers, $1 \leqslant p,q,r \leqslant \infty$, $\alpha,\beta \geqslant 0$, $T=\mathbb{R}$ or $\mathbb{R}_+$) are called *Landau–Kolmogorov inequalities*. (Kolmogorov found a sharp constant for $p=q=r=\infty$, $T=\mathbb{R}$.) For a fixed T the inequalities $(1.3')$ depend on five parameters — n, k, p, q, r. (The quantities α and β are uniquely determined by the other five: $\alpha = \bigl(n-k-\frac{1}{r}+\frac{1}{q}\bigr)\big/\bigl(n-\frac{1}{r}+\frac{1}{p}\bigr)$, $\beta = 1-\alpha$.) A great deal of research has been devoted to inequalities of Landau–Kolmogorov type, and this is because, in particular, they play an important role in approximation theory, for example, in Stechkin's problem of the approximation of unbounded operators by bounded operators (for that reason, many papers devoted to Landau–Kolmogorov inequalities were written in the Sverdlovsk school of Stechkin — see [2]), and in problems involving optimal recovery. However, the love that mathematicians have for the proof of sharp inequalities cannot be explained only by pragmatic purposes; in many respects it has aesthetic roots.

4) In the mid-1930s Favard (1936) solved the following problem (generalizing results of Harald Bohr and Bernshtein):

$$\begin{gathered}\|x(\cdot)\|_{C(\mathbb{T})} \to \max, \quad x(\cdot) \in W_\infty^r(\mathbb{T}), \\ \int_{\mathbb{T}} x(t)\cos kt\,\mathrm{d}t = \int_{\mathbb{T}} x(t)\sin kt\,\mathrm{d}t = 0, \quad 0 \leqslant k \leqslant n-1.\end{gathered} \tag{1.4}$$

Here $\mathbb{T}$ is a circle realized as the closed interval $[-\pi, \pi]$ with endpoints identified, $W_\infty^r(\mathbb{T})$ is the class of functions $x(\cdot) \in C^{r-1}(\mathbb{T})$ whose derivative $x^{(r-1)}(\cdot)$ satisfies a Lipschitz inequality with constant equal to 1. The value of the maximum turned out to be K_r/n^r, where K_r are known constants. The inequality that follows from (1.4) is called the *Bohr–Favard inequality*.

Favard's research received a lot of development by the efforts of Akhiezer, Krein, Szőkefalvi-Nagy, Nikol'skii, Stechkin, Boyanov, and many others. This topic is partly expounded in the monograph [1].

Together with the extremal problems for smooth functions like (1.4), problems of a similar type were solved for analytic functions, such as:

$$|f(\zeta)| \to \max, \quad f(\cdot) \in H_\infty(D), \quad f^{(k)}(0) = 0, \quad 0 \leqslant k \leqslant n-1, \tag{1.4$'$}$$

where $H_\infty(D)$ is the Hardy class of functions that are analytic in the unit disk D and bounded by 1 on the disk. Problems of the type (1.4) and (1.4$'$) turned out to be closely connected with problems of optimal approximation and recovery of smooth and analytic functions. This will be discussed below.

Discussion of the solutions. The problems (1.1)–(1.4) and various generalizations of them form a significant portion of the extremal problems that have been studied throughout the whole history of approximation theory. In the overwhelming majority of cases these problems were solved individually, by methods created specially for a given particular problem. It is natural to try to survey the whole collection of these problems from the unified point of view of extremum theory. The set of exactly solved problems of analysis and geometry (in particular, approximation-theory problems) is a natural testing ground for extremum theory, and I seem to see in it one of the essential motives for finding exact solutions. Extremum theory gives general approaches to the solution of optimization problems of various nature. Let us examine how it applies to the problems described above.

To almost all the problems discussed above, the *Lagrange principle for necessary conditions* can be applied. This principle says that if a problem can be formalized as the problem of minimizing a functional with equality constraints (and smooth convex structures are present in the problem), one should form the

Lagrangian for the problem, and then a necessary condition for an extremum will be a certain analog of Fermat's theorem. The essence of the principle is that the tangent space, or hyperplane of support in the convex case, drawn to the region above the graph of the Lagrangian at an extremum must be horizontal. In convex problems the necessary conditions (in nondegenerate cases) coincide with the sufficient conditions: there exist Lagrange multipliers at which the Lagrangian attains a minimum (in a problem without constraints, if all the constraints were included in the definition of this function, or with respect to those constraints that were not included in the function) at an element that is a solution of the problem. (One can read more about the Lagrange principle in [22].)

This fundamental and simple idea is applicable to the problems (1.1)–(1.4). We can illustrate it in sufficient detail using the problems (1.1) and (1.2). The problem (1.1) admits the following formalization:

$$f(x) \to \min, \quad x \in \mathbb{R}^{n+1}, \quad f(x) = \max_{t\in[a,b]} F(t,x),$$

where $F(t,x) = \left|y(t) - \sum_{k=1}^{n+1} x_k t^{k-1}\right|$ and $x = (x_1,\dots,x_{n+1})$. This is a convex problem without constraints. An element $\widehat{x}$ is a solution of it if and only if there exists a horizontal hyperplane that supports the region above the graph of f at the point $\bigl(\widehat{x}, f(\widehat{x})\bigr)$. (This is the analog of Fermat's theorem for convex problems without constraints.) The function f is a maximum of the family of affine functions $x \mapsto \pm\bigl(y(t) - \sum_{k=1}^{n+1} x_k t^{k-1}\bigr)$. It is not difficult to see that if $\widehat{x}$ minimizes the function f, then the normal vector to the horizontal hyperplane of support is the convex hull of at most $n+2$ normals to functions of the family $\bigl\{F(t,\cdot)\bigr\}_{t\in[a,b]}$. (This is the content of Carathéodory's theorem in convex geometry.) As a result, we find that there exist an integer $r \leqslant n+2$ and r points $\tau_i \in [a,b]$ such that

$$\sum_{i=1}^{r} \alpha_i F_x(\tau_i,\widehat{x}) = 0 \iff \sum_{i=1}^{r} \mu_i \bigl(y(\tau_i) - \widehat{x}(\tau_i)\bigr)x(\tau_i) = 0 \quad \forall x(\cdot) \in \mathscr{P}_n, \quad \mu_i > 0, \tag{i}$$

and here $|y(\tau_i) - \widehat{x}(\tau_i)| = \|y(\cdot) - \widehat{x}(\cdot)\|_{C([a,b])}$. This is the test for a minimum in (1.1). From it we deduce a second test, known as the *Chebyshev alternance theorem*. According to this theorem the test for a solution in the problem (1.1) is the existence of $n+2$ points $\{\tau_k\}_{k=1}^{n+2}$ at which the function $y(\cdot) - \widehat{x}(\cdot)$, alternating its signs, assumes its maximal and minimal value. (In this case we say that the difference $y(\cdot) - \widehat{x}(\cdot)$ has $(n+2)$-*alternance*.)

If we assume that $r \leqslant n+1$ in formula (i), then by inserting the polynomial $x_1(t) = \prod_{i=2}^{r}(t - \tau_i)$ into the identity (i), we find that $\mu_1 = 0$, which contradicts the inequality $\mu_1 > 0$. Thus $r = n+2$. And the assumption that alternance does not occur — that is, there exists an index j, $1 \leqslant j \leqslant n+1$, such that $\big(y(\tau_j) - \widehat{x}(\tau_j)\big)\big(y(\tau_{j+1}) - \widehat{x}(\tau_{j+1})\big) > 0$ — also leads to a contradiction, since if we then substitute the polynomial $x_2(t) = \prod_{i-1,i\neq j,j+1}^{n+2}(t-\tau_i)$ into (i), we obtain $0 \neq 0$. This proves the necessity in the alternance theorem. The sufficiency follows from the fact that in convex problems nondegenerate necessary conditions are also sufficient. In a similar manner one can obtain the tests for polynomials of best approximation proved in the papers of Bernshtein, Kolmogorov, and others mentioned above.

An immediate corollary of the alternance theorem is an explicit expression (found by Chebyshev) for the polynomial of degree n with leading coefficient equal to 1 that is closest to zero in the uniform metric on $[-1,1]$. The expression is $2^{-(n-1)}T_n(t)$, where $T_n(t) = \cos(n \arccos t)$ is the *Chebyshev polynomial* of degree n. It is not difficult to show that a polynomial of degree n with $(n+1)$-alternance (and indeed, any polynomial that attains its sup-norm at $n+1$ points) is proportional to T_n. Zolotarev found explicit expressions in terms of elliptic functions for the polynomials closest to zero having the first two coefficients fixed. They are called the *Zolotarev polynomials* and are characterized by having n-alternance. Explicit expressions for polynomials and rational functions of best approximation to several particular functions were found by Chebyshev, Markov, and Zolotarev.

In discussing explicit solutions obtained in Chebyshev's school, we recall the words of Hadamard: "The shortest path between two real truths passes through the complex plane." Indeed, in practically all the cases studied in Chebyshev's school, the difference $\xi(\cdot) = y(\cdot) - \widehat{x}(\cdot)$ (between the function being approximated and the polynomial of best approximation) can be represented in parametric form: $\xi = f(w)$, $t = g(w)$ in such a way that the function $t \mapsto \xi(t)$ has the alternance required by the Chebyshev test on $[-1,1]$. For example, the polynomial $T_n(\cdot)$ is representable in the form $T_n(t) = 2^{-n}(w^n + w^{-n})$, $t = 2^{-1}(w + w^{-1})$. Here the expressions for the best approximations are dictated more or less explicitly by the statement of the problem.

Let us now pass to our second topic — inequalities for the derivatives of polynomials. We shall slightly generalize the problem (1.2a) by considering an arbitrary linear functional $l\big(x(\cdot)\big)$, $x(\cdot) \in \mathscr{P}_n$ instead of the value of the k-th derivative at a point. This problem admits the following formalization:

$$l(x) \to \max, \quad f_1(x) \leqslant \delta, \quad x = (x_1, \ldots, x_{n+1}), \quad x(t) = \sum_{j=1}^{n+1} x_k t^{k-1}, \quad x(\cdot) \Leftrightarrow x,$$

where $f_1(x) = \max_{t\in[-1,1]} \big| \sum_{j=1}^{n+1} x_k t^{k-1} \big|$.

This is a convex problem with constraints. According to Lagrange's principle for such problems (which we apply heuristically), if $\widehat{x}$ is a solution of the problem, then there exists a Lagrange multiplier $\lambda > 0$ such that the Lagrangian $\mathscr{L}(x, \lambda, -1) = -l(x) + \lambda f_1(x)$ attains an absolute minimum at $\widehat{x}$, that is, once again, the hyperplane of support to the region above the graph of the Lagrangian at the point $\widehat{x}$ must be horizontal. Using the fact that a polynomial of degree n cannot attain its extreme values on an interval at more than $n+1$ points (or again using Carathéodory's theorem), we arrive at an identity analogous to (i):

$$l\big(x(\cdot)\big) = \sum_{i=1}^{r} \mu_i \widehat{x}(\tau_i) x(\tau_i) = 0, \quad x(\cdot) \in \mathscr{P}_n, \quad r \leqslant n+1, \tag{ii}$$

and moreover $\big|\widehat{x}(\tau_i)\big| = \delta$, $1 \leqslant i \leqslant r$.

If we return to the functional $x^{(k)}(\tau)$, one can easily deduce from (ii) that the only solutions of the problem are the Chebyshev and Zolotarev polynomials. We shall show how to do this in the problem of extrapolating polynomials. (The solution in this case will be the Chebyshev polynomials.) If we assume that $r \leqslant n$, we must substitute into (ii) (with $l\big(x(\cdot)\big) = x(\tau)$, $|\tau| > 1$) the polynomial

$$x_3(t) = \prod_{k=1}^{r} (t - \tau_k), \tag{iii}$$

and we arrive at a contradiction immediately: $0 \overset{\text{(iii)}}{\neq} x_3(\tau) \overset{\text{(ii)}}{=} 0$. Thus the extremal polynomial attains its norm at $n+1$ points and so must be equal either to $\delta T_n(\cdot)$ or $-\delta T_n(\cdot)$. One can easily describe *all* linear functionals whose solutions are polynomials proportional to the Chebyshev polynomials. Just as simply as one solves the problem of extrapolating polynomials, one can solve the problem (1.2a) for trigonometric polynomials $\mathscr{T}_n$ of degree n, and thereby prove the well-known inequality of Bernshtein $\big(\|\dot{x}(\cdot)\|_{C(\mathbb{T})} \leqslant n\|x(\cdot)\|_{C(\mathbb{T})}\big)$ for trigonometric polynomials of degree n. In a similar way one can prove many other inequalities for trigonometric polynomials, for example, the Bernshtein–Stechkin inequality, in which the derivatives are replaced by difference quotients and the like.

The analog of Bernshtein's inequality for algebraic polynomials of degree n is the inequality of Vladimir Markov

$$\|x^{(k)}(\cdot)\|_{C([-1,1])} \leqslant T_n^{(k)}(1)\|x(\cdot)\|_{C([-1,1])}$$

(for $k = 1$ this inequality was proved by Andrei Markov), which solves the problem (1.2b) and can also be obtained by applying Lagrange's principle. But since the problem (1.2b) is not convex, one is forced to apply second-order extremum conditions.

We note a difference in the exact constants in the inequalities of Bernshtein and A. Markov (n and $\dot{T}_n(1) = n^2$). The explanation of this phenomenon also "passes through the complex plane." (This fact was established by Szegő.)

We cannot discuss in detail how problems involving inequalities of Landau–Kolmogorov type are solved using Lagrange's principle. In this topic there are two branches: the so-called *general solutions* (when inequality (1.3′) is being solved for all n and k with fixed p, q, and r) and *particular solutions* (obtained for fixed n and k for certain domains in the parameter set (p,q,r)). There are altogether six general solutions. The article [21] is devoted to them and their generalizations. Certain particular solutions (the most fundamental from our point of view) were discussed in [22]. Actually, all solutions can be obtained by applying Lagrange's principle. We shall discuss the problem (1.4) in Section 3 below.

A fundamental idea, which in my opinion belongs to the present section, can be stated as follows: *The majority of extremal problems, considered by classicists and their successors in approximation theory, are solvable by use of the Lagrange principle* (and that principle makes it possible to enlarge considerably the circle of solvable problems). But a number of problems, in particular, problems solved in the Ukrainian school of Korneichuk (see [16]–[17] and [4]), have not been given an adequate expression in general extremum theory, although one may hope that this will yet happen.

2. Smoothness and Approximation (The Circle of Ideas of S. N. Bernshtein)

The source of development of the second branch of approximation theory was the dissertation of Jackson [12] (directed by Landau) and the memoir of Bernshtein (1912) [6, pp. 11–104] "On best approximation of continuous functions by polynomials of a given degree" (written under the influence of ideas of Vallée Poussin — see [30]). This memoir was awarded a prize by the Belgian Academy of Sciences.

S. N. Bernshtein
(1880–1968)

The present section is devoted mainly to this particular problem — *The connection between the smoothness of functions and their rate of approximation by various methods.* A brief summary here goes as follows: *Smoothness generates a certain corresponding rate of optimal approximation, and conversely*; *moreover the optimal approximation is, as a rule, based on different methods of smoothing and/or summation of series.*

The Weierstrass theorems and their development. In 1885 two fundamental results were proved [32]:

Karl Weierstrass
(1815–1897)

A function that is continuous on a finite closed interval can be uniformly approximated with arbitrary precision on that interval by algebraic polynomials (Weierstrass' first theorem), and *a continuous periodic function can be uniformly approximated with arbitrary precision by trigonometric polynomials* (Weierstrass' second theorem).

Algebraic and trigonometric polynomials were the principal means of approximation in the early stages of development of the theory. Bernshtein began to develop a new technique for approximation — entire functions of finite degree, that is, entire functions $x(\cdot)$ satisfying an inequality $|x(z)| \leqslant C\big(x(\cdot)\big)e^{\sigma|\Im z|}$. The space of these functions is denoted $\mathscr{B}_\sigma(\mathbb{R})$. The following theorem of Weierstrass type is due to Bernshtein: *A necessary and sufficient condition for a function defined on* $\mathbb{R}$ *to be approximable by entire functions of exponential type is that it be bounded and uniformly continuous.*

The original methods of approximation theory were developed in proofs of the Weierstrass theorems. The main one was the *smoothing method*. The function being approximated (or some extension of it) was convoluted with some approximate identity, usually with a family of functions $\varphi_\lambda(\cdot)$, $\lambda \in \Lambda = \mathbb{N}$ or $\mathbb{R}_+$ such that $\varphi_\lambda(t) \geqslant 0\ \forall t$, $\int_T \varphi_\lambda(t)\,dt = 1$, $T = \mathbb{R}$ or $\mathbb{T}$, and for all $\varepsilon > 0$, $\delta > 0$, there exists Λ_0 such that $\int_{|t|>\delta} \varphi_\lambda(t)\,dt < \varepsilon$ for all $\lambda \in \Lambda \setminus \Lambda_0$. It is then easy to show that if $x(\cdot) \in C(\mathbb{T})$ (or $x(\cdot) \in C^b(\mathbb{R})$ and $x(\cdot)$ is uniformly continuous on $\mathbb{R}$, then the following convergence holds: $x_\lambda(\cdot) = x * \varphi_\lambda(\cdot) \to x(\cdot)$ (uniformly as $\lambda \to \infty$ in the case $\Lambda = \mathbb{N}$, or $\lambda \to 0$ in the case $\Lambda = \mathbb{R}_+$). Weierstrass convoluted a continuous function $x(\cdot)$ on a finite closed interval (extending it to a function of compact support) with Gauss–Poisson kernels $\varphi_\lambda(t) = (2\pi\lambda)^{-1/2}\exp(-t^2/2\lambda)$. (These kernels are entire functions of t, and consequently $x_\lambda(\cdot)$ is also an entire function. The function $x_\lambda(\cdot)$ was then approximated by a partial sum of its Taylor series. In this way Weierstrass proved his first theorem.) Landau and Vallée Poussin proved the Weierstrass theorems by convoluting functions with polynomial kernels.

Two known proofs stand a bit apart from all the rest, although the essence of them is the same (smoothing). Bernshtein's 1912 proof [6, pp. 105–106] of the first theorem (which made a great impression on contemporaries) was based on probability theory. A function defined on $[0,1]$ was approximated by Bernshtein using the polynomials $B_n x(t) = \sum_{k=0}^{n} \binom{n}{k} x\left(\frac{k}{n}\right) t^k (1-t)^{n-k}$, which are called *Bernshtein polynomials*, and the proof of convergence was based on the law of large numbers.

Lebesgue's 1899 proof was based on the fact that (a) a continuous function on a closed interval can be uniformly approximated by a broken line (a piecewise linear function); (b) a broken line is a linear combination of shifts of the function $t \mapsto N(t) = |t|$, $t \in [0,1]$; (c) the function $N(t) = \sqrt{t^2} = |t| = \big(1-(1-t)\big)^{1/2} = 1-(1/2)(1-t^2)+\cdots$ can be uniformly approximated on $[-1,1]$ by a series of polynomials (obtained from the binomial expansion).

Simultaneously with Weierstrass' theorems (in the same year 1885), their complex analogs were found (Runge's theorem).

The theorems just discussed were further developed. We give two finished results. The first encompasses approximation by algebraic polynomials in the multi-dimensional case; the second still awaits a multi-dimensional generalization.

Let T be a compact space and A an algebra of real-valued functions containing the constants and separating points of the compact space (*for all $t_1, t_2 \in T$ there exists $x(\cdot) \in A$ such that $x(t_1) \neq x(t_2)$*). *Then any continuous function on T can be uniformly approximated by elements of A* (the Stone–Weierstrass theorem).

A function $x(\cdot)$ defined on a compact subset $T \subset \mathbb{C}$, continuous on T, and holomorphic in $\operatorname{int} T$ *can be uniformly approximated by polynomials if and only if $\mathbb{C} \setminus T$ is connected and contains the point at infinity* (Mergelyan's theorem).

Bernshtein emphasized repeatedly that constructive function theory arose "on the basis of a synthesis of the ideas of two great mathematicians of the past century — Weierstrass and Chebyshev." His ulterior motive for developing this theory was the hope of reconstructing all of classical analysis. Inspired by his early successes in the development of a new branch of approximation theory, he wrote that from the moment Weierstrass proved his theorem on approximation of continuous functions by polynomials, "the theory of functions of a complex variable, which had by then reached the summit of its development, gradually receded into the background, moving forward the study of functions of a real variable." Fortunately, these hopes were not fated to be realized, but the impact of constructive function theory in analysis turned out to be quite significant.

We shall now take up one of the fundamental topics of constructive function theory — the interconnections between the smoothness of a function and the rate of approximation of it. Let us briefly touch on the contents of the main terms: *smoothness* and *means of approximation*.

Smoothness. There exist two main approaches to smoothness. One is based on the local behavior of functions and is defined in terms of the moduli of continuity, moduli of smoothness, and the like. The other is based on global characteristics of functions, defined mainly using the methods of harmonic analysis. The evolution of the concept of smoothness along the first path (which began with the research of Jackson and Bernshtein) led to the definition of the Besov spaces $\mathscr{B}^{\alpha}_{p\theta}$, characterized by three parameters. Subsequently, Lizorkin and Triebel constructed another three-parameter family that contained the majority of spaces of smooth functions defined in other ways. But we shall describe in detail an intermediate stage and define two two-parameter families of spaces of smooth functions on the one-dimensional manifolds $\mathbb{T}$ and $\mathbb{R}$.

On the basis of the first approach, one constructs the *Hölder–Nikol'skii* spaces $\mathscr{H}^{\alpha}_{p}(T)$, where $T=\mathbb{T}$ or $\mathbb{R}$. They consist of functions $x(\cdot)\in L_p(T)$ such that $\omega_r\big(t,x(\cdot,L_p(T)\big)\ll t^{\alpha}$, where $r>\alpha$ and $\omega_r\big(t,x(\cdot),L_p(T)\big)$ is the modulus of smoothness of order r (equal to $\sup_{|h|\leqslant t}\big\|\sum_{k=0}^{n}(-1)^k\binom{n}{k}x(\cdot+kh)\big\|_{L_p(T)}$). In the second approach, the *Sobolev spaces* $\mathscr{W}^{\alpha}_{p}(T)$ were defined. Here is one possible definition of them: $\mathscr{W}^{\alpha}_{p}(T)$ consists of generalized functions on T for which $\mathscr{D}^{\alpha}x(\cdot)\in L_p(T)$, where $\mathscr{D}^{\alpha}$ is a fractional-derivative operator in some sense. For example, on the circle the most widely known method is *Weyl differentiation*, which assigns to the generalized function $x(\cdot)=\sum_{k\in\mathbb{Z}}x_k\mathrm{e}^{\mathrm{i}k}$ the function $D^{\alpha}x(\cdot)=\sum_{k\in\mathbb{Z}\setminus\{0\}}(\mathrm{i}k)^{\alpha}x_k\mathrm{e}^{\mathrm{i}k}$, $\alpha\in\mathbb{R}$, $(\mathrm{i}k)^{\alpha}=|k|^{\alpha}\mathrm{e}^{\frac{\pi\mathrm{i}}{2}\operatorname{sgn}\alpha}$.

Means of approximation. Up to now only two such means — algebraic and trigonometric polynomials — have been considered, and we have mentioned spaces of entire functions of finite degree.

As a means of approximation, trigonometric polynomials arose simultaneously with the birth of classical analysis; the spaces $\mathscr{B}_{\sigma}(\mathbb{R})$ as a means of approximating functions defined on the entire line were introduced by Bernshtein in the second decade of the twentieth century. Bernshtein laid out an extensive program of research into approximation by this new machinery. He wrote, “The theory of optimal approximation using functions of finite degree is an essential complement and development of the theory of optimal approximation by means of polynomials.”

We would like to convince the reader that the theory of approximation of periodic functions by trigonometric polynomials and the theory of approximation of functions on the line by entire functions of finite degree are essentially the same. This unity is based primarily on the very definition of these spaces. The trigonometric polynomials $\mathscr{T}_n$ are defined on the circle $\mathbb{T}$, and the spaces $\mathscr{B}_\sigma(\mathbb{R})$ on the line $\mathbb{R}$. Both of these manifolds are Abelian groups (compact and locally compact respectively). The means of approximation similar to $\mathscr{T}_n$ and $\mathscr{B}_\sigma(\mathbb{R})$ can be defined on any locally compact Abelian group G as $\operatorname{span}\{\operatorname{ch}(\cdot,g^*),\, g^* \in K\}$, where $\operatorname{ch}\colon G \times G^* \to \mathbb{C}$ is a character of the groups G and G^* and K is a certain compact set contained in the dual group G^*. In the case $G = \mathbb{T}$, the compact set K consists of integers $\{k \in \mathbb{Z} : -n \leqslant k \leqslant n\}$; for $G = \mathbb{R}$, we have $K = [-\sigma,\sigma]$.

The algebraic polynomials and the methods of harmonic analysis (especially trigonometric polynomials and entire functions of finite degree) should be classified among the classical means of approximation. In comparatively recent years they have been supplemented by two new means of approximation — splines and wavelets. Splines are piecewise-polynomial figures; they can be introduced on a rather wide class of manifolds. Wavelets are special orthogonal and/or spline systems. Up to now, they have been defined on a rather narrow class of manifolds: again the most advanced theory of wavelets exists for $\mathbb{T}^n$ and $\mathbb{R}^n$.

The overwhelming majority of results on the approximation of periodic functions by trigonometric polynomials and functions on the line by entire functions of finite degree can be arranged in parallel rows. One reason why it is possible to extract from theorems on the line a parallel result on the circle is the possibility of *periodization*, under which the line is, so to speak, wound onto a cylinder and a function becomes periodic in the process. The opposite transition from the circle to the line is often based on "deperiodization," when the closed interval with identified endpoints expands to fill the entire line. We shall have occasion to demonstrate how this is done in practice.

Direct and inverse approximation theorems. It turned out to be possible to characterize the Hölder–Nikol'skii spaces by the rate of approximation of functions in these spaces by trigonometric polynomials and entire functions. To be specific, the following result holds:

A function $x(\cdot)$ belongs to $\mathscr{H}_p^\alpha(\mathbb{T})$ (resp. $\mathscr{H}_p^\alpha(\mathbb{R})$) *if and only if*

$$d\big(x(\cdot),\mathscr{T}_n,L_p(\mathbb{T})\big) \ll n^{-\alpha} \quad (\text{resp. } d\big(x(\cdot),\mathscr{B}_\sigma(\mathbb{R}),L_p(\mathbb{R})\big) \ll \sigma^{-\alpha})$$

(the Jackson – Bernshtein – Vallée Poussin – Zygmund – Stechkin theorem). Then, if (X,d) is a metric space and A a subset of it, $d(x,A,X)$ denotes the distance from x to A, that is, $\inf\{d(x,\xi) \mid \xi \in A\}$.

COMMENT. For a long time the smoothness of a function (of one variable) was characterized by a single integer $n \in \mathbb{N}$, namely the number of derivatives it possessed. Jackson, Bernshtein, and Vallée Poussin (1911–1919) (see [31]) defined a continuous scale of smoothness in C-spaces. (By their definition a function $x(\cdot)$ has smoothness $\alpha > 0$ if $\alpha = n + \beta$, $n \in \mathbb{Z}_+$, $0 < \beta \leqslant 1$, $x(\cdot) \in C^n(\mathbb{T})$, and $x^{(n)}(\cdot)$ satisfies a Hölder condition of order β.) For noninteger α they succeeded in proving the direct and inverse theorems. After F. Riesz (1909) defined the spaces L_p, it became possible to characterize the spaces by a pair of numbers $(1/p, \alpha)$ (and represent them by a point in a plane). The spaces $\mathscr{H}_p^\alpha$ were actually defined by Zygmund (1945), but their final definition seems to have been given first by Stechkin (see [26], pp. 18–39). As a result, the direct theorems could be inverted for all α.

The methods of proving a direct theorem are also based on smoothing. Here it is even a little easier to prove a direct theorem on $\mathbb{R}$. It suffices to convolute the function $x(\cdot)$ with an approximate identity of Jackson type: $J_{\sigma r}(t) = c_{r\sigma}(\sin \sigma t/t)^r$, for sufficiently large r. A simple computation proves the direct theorem. In the periodic case the goal is reached by convoluting with the kernel $\tilde{J}_{\sigma r}(\cdot)$, which is the periodization of the kernel $J_{\sigma r}(\cdot)$. ($\tilde{J}_{\sigma r}(t) = \sum_{k \in \mathbb{Z}} J_{\sigma r}(t+k)$; this function is a trigonometric polynomial of degree $[\sigma]$.) Thus the parallelism of $\mathbb{T}$ and $\mathbb{R}$ is realized in this case.

Inverse theorems can be proved by decomposing the function into "blocks of harmonics." Bernshtein proceeded as follows (we consider the periodic case below; the case of the line is similar). For each $k \in \mathbb{N}$ there exists, by the hypothesis of the theorem, a trigonometric polynomial $y_k(\cdot)$ that approximates $x(\cdot)$ up to $\ll 2^{-k\alpha}$. Then

$$x(\cdot) = \sum_{k \in \mathbb{N}} x_k(\cdot), \quad x_1(\cdot) = y_1(\cdot), \quad x_k(\cdot) = y_k(\cdot) - y_{k-1}(\cdot),$$
$$x_k(\cdot) \in \mathscr{T}_{2^k}, \quad \|x_k(\cdot)\|_{L_p(\mathbb{T})} \ll 2^{-k\alpha}.$$

At this point one must use the Bernshtein–Stechkin inequality (the analog of the Bernshtein–Zygmund inequality for difference operators), and we then arrive at the proof of the inverse theorem:

$$\|\Delta_\tau^r x(\cdot)\|_{L_p(\mathbb{T})} \ll \sum_{k=1}^{[\log 1/\tau]} \|\Delta_\tau^r x_k(\cdot)\|_{L_p(\mathbb{T})} + \sum_{k > [\log 1/\tau]} \|x_k(\cdot)\|_{L_p(\mathbb{T})}$$
$$\ll \tau^r \left(\sum_{k=1}^{[\log 1/\tau]} 2^{-k\alpha + kr} \right) + \tau^{-\alpha} \ll \tau^{-\alpha}.$$

We note that the theorem just proved has been extended to approximations by algebraic polynomials on a closed interval. (Here one must name S. M. Nikol'skii, V. K. Dzyadyk, A. F. Timan, P. M. Tamrazov, and others.) In this case also it makes sense to recall Hadamard's words about real truths, which always pass through the complex plane. The form of direct and inverse theorems for approximations by algebraic polynomials on a closed interval is slightly different — there the quality of the approximation depends on the position of the point in the interval. This fact has a complex explanation similar to the interpretation of V. Markov's inequality by Szegő.

Embeddings of functional spaces. In the 1950s Nikol'skii applied the methods of constructive function theory to the *theory of embeddings of functional spaces*.

S. M. Nikol'skii
(born 1905)

One of the topics studied in embedding theory is the following. Suppose a function $x(\cdot)$ is known to have smoothness $(1/p, \alpha)$ in the sense of Sobolev or Hölder–Nikol'skii. It is required to describe *all* the smoothnesses $(1/q, \beta)$ that it possesses. Let us give precise definitions.

Let X and Y be normed spaces (or semi-normed spaces; that is, spaces in which the norm of a nonzero element may be zero). We say that X is *continuously embedded in* Y if X (as a set) is a subset of Y and the embedding operator is continuous.[1] The following result holds: *A necessary and sufficient condition for the space* $\mathscr{W}_p^\alpha(\mathbb{T})$ (resp. $\mathscr{H}_p^\alpha(\mathbb{T})$) *to be embedded in* $\mathscr{W}_q^\beta(\mathbb{T})$ (resp. $\mathscr{H}_p^\beta(\mathbb{T})$) *is that* $\alpha - (1/p - 1/q)_+ \geqslant \beta$ (the embedding theorem for Hölder–Nikol'skii and Sobolev spaces).

Thus the set of smoothnesses generated by a point $(1/p, \alpha)$ consists of the points $(1/q, \beta)$ located below a horizontal line at height α if $1/q \geqslant 1/p$, and under a line inclined at 45° to the horizontal axis and passing through the point $(1/p, \alpha)$ if $0 \leqslant 1/q \leqslant 1/p$.

COMMENT. Two fundamental investigations generated embedding theory: the 1928 paper of Hardy and Littlewood, where the Sobolev and Hölder–Nikol'skii spaces were in essence introduced in the one-dimensional case and results close to the theorem just stated were proved; and the 1936 paper of Sobolev, which turned out to be a milestone in the history of mathematics, in

[1] That is, there exists a constant $C > 0$ such that $\|x\|_Y \leqslant C\|x\|_X$.

which the foundations of the theory of generalized functions were laid and the need for embedding theory in existence problems for solutions of variational problems was revealed (see [7]).

The proof of the theorem for Sobolev spaces is based on the Hardy–Littlewood inequality for fractional-derivative operators as transformations from L_p into L_q (which is equivalent to embedding $\mathscr{W}_p^{\alpha}(\mathbb{T})$ into $\mathscr{W}_p^{\alpha-(1/p-1/q)}(\mathbb{T})$).

The proof of the theorem for Hölder–Nikol'skii spaces is based on ideas and methods of constructive function theory (this was the fundamental idea of Nikol'skii). Functions $x(\cdot)$ from the Hölder–Nikol'skii space are expanded in a sum of the form $x(\cdot)=\sum_{k\in\mathbb{N}} x_k(\cdot)$ (over harmonics, splines, or wavelets). Here $x_k(\cdot)$ belongs to a space L_{d_k} of dimension d_k: $x_k(\cdot)=\sum_{j=1}^{d_k} x_{jk}\xi_{jk}$. The functions $x_k(\cdot)$ have the following properties:

(a) $\|x_k(\cdot)\|_{L_p(\mathbb{T})} \ll 2^{-k\alpha}$,

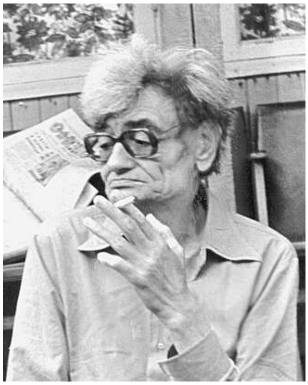
(b) $\|x_k^{(r)}(\cdot)\|_{L_p(\mathbb{T})} \ll 2^{-kr}\|x_k(\cdot)\|_{L_p(\mathbb{T})}$,

(c) $\|x_k(\cdot)\|_{L_p(\mathbb{T})} \asymp 2^{-1/p}\|x_k\|_{l_p^{d_k}}, x_k=(x_{1k},\dots,x_{d_k k})$.

The relations (a) are called the *Jackson inequalities*, (b) are called the *Bernshtein inequalities*, and (c) is called the *Marcinkiewicz–Zygmund property*. These relations lead not only to the proof of the embedding theorem, but also to a description (in the sense of weak asymptotics) of the optimal approximation methods for the Sobolev and Hölder–Nikol'skii classes. We shall discuss this in the next section.

Estimates for methods of approximation on classes of functions. In 1935 Kolmogorov published his first paper on approximation theory — [14, pp. 179–185]. In that paper he posed the problem of finding sharp estimates of the approximation methods *on classes of functions*. In the process, Kolmogorov computed the strong asymptotics of the precision of approximation by the Fourier method on the class $W_\infty^r(\mathbb{T})$. The following year, based on his 1936 paper, Favard (and after him, Akhiezer and Krein) computed the *deviation* of this Sobolev class from the space $\mathscr{T}_n$ of trigonometric polynomials. After that time "approximation on classes" occupied the main place in approximation theory. The first papers of Nikol'skii on approximation theory were devoted to this topic, and it exerted a strong influence on the formation of the schools of approximation theory in Moscow and Dnepropetrovsk. Bernshtein was at first indifferent to this area, expressing the view that it did not form a part of approximation theory. But later he participated in the development of the topic and obtained one of fundamental results.

Classes of analytic functions were later adjoined to the Sobolev classes of smooth functions. Wishing to emphasize the unity of approximation theory for smooth and analytic functions, from now on we shall consider together the Sobolev class $W^r_\infty(\mathbb{T})$ and the Hardy–Sobolev class $W^rH_\infty(D)$, which consists of functions $f(\cdot)$ that are analytic in the unit disk $\{z \in \mathbb{C} : |z| < 1\}$ for which $|f^{(r)}(z)| \leqslant 1$ for all $z \in D$. The analog of Kolmogorov's theorem for the Hardy–Sobolev class was obtained by Stechkin (1953).



S. B. Stechkin (1920–1995)

We now present the result of Kolmogorov and Stechkin. *The following formulas hold*:

$$\text{(A)} \quad \sup_{x(\cdot)\in W^r_\infty(\mathbb{T})} \|x(\cdot) - S_n x(\cdot)\|_{C(\mathbb{T})} = \frac{4}{\pi^2}\frac{\ln n}{n^r} + O(n^{-r}),$$

$$\text{(B)} \quad \sup_{f(\cdot)\in W^rH_\infty(\mathbb{T})} \|f(\cdot) - \mathrm{Tay}_n f(\cdot)\|_{C(\rho D)} = \frac{1}{\pi}\frac{\ln n}{n^r} + O(n^{-r}).$$

Here S_n denotes the Fourier operator that assigns to a function the n-th partial sum of its Fourier series, and Tay_n denotes the operator that assigns to a function the n-th partial sum of its Taylor series about 0. For $r = 0$ the result (A) is due to Lebesgue, and for $r > 1$ it is due to Kolmogorov (1935). For $r = 0$ the result (B) is due to Landau and for $r > 1$ to Stechkin (1953).

The proofs of (A) and (B) are similar. The differences between the functions and their Fourier and Taylor series are represented as integral operators on the r-th derivative in the smooth case, and on the boundary values of the r-th derivative in the analytic case. Then, by a double application of Abel's transformation, the principal term is determined (henceforth $D_n(\cdot)$ denotes the Dirichlet kernel):

$$\|y(\cdot) - S_n y(\cdot)\|_{C(\mathbb{T})} = \frac{1}{\pi(n+1)^r} \sup_{x(\cdot)\in W^r_\infty(\mathbb{T})} \left| \int_{\mathbb{T}} D_n(\tau) x^{(r)}(\tau)\,\mathrm{d}\tau \right| + O(n^{-r}) = \frac{1}{\pi(n+1)^r}\lambda_n + O(n^{-r}),$$

$$\|f(\cdot) - \mathrm{Tay}_n f(\cdot)\|_{C(\rho D)} = \frac{\rho^n}{\pi} \sup_{f(\cdot)\in W^rH_\infty(D)} \left| \int_{\mathbb{T}} D_n(\mathrm{e}^{\mathrm{i}\tau}) f^{(r)}(\mathrm{e}^{\mathrm{i}\tau})\,\mathrm{d}\tau \right| + O(n^{-r}) = \frac{\rho^n \alpha_{nr}}{\pi} l_n + O(n^{-r}),$$

and it remains only to invoke the results of Lebesgue ($\lambda_n/\pi = 4/\pi^2 \log n + O(1)$) and Landau ($l_n = \log n + O(1)$).

Deviations of the function classes from spaces of polynomials. Precise results on the deviations of the Sobolev and Hardy–Sobolev classes from the spaces of trigonometric and algebraic polynomials were obtained by Favard and Babenko, respectively. Here is a statement of their result:

Let $r \in \mathbb{N}$. Then the following inequalities hold ($D_\rho = \{z : |z| \leqslant \rho\}$):

(A) $d\big(W^r_\infty(\mathbb{T}), \mathscr{T}_{n-1}, C(\mathbb{T})\big) = \dfrac{K_r}{n^r}$,

(B) $d\big(W^r H_\infty(D), \mathscr{P}_{n-1}, C(D_\rho)\big) = \rho^n \alpha_{nr}$,

where

$$K_r = \frac{4}{\pi} \sum_{k \in \mathbb{N}} \frac{(-1)^{k(r+1)}}{(2k-1)^{r+1}}, \quad \alpha_{nr} = (n(n-1)\cdots(n-r+1))^{-1}.$$

The proof of this theorem and many others like it (a considerable portion of the monograph [1] is devoted to such results) is closely connected with the solutions of problems of type (1.4) and (1.4′), discussed in Section 1 above (see [22]).

Let us now pass to the discussion of optimal methods of approximation and recovery.

3. Optimal Methods of Approximation and Recovery of Functions (The Circle of Ideas of Kolmogorov)

As mentioned in the introduction to this article, one of the main purposes of approximation theory is “finitization” (transformation to the finite) of the infinite information contained in the concept of a function.

To get a clearer understanding of what has just been said, one can give an example inspired by the ideas of information theory. Imagine that we receive certain information (the graph of a function, a photograph, some drawing, or the like) and are required to pass this information on by telegraph in such a way that a specialist receiving the signal on a receiving device can recover the original information with the required precision. The purpose is to achieve all this in the most economic way possible. To give a precise meaning to what has just been said, we need to explain a bit and answer some questions.

How is finitization accomplished? Usually as follows. The function is first “encoded” using a finite number of elements or a finite set of numbers (say, computing n values of the function at different points or n Fourier or Taylor coefficients, or forming a table of 2^n spaces of elements, and the like). Next, some "decoding" operator is formed using this code, leading to the *recovery of the function with a certain precision*. This may be an interpolation formula (where interpolation is carried out using polynomials, splines, and the like), or it may be Fourier, Fejér, or Jackson sums, or a segment of the Taylor series, and many other things. Sometimes this finite information is already given to us, and then the desire arises to find the best-possible algorithm for decoding a function from its code; but usually we have a choice of both the code itself (from some family of codes) and a certain family of decodings of the given codes. And we are required to choose an optimal coding-decoding pair. Here the question arises: *How is this to be done*?

One approach is as follows. It is natural to assume that we have at our disposal certain "global (a priori) information" about the functions we may encounter. This information combines all the admissible functions into a certain functional class. But for the recovery we use certain "individual" information that yields a method of recovering an element with prescribed precision by decoding it.

It is possible to pose the problem of the *optimal approximation method* when we seek the best subspace among all subspaces of a given dimension as the means of approximation of the given class, or the problem of the *optimal linear operator* as the optimal method of approximating a given class.

In 1936 Kolmogorov published a paper [14, pp. 186–189] in approximation theory in which in essence he posed the problem of the *optimal method of approximation in a class of functions when the approximating machinery consists of all possible n-dimensional subspaces*. In the process, he introduced a quantity that came to be known as the *Kolmogorov n-width*.

A. N. Kolmogorov
(1903–1987)

In 1956, starting from the ideas of Shannon's information theory, Kolmogorov introduced the concept of the *ε-entropy of a class of elements*, posing the problem of *optimal coding* of functions of the class. The concept of ε-entropy turned out to be closely connected with the concept of a width.

The *Kolmogorov width of a class C* contained in a normed space X is

$$d_n(C,X) = \inf_{L_n} d(C,L_n,X) = \inf_{L_n} \sup_{x\in C} \inf_{\xi\in L_n} \|x-\xi\|,$$

where the infimum extends over all n-dimensional subspaces L_n. The quantity inverse to the ε-entropy of a class C (called the *entropic width*) is the number

$$e_n(C,X) = \inf_{M_n} d(C,M_n,X) = \inf_{M_n} \sup_{x\in C} \inf_{\xi\in M_n} \|x-\xi\|,$$

where the infimum extends over all 2^n-point subsets of X.

The first concept of a width was introduced by Uryson (1922) for the needs of dimension theory and then modified by Aleksandrov (1933). Kolmogorov (1936) gave the definition of width as a geometric characteristic of a set, but applied it in approximation theory. Nowadays several other asymptotic characteristics of pairs (C,X), called widths, are considered.

Let X be a normed space, let $C \subset X$ be a subspace (usually a class of functions), and let $\mathscr{F} = \{f\colon C \to X\}$ be a set of "approximation methods," that is, mappings of C into X. We introduce the following quantities

$$p_{\mathscr{F}}(C,X) := \inf_{f\in\mathscr{F}} \sup_{x\in C} \|x - f(x)\|.$$

The quantities $p_{\mathscr{F}}$ characterize the possibilities of these approximation methods. If X is a normed space and the set $\mathscr{F} = \mathscr{F}_n$ consists of linear n-dimensional operators, the corresponding width is called *linear* and denoted $\lambda_n(A,X)$. If X is a normed space and there exists a Hilbert space H in which C is embedded, and if $\mathscr{F}_n$ consists of orthogonal projections $x \mapsto \big(\langle x,e_1\rangle e_1,\dots,\langle x,e_n\rangle e_n\big)$, where $\langle\cdot,\cdot\rangle$ is the inner product in H, and if in addition $\mathscr{F}_n$ consists of compositions of this orthogonal projection and mappings $(z_1,\dots,z_n) \mapsto \sum_{j=1}^n z_j e_j$, then the corresponding width is called the *Fourier* (or *orthogonal-projection*) width of C and denoted $\varphi_n(C,X)$. The Kolmogorov width and the entropic width can also be easily inserted into this scheme: one has only to consider the set of all mappings from C into n-dimensional subspaces or 2^n-point sets. Finally, let $\mathscr{K}^n$ be a universal n-dimensional compact space. Then if $\mathscr{F}_n$ consists of compositions of continuous mappings from C into $\mathscr{K}^n$ and from $\mathscr{K}^n$ into X, the corresponding width is called the *Aleksandrov width* and denoted $a_n(A,X)$.

Quantities like widths make it possible to pose and solve questions of the following type: *Which Fourier method is optimal as a mechanism for approximating a given class of functions*? *Which linear n-dimensional operator has the same optimal property*? *Which n-dimensional subspace approximates a given class best*? *Which continuous operator with n-dimensional image has the best approximating characteristics*? The answers to these questions on the asymptotic level for the Hölder–Nikol'skii and Sobolev classes are given in the next section. (The monographs [27] and [25] are devoted to widths.)

Asymptotics of widths of classes of smooth functions. To state our result we must partition the square I^2 into regions where the asymptotics differ from one another. The square $(1/p, 1/q) \in I^2$, $I = [0,1]$, can be partitioned into five regions. I — the "upper triangle" formed by the points $1/q \geqslant 1/p$; II — the "Ismagilov triangle" (so-called because Ismagilov (1968) computed the asymptotics of the Kolmogorov width in this triangle) formed by the points where $1/p \geqslant 1/q \geqslant 1/2$. The remaining portion of the square is called the "Kashin trapezoid." Kashin (1975) computed the asymptotics of the Kolmogorov width in this trapezoid. The Kashin trapezoid is subdivided into three triangles: III — $1/q \leqslant \min(1/p, 1/2), 1/p + 1/q \geqslant 1$; IV — $1/q \leqslant \min(1/p, 1/2), 1/p + 1/q \leqslant 1$; V — $1/q \leqslant 1/p \leqslant 1/2$.

Let p_n be one of the widths φ_n, λ_n, d_n, or a_n, and let $p = p(x,y) : I^2 \to \mathbb{R}$ be one of the quantities φ, λ, d, $a : I^2 \to \mathbb{R}$.

$$\varphi(x,y) = (x-y)_+ \ \text{ in I–V}, \qquad a(x,y) = 0 \ \text{ in I–V},$$
$$d(x,y) = (x-y)_+ \ \text{ in } \mathrm{I} \cup \mathrm{II}, \quad (1/p - 1/2) \ \text{ in } \mathrm{III} \cup \mathrm{IV}, \quad (x-y) \ \text{ in V},$$
$$\lambda(x,y) = (x-y)_+ \ \text{ in } \mathrm{I} \cup \mathrm{II}, \quad (x - 1/2) \ \text{ in III}, \quad (1/q - 1/2) \ \text{ in IV}, \quad 0 \ \text{ in V}.$$

The following theorem on the asymptotics of widths of one-dimensional Sobolev and Hölder–Nikol'skii classes holds:

Let $1 \leqslant p, q \leqslant \infty$. Then for $\alpha \geqslant \alpha_0$ the following formulas hold:

$$p_n\big(W_p^\alpha(\mathbb{T}), L_q(\mathbb{T})\big) \asymp p_n\big(H_p^\alpha(\mathbb{T}), L_q(\mathbb{T})\big) \asymp n^{-\alpha + p(1/p, 1/q)}, \tag{3.1}$$

where the numbers p_n are connected with $p(1/p, 1/q)$, as explained above.

The proof of this theorem is based on the expansions in harmonics, splines, or wavelets described in the preceding section. The optimal methods (from the point of view of the weak asymptotics of the widths) of approximating the classes $H_p^\alpha(\mathbb{T})$ or $W_p^\alpha(\mathbb{T})$ in $L_q(\mathbb{T})$ differ depending on the location of the point $(1/p, 1/q)$ in the unit square. In the union of the "upper triangle" (when $1/q \geqslant 1/p$) and the "Ismagilov triangle" (when $1/p \geqslant 1/q \geqslant 1/2$) the optimal method is linear and completely classical: the function must be expanded in a series and approximated by the leading terms of the series. In the rest of the square the optimal methods are nonlinear. For the Aleksandrov width we apply a device that we call the standard nonlinear method, in which the most significant coefficients are selected and they are the ones approximated (of course, to assure continuity in this process it is necessary to carry out some smoothing). The optimal approximation methods for the linear and Kolmogorov width are based on subtle geometric estimates of the widths of finite-dimensional sets.

Many mathematicians took part in the proof of the theorem just stated and its multi-dimensional generalizations: Kolmogorov, Rudin, Stechkin, Tikhomirov, Babenko, Mityagin, Makovoz, Solomyak, Ismagilov, Kashin, Gluskin, Dinh Đung, Maiorov, Höllig, Stesin, Galeev, Temlyakov, and others.

Exact values of the widths. The first exact values of widths were obtained by Kolmogorov in 1936, in his first paper on widths. He proved the following equalities:

$$d_{2n-1}\big(W_2^r(\mathbb{T}),L_2(\mathbb{T})\big)=d_{2n}=\frac{1}{n^r},\quad n\geqslant 1.$$

The class $W_2^r(\mathbb{T})$ in $L_2(\mathbb{T})$ is an elliptic cylinder, and its widths are the axes of this elliptic cylinder. Hence the optimal subspaces are the spaces $\mathscr{T}_n$.

In a 1960 paper, I proved the equalities $d_{2n-1}\big(W_\infty^r(\mathbb{T}),C(\mathbb{T})\big)=K_r/n^r$ and $d_n\big(W^rH_\infty(D),C(D_\rho)\big)=\alpha_{nr}\rho^n$. Getting the lower bound involved applying topological considerations (the Borsuk antipodal theorem, which was later applied countless times in similar situations) for the first time in approximation theory. Comparison with the theorem on deviation of the Sobolev class from the space of trigonometric polynomials (see the preceding section) shows that the subspaces $\mathscr{T}_{n-1}$ and $\mathscr{P}_n$ (respectively) yield the best approximation of the classes $W_\infty^r(\mathbb{T})$ and $W^rH_\infty(D)$. But in the smooth case, in addition to the space of trigonometric polynomials, the space of splines of order $r-1$ with $2n$ uniformly distributed nodes is also an optimal space. (I pointed out this fact in 1969.) Subsequently, A. A. Ligun showed that the extremal spaces for the class $W_\infty^r(\mathbb{T})$ are the spaces of splines of every order $m\geqslant r-1$.

No fundamentally new optimal methods of approximation have so far been discovered. The optimal methods are either the Fourier method (as in the case of elliptic cylinders and generalized octahedra in Hilbert space) or certain interpolation methods connected with special splines. Such are the extremal subspaces for approximating the Sobolev classes $W_\infty^r([-1,1])$ in the spaces $L_q([-1,1])$ for $q\geqslant p$. Here special functions $x_{nrpq}(\cdot)$ arise, which are the eigenfunctions of a nonlinear differential equation of Sturm–Liouville type. (In, say, the case $r=1$, $p=2$, $q=4$, these equations have the form $\ddot{x}(\cdot)+\lambda x^3=0$.) The family $x_{nrpq}(\cdot)$ contains a large number of known special functions (the Legendre polynomials, the Chebyshev polynomials, and the like). For this, see the monograph [20].

Methods of recovery. In conclusion we shall discuss optimal methods of recovery.

The general statement of the problem is as follows. Suppose a class C and a mapping f from C into a metric space (Z,d) are given. It is assumed that for each element of C we have at our disposal certain information.

Analytically, this condition is expressed by saying that a certain mapping F from C into a certain set Y is given. We also consider the case when the information is given imprecisely: in this case F is a multi-valued mapping. The problem is to recover the function f in the class C from the information F. This has the following meaning. Every function $\varphi\colon Y \to Z$ is called a recovery method, and the error in such a method is estimated by the quantity $e(f,C,F,Z,\varphi) := \sup_{x\in C,\, y\in F(x)} d\big(f(x),\varphi(y)\big)$. We are interested in optimal recovery error, that is, the quantity $E(f,C,F,Z) = \inf_\varphi e(f,C,F,Z,\varphi)$, where the infimum extends over all functions (methods) $\varphi\colon Y \to Z$, and also in the optimal recovery method, that is, the method at which this lower bound is attained. This approach is quite broad and theoretically encompasses other methods of approximation theory that we considered above.

Problems of this type were considered starting in the 1940s. The case when C is a convex, centrally-symmetric subset of a real vector space X, Y is a finite-dimensional subspace, $f(x) = \langle x', x\rangle$ is a linear functional, and $F\colon X \to Y$ is a linear operator was first considered by S. A. Smolyak. One can exhibit a unified approach to the solution of problems of this type. The extremal problem

$$\langle x', x\rangle \to \max, \qquad x \in F^{-1}(0), \qquad x \in C, \tag{i}$$

is connected with the problem of recovering x' on a centrally-symmetric subset C of the vector space X using the operator $F\colon X \to Y$. This is a convex problem, to which Lagrange's principle applies, according to which the minimal principle holds:

$$\min_{x\in C,\, y\in F(x)} \mathscr{L}\big((x,y)\widehat{\lambda}, -1\big) = \mathscr{L}\big((\widehat{x},0)\widehat{\lambda}, -1\big),$$

where

$$\mathscr{L}\big((x,y)\widehat{\lambda}, -1\big) = -\langle x', x\rangle + \langle \lambda, y\rangle.$$

Here $\widehat{\lambda}$ is the optimal recovery method.

Suppose we have the graph of a function $y(\cdot)$ on the interval $[-1,1]$, about which it is known that the norm in $C([-1,1])$ between the graphs of a certain polynomial and the given function is δ. And suppose our task is to recover optimally the value of the polynomial at a point $\tau > 1$. It is not difficult to see that the dual to this problem is the Chebyshev extrapolation problem, which we considered in Section 1 above. If we use the results obtained there, we obtain the following formula for optimal recovery:

$$x(\tau) \approx \sum_{j=0}^{n} x\big(\zeta_j(\tau)\big) y\left(\cos\frac{j\pi}{n}\right),$$

where $\zeta_j(\cdot)$ are polynomials in $\mathscr{P}_n$ such that $\zeta_j\left(\cos\frac{k\pi}{n}\right) = \delta_{jk}$. We see that for optimal recovery one must take the values of the function $y(\cdot)$ at certain points.

To return to the first sentence of this article, we can summarize what we have done as follows: The optimal methods of transforming information about a function consist, as a rule, of recovering the function from its values or from the coefficients of some series (naturally connected with the information that we know about the function).

Conclusion

Our purpose in this article has not been to give a survey of *papers* on approximation theory but to *describe the original statements of the problems* that the masters of our subject posed and to *note certain pivotal concepts, ideas, and methods of the theory*. There was no possibility of naming all the mathematicians whose works have a direct relation to the concepts, ideas, methods, and results discussed above. The names of over 150 mathematicians are implicitly involved in this article, and the list of papers would have had to consist of over 300 works. These lists provide the possibility of stating some conjectures as to the number of mathematicians who have written on approximation theory. There seem to be no fewer than 500 of them (it would not be a bad idea to compile an atlas of specialists in approximation theory, as has been done in topology).

Let us take a brief tour of the literature devoted to approximation theory. A fairly complete bibliography can be found in [28].

The majority of the Russian and Soviet masters in the area of approximation theory are represented by the collections [8], [6], [23], [14], [26].

A large number of monographs have been devoted to approximation theory. Among those in Russian we note [1], [5], [9], [16]–[19], [29], and [27]. Of the books not published in Russian we note the monographs [25] and [20].

It is natural to attempt to encompass everything that has been done in approximation theory from a unified point of view, and in order to see the extent to which the purposes ascribed to approximation theory have been realized. It is also natural to outline plans for the future.

Undoubtedly, approximation theory has brought a considerable amount of new, brilliant, and fundamental material into mathematical analysis. Analysis has been enriched by many special functions such as Chebyshev polynomials of first and second kind, Zolotarev polynomials, special orthogonal polynomials, and the like. New chapters in the theory of elliptic functions have been developed. New means of approximation have been discovered — entire functions of exponential type, splines, and wavelets. The theory of representation series for smooth and analytic functions has been greatly advanced. The solution of many extremal problems of approximation theory has furnished the groundwork for a general extremum theory. The general concept of optimization of methods of approximation and recovery has been developed. Much has been done in the area of nonlinear analysis; in particular, in the spectral theory of linear and nonlinear differential equations, and the like.

In the process, many of the original priorities have remained unchanged. The theory of mechanisms, which for Chebyshev provided the basis of approximation theory itself, has long been obsolete; no one now computes the values of special functions using the polynomial and rational-function approximations that Chebyshev and his cohorts obtained. But naturally new motives have arisen, which we shall now discuss.

Approximation theory, it seems, should have realized its aim more completely and become the theoretical base of numerical analysis. Moreover, it should do so at the new stage of development of computer technology. The concepts of optimality with respect to precision of approximation, which have been dominant in the theory up to the present, should combine with the concepts of complexity and greediness. It is necessary to create a general theory of greedy algorithms, which react adequately to the smoothness of the data of a problem (K. I. Babenko insisted repeatedly on this — see, for example, his book [3]). There is also no doubt that approximation theory must enlarge the class of manifolds on which the theory is constructed (these have mainly been $\mathbb{T}^n$ and $\mathbb{R}^n$) and pass to general homogeneous spaces and special functions on them.

In the theory of extremal problems (to which Section 1 above was devoted) the jump from one variable to several must be completed. It is necessary to know the forms that Lagrange's principle assumes for functions of several variables. For the long-term productive development of the theory the experience of solving specific problems of approximation theory (of Landau–Kolmogorov type for several variables) will be invaluable. The development of the Lagrange principle for problems on noncompact manifolds (say, the line and the half-line), and the creation of a general theory of extrema for solving problems with analytic functions, is also on the agenda.

In the topics touched on in Section 2 there remain many unfinished problems. The generalization of Mergelyan's theorem to the multi-dimensional case seems to be a serious problem. Several natural questions arise in connection with the Weierstrass–Bernshtein theorem.

The theory of embedding and widths for intersections of spaces is not yet perfected.

Of the problems treated in Section 3, the central ones are again multi-dimensional problems: in the one-dimensional case exact solutions were connected with nonoscillation, which theoretically do not exist in the multi-dimensional case (there are no T-spaces on the plane!). It is necessary to create a modified theory that will make it possible to create special functions and special methods of approximation for smooth and analytic functions of several variables.

The creation of synthetic theories, that combine approximation of smooth and analytic functions, as well as approximations on $\mathbb{T}^n$ and $\mathbb{R}^n$, also seems inevitable.

Bibliography

[1] N. I. Akhiezer. *Lectures on Approximation Theory*. Moscow: Nauka, 1965 (Russian).

[2] V. V. Arestov. Approximation of unbounded operators by bounded operators and related extremal problems. *Russ. Math. Surveys*, 1996, **51**(6), 1093–1126.

[3] K. I. Babenko. *Foundations of Numerical Analysis*. Moscow: Nauka, 1986 (Russian).

[4] S. K. Bagdasarov. *Chebyshev Splines and Kolmogorov Inequalities*. Berlin: Birkhäuser, 1998.

[5] S. N. Bernshtein. *Extremal Properties of Polynomials*. Moscow–Leningrad: ONTI (United Science and Technology Publishers), 1937.

[6] S. N. Bernshtein. *Constructive Function Theory*. *Collected Works*, Vols. I, II. Moscow: USSR Academy of Sciences, 1952 (Russian).

[7] O. V. Besov, V. P. Il'in, S. M. Nikol'skii. *Integral Representations of Functions and Imbedding Theorems*, Vols. I, II. Washington: Winston & Sons; New York: Halsted Press, 1978–1979.

[8] P. L. Chebyshev. *Complete Works*, Vol. 2. Moscow–Leningrad: USSR Academy of Sciences, 1948 (Russian).

[9] V. K. Dzyadyk. *Introduction to the Theory of Uniform Approximation of Functions by Polynomials*. Moscow: Nauka, 1977 (Russian).

[10] A. L. Garkavi. Theory of best approximation in normed vector spaces. In: *Itogi Nauki i Tekhniki. Matem. Analiz, 1967*. Moscow: VINITI, 1969, 75–132 (Russian).

[11] V. L. Goncharov. *Theory of Best Approximation of Functions*. Moscow: GITTL (State Publishing House for Technical and Theoretical Literature), 1945 (Russian).

[12] D. Jackson. Über die Genauigkeit der Annäherung stetiger Funktionen durch ganze rationale Funktionen gegebenen Grades und trigonometrische Summen gegebener Ordnung. Dissertation. Göttingen, 1911.

[13] B. S. Kashin. Diameters of some finite-dimensional sets and classes of smooth functions. *Math. USSR, Izv.*, 1977, **11**, 317–333.

[14] A. N. Kolmogorov. *Selected Works. Mathematics and Mechanics*. Moscow: Nauka, 1985 (Russian).

[15] A. N. Kolmogorov, V. M. Tikhomirov. ε-Entropy and ε-capacity of sets in functional spaces. *Uspekhi Mat. Nauk*, 1959, **14**(2), 3–86 (Russian); *AMS Transl.* (2), 1961, **17**, 227–364.

[16] N. P. Korneichuk. *Extremal Problems of Approximation Theory*. Moscow: Nauka, 1976 (Russian).

[17] N. P. Korneichuk. *Splines in Approximation Theory*. Moscow: Nauka, 1984 (Russian).

[18] N. P. Korneichuk, A. A. Ligun, V. G. Doronin. *Approximation with Constraints*. Kiev: Naukova Dumka, 1976 (Russian).

[19] P. P. Korovkin. *Linear Operators and Approximation Theory*. Moscow: Nauka, 1959 (Russian).

[20] G. G. Lorentz, M. Golitschek, Yu. Makovoz. *Constructive Approximation. Advanced Problems*. Berlin: Springer, 1996 (Grundlehren Math. Wiss., **304**).

[21] G. G. Magaril-Il'yaev, V. M. Tikhomirov. On inequalities of Kolmogorov type for derivatives. *Sb. Math.*, 1997, **188**(12), 1799–1832.

[22] G. G. Magaril-Il'yaev, V. M. Tikhomirov. *Convex Analysis and its Applications*. Moscow: Editorial URSS, 2000 (Russian).

[23] A. A. Markov. *Selected Works*. Moscow–Leningrad, 1948 (Russian).

[24] V. A. Markov. *On Functions of Least Deviation from Zero in a Given Interval*. St. Petersburg: Russian Academy of Sciences, 1892 (Russian).

[25] A. Pinkus. *n-Width in Approximation Theory*. Berlin: Springer, 1985.

[26] S. B. Stechkin. *Selected Works. Mathematics*. Moscow: Nauka (Fizmatlit), 1998 (Russian).

[27] V. M. Tikhomirov. *Some Questions of Approximation Theory*. Moscow University Press, 1976 (Russian).

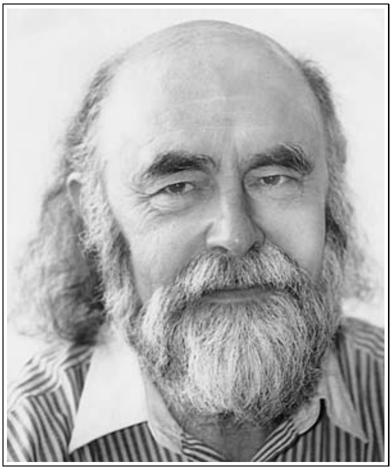
[28] V. M. Tikhomirov. Approximation theory. In: *Analysis*. II. *Convex Analysis and Approximation Theory*. Berlin: Springer, 1990, 93–243 (Encyclopædia Math. Sci., **14**).

[29] A. F. Timan. *Theory of Approximation of Functions of a Real Variable*. Moscow: Fizmatgiz, 1960 (Russian).

[30] C. J. Vallée Poussin. Note sur l'approximation par un polynôme d'une fonction dont la dérivée est à variation bornée. *Bull. Cl. Sci. Acad. Roy. Belg.*, 1908, **4**, 403–410.

[31] C. J. Vallée Poussin. *Leçons sur l'approximation des fonctions d'une variable réelle*. Paris: Gauthier-Villars, 1919.

[32] K. Weierstrass. Über die analytische Darstellung sogenannter willkürlicher Funktionen einer reelen Veränderlichen. *Sitz. Akad. Berlin*, 1885, 633–639, 789–805.



A. M. Vershik

The Life and Fate of Functional Analysis in the Twentieth Century

Translated by R. Cooke

Two tendencies in mathematics are in constant competition with each other, and from time to time one of them displaces the other: the first is the striving to construct general theories and concepts, a new world view and a new vocabulary; the second is to study particular basic examples.

It is curious that in the history of mathematics over the last three centuries the first tendency has predominated, more or less, during the first half of each century, while in the second half its momentum slackens and dies down, and the second tendency comes to the fore. A surge of interest arises in the solution of classical problems on the basis of the concepts developed during the first half of the century, and mathematics returns in a certain sense to older topics. This periodicity was not strongly expressed in the eighteenth century, since the giants who founded analysis were universal men; but it is more explicit in the nineteenth and twentieth centuries. (Of course, the time frames do not completely coincide with the beginning and end of centuries.) Such a conclusion is easy to confirm by examples of the mathematical biographies of the nineteenth century — it suffices to consider the well-known book of Felix Klein from this point of view. As for the twentieth-century mathematics, the change of fashion — more precisely, the change in interest from general and conceptual problems to more particular ones — occurred before our very eyes.

Of course, this tendency was promoted by perfectly definite mathematical discoveries connecting the classical examples with modern concepts; but undoubtedly, these discoveries themselves should properly be perceived as a manifestation of this regularity. An example is the discovery of new mechanisms of integrability during the 1960s and the consequent revolution, which led in particular to a new way of looking at classical integrable systems, connections with algebraic geometry, the inversion problem, and many other questions. Another example comes from the theory of dynamical systems: after the construction of a general theory of dynamical systems (topological, metric, symbolic, and others) in the first half of the twentieth century, interest shifted toward the study of more specific problems – KAM theory, standard mappings, arithmetical systems, and so on. However, the following discussion is not simply about a change in emphasis and interest within a given area, as in these examples, but about a crucial change in the relationship to the subject.

The history of functional analysis (its birth, flourishing and present fate), its role and the evolution of the relationship to it, is perhaps the clearest and best articulated example of this kind, and this note is about that subject. Whether the cause of all this is the aforementioned hundred-year regularity of mathematical fashion or whether some deeper cause exists, is not of great importance.

Functional analysis arose in the early twentieth century and gradually, conquering one stronghold after another, became a nearly universal mathematical doctrine, not merely a new area of mathematics, but a new mathematical world view. Its appearance was the inevitable consequence of the evolution of all of nineteenth-century mathematics, in particular classical analysis and mathematical physics. Its original basis was formed by Cantor's theory of sets and linear algebra. Its existence answered the question of how to state general principles of a broadly interpreted analysis in a way suitable for the most diverse situations. We shall speak of this in somewhat more detail below.

By mid-century, functional analysis had become almost the strongest center of attraction of general interest. The general spectral theory of operators was perfected and applied to the theory of differential equations. The theory of representations arose and developed significantly. The theory of C^*-algebras and W^*-algebras was created. The theory of distributions arose and embedding theorems were formulated. The basic theory of infinite-dimensional integration was developed, as was Banach geometry. Finally, a little later, index theory was discovered, with its fundamental connections to topology, K-theory and mathematical physics, and other areas.

But, as early as the 1950s and 1960s, there arose a sense that functional analysis had ceased to be the general platform for those areas that owed their origin to it, that it united them only in an artificial way. By now each of them was an autonomous area based on its own theories, sometimes more traditional, and sometimes new, but usually special and more profound. A new era was coming, and in accordance with the tradition of the second halves of centuries mentioned above, interest was shifting from general concepts to classical facts. Functional analysis was not only ceasing to be the center of attention and yielding its place to others (as sometimes happens with a transient fashion), but was simply dissolving and disappearing as an integral area. It was remaining as a language, as vocabulary, but losing its own subject matter. Many subjects may flower and then decline. For example, topology, after undergoing the loftiest flight in the 1950s and 1960s, quieted down for a while, but has recently been heard from again through its connections with quantum field theory; classical complex analysis of one variable was in the main perfected long ago, but later transformed itself into the theory of complex manifolds, and so on. But the present case is different: *By the end of the twentieth century, functional analysis was losing its subject matter, giving up everything it had created, most of all its language, to neighboring and daughter subjects.*

Skeptics, who have been around almost since the beginning of functional analysis — and they were many — will say, "There is nothing left," and will add, "This was plain from the outset." And, of course, they will be wrong. Other, more cautious critics will express a more carefully weighed judgment: "Functional analysis has played its role and has dispersed into various theories where its concepts, notation, and theorems have been adapted and are being actively used." Finally, its enthusiasts, who still exist, will enumerate its indisputable achievements and on that basis, argue that it continues a full-blooded existence. This last claim is particularly characteristic of those who study the most traditional topics of functional analysis and therefore live a rather cloistered life. Since I do not share either of the extreme opinions on this subject, I believe that it is useful to analyze objectively the dramatic changes that have occurred in this branch of mathematics — a phenomenon that has been encountered previously in the history of our subject, but not on such a scale.

The present article makes no claim to even relative completeness in its historical references or any complete listing of important theories and results, names, and ideas. Its only purpose is to fix an impression (perhaps quite superficial) that has formed after reflecting on the fate of one of the most popular mathematical concepts of the early and mid-twentieth century — functional analysis. The history of twentieth-century mathematics has not yet begun to be created (judging from the topics known to me). Actually, only Felix Klein knew how to package all that was essential of what was created during the nineteenth century in a single book. It will be much more difficult for a new Klein; such a feat is hardly even possible in the twentieth century.

Strictly speaking, the history of functional analysis should be counted from 1696 — the time when the calculus of variations appeared in the form of Johann Bernoulli's brachistochrone problem. It is in the calculus of variations that natural infinite-dimensional spaces, manifolds, variational derivatives, and other concepts arise of necessity. It remains a mystery why it was necessary to wait 200 years for the definitions and concepts of differentials (of Fréchet and others) of functions of an infinite number of variables or functions of curves, when essentially all of this was implicitly contained in the work of Euler and his successors. Moreover, because of this delay functional analysis remained largely a linear theory for too long, and the so-called global analysis (the need for which was felt long before its appearance) arose only much later, mainly in connection with infinite-dimensional topological problems and calculus of variations in the large. Symplecticity, embedded in variational problems, also manifested itself only much later.

The point of departure for functional analysis is, of course, the idea of replacing a mathematical object by a suitable space of functions on it; it is this, not just the transition to infinite dimensions and function spaces, that forms the essence of the subject and made it into a new philosophy. This idea now seems so natural and even banal (why functions and not sections of a fiber bundle?), that we nowadays find it difficult to appreciate its novelty and the enthusiasm it evoked. The rise of linear functional analysis at precisely the start of the twentieth century was, of course, not a random event, and the explanation lies on the surface — the theories of integral equations of Fredholm and Hilbert, motivated by mathematical physics, had shown that many facts of *linear algebra* can be carried over with suitable caution from ordinary matrices to infinite matrices, that is, to operators. Fredholm determinants and the spectral theorem for bounded self-adjoint operators were the first facts in the new theory. Undoubtedly, this made a strong impression on contemporaries: infinite dimensionality had become geometrically palpable and, most importantly, was in demand.

To be sure, Hilbert (according to an apocryphal story) was not particularly interested in this aspect of the matter. ("What are these Hilbert spaces you keep talking about?" he is said to have asked one of his students, according to Constance Reid.)

The rise of functional analysis came at the same time as another fundamental event in science (and not only in science) — the discovery of quantum mechanics. To this day, some historians of science (see, for example, Bourbaki's "Historical Essay") regard this coincidence as the proof of an almost mystical interrelationship between mathematics and the natural sciences. Indeed, the theory of operators on a Hilbert space and the geometric theory of these spaces arose, as noted above, as a logical consequence of the development of mathematics itself. But on the other hand, as von Neumann and others showed, the Schrödinger equation (whose formulation owes something to Hermann Weyl) and all quantum-mechanical formalism, including the Fock second quantization, can be precisely described only in terms of Hilbert spaces, operators on them, algebras of operators, and the like. One might even conjecture that if the functional analysis of Hilbert spaces had not yet existed at the time when quantum mechanics arose, it would have been created out of necessity. For that reason, it is no exaggeration to say that the extremely close connection between the latest physics of the first half of the twentieth century and functional analysis gave the latter even greater authority.

Linear algebra and the geometry of vector spaces became the basis of functional analysis for a long time. Point-set topology, the related analysis of spaces of continuous functions on topological spaces, and the theory of the Lebesgue integral and the related study of various spaces of measurable functions and operators on them, gave the next impetus in the development of linear functional analysis. They led to the appearance of Banach spaces in the early 1920s, which were called Wiener–Banach spaces for a while, since Wiener had also introduced complete normed spaces, independently of Banach. Later, in his autobiography, Wiener noted acerbically how, shortly afterwards, he realized that this area would be the source of endless dissertations and decided as a consequence to get out of it.

By the early 1930s the young functional analysis consisted mainly of the spectral theory of operators in Hilbert spaces and the theory of particular Banach spaces. This covered the spectral theory of linear differential equations, a large part of the theory of functions of a real variable and approximation theory (constructive function theory), elementary linear integral equations, parts of harmonic analysis on locally compact groups, and the theory of functions of complex variables, the latter only to a very modest extent (up to the 1950s). The focus on linearity that we have emphasized concealed a great danger for the future: the new algebraic theories that developed during the 1930s and 1940s — homological algebra and combinatorial topology — had no overlap at all with functional analysis.

However, the 1930s saw a rapid development of this subject. By that time three main schools had formed in the USSR — in Moscow, Leningrad, and Ukraine. The Moscow school of the early years consisted of Kolmogorov, Lyusternik, Gel'fand and their students, as well as Plessner, who (according to the testimony of Gel'fand, Rokhlin, and other witnesses) brought functional analysis to Moscow from Europe, and taught a course on functional analysis for professors and students at Moscow University. In Leningrad it consisted of Smirnov and (later) his student Sobolev, who were interested in the theory of operators in applications to mathematical physics. Fikhtengol'ts and his student, the *Wunderkind* Kantorovich, also belonged to this school; they began to study functional analysis in connection with measure theory and approximation theory, and later founded their own school and developed a number of specialized areas of functional analysis. A large school also formed in Ukraine (Odessa and Khar'kov), headed by Krein and Akhiezer. These schools worked energetically from the 1930s to the 1960s. Somewhat later, a new center arose in Novosibirsk. It may be that in no other country was such intense work on functional analysis being carried on as in the USSR at that time.

One of the first mathematical conferences after the beginning of the "thaw" (when all-union conferences became possible at all) took place in January 1956 in Moscow; it rather resembled a mathematical congress. Later these conferences became traditional: Odessa (1958), Baku (1960), the Voronezh schools, the Siberian (Sobolev) conferences, and so on.

Since that time, courses in functional analysis have been part of the core curriculum (sometimes under the heading of "Analysis–N") for the majority of mathematics departments in Russian universities. Moreover, the functional-analytic approach has penetrated almost all of mathematical education and become a compulsory course for mathematicians, and (as a technique) has entered courses on differential equations, calculus of variations, mathematical and theoretical physics, computational methods, and so on. This constitutes the indisputable success of functional analysis — it has become the working language of mathematics.

It is not part of my purpose, and it would not make sense, to list here the results of the "Sturm und Drang" period and the subsequent achievements of functional analysis of those years. Large surveys have been written on this: in *Mathematics in the USSR after 30 Years*; the section on "Functional analysis" in *Mathematics in the USSR after 40 Years* (the most voluminous); a number of surveys in the *Uspekhi Matematicheskikh Nauk*; and other places. The section "Functional analysis" in the *Referativnyi Zhurnal "Matematika"* and the corresponding issues of *Itogi Nauki* give some idea of the scale of research on this subject in the USSR and the rest of the world during this period.

However, it is useful to give some details on the fundamental role of functional analysis and examine what has happened to it.

1. In the 1920s and 1930s three pillars (to use an expression of Dunford and Schwartz) of Banach functional analysis were stated, in the form of three theorems: the Hahn–Banach theorem on extension of functionals; the Banach–Steinhaus theorem on convergence of operators; and the inverse mapping theorem of Banach. The first of these is a fact of convex geometry, which contains a significant portion of linear and geometric functional analysis, in particular, the Krein–Milman theorem, Choquet's theorem, and almost all of duality theory. The Kantorovich – von Neumann theory of linear programming and the theory of linear inequalities are also a portion of convex geometry or, as it later came to be called, convex analysis. Incidentally, this point of view is noticeable in the fifth volume of Bourbaki (topological vector spaces). Convex geometry and convex analysis (without any topology, but including duality) have subsumed this significant portion of the old functional analysis. The Banach–Steinhaus theorem does indeed cover a variety of theorems of the theory of (mainly real) functions, but on closer inspection it is merely a useful abstract form of these theorems. These branches have long since become classical and have been perfected.

From that point on, the role of functional analysis in the theory of functions showed up only in approximation theory, where geometric concepts (diameters, ε-entropy, and so on) in function spaces became the language of approximations.

2. The general theory of operators (including non-self-adjoint operators) on Hilbert space, or the theory of operators on other spaces, turned out to be immense, and the more substantive part of it returned to (complex or real) analysis, where functional analysis serves only as a language, not as a method of investigation. Classical spectral theory, which is one of the principal achievements of the early period, was perfected in its abstract form before World War II. It served as the foundation for the impressive later progress of the spectral theory of differential and other operators. Despite that, the functional-analytic frameworks were mostly background, while the method was classical analysis. Individual particular cases, connected as a rule with applied and physical problems, received a profound development and were quickly isolated. Such are the subtheories of Krein–Gohberg, Foiaş–Nagy, and so on.

On the other hand, the general questions that interested everyone in the early years of development of functional analysis — the invariant-subspace problem and the basis problem — very soon became marginalized and ended (or will end), as a rule, in the construction of counterexamples. Such a thing happened with a cycle of questions about bases: the fantastic example of Enflo brought about a revolution in the understanding of the situation as a whole, and showed how naive the pictures of general Banach spaces had been up to that time.

3. Laurent Schwartz, and before him in some special cases Kantorovich, Fock, Sobolev, Gel'fand, and others, created the important theory of distributions (which the Russians called generalized functions), which was to become generally accepted machinery in the theory of generalized solutions of differential equations, generalized stochastic processes, and other areas. The history of Schwartz's theory of distributions characteristically repeats (on a smaller scale) the history of all of functional analysis — it really did provide a convenient and flexible language for the theory of linear differential operators, but any serious achievement in the latter required the application of "genuine" analysis, complex or other. The theory of Sobolev (and other) classes of functions and embedding theorems turned out to be more fruitful than the general theory of distributions. In any case, it is this that is regarded as an application of functional analysis in the theory of partial differential equations.

4. The theory of locally convex topological spaces and nuclear spaces, which was initiated by the theory of distributions, promoted by Dieudonné in the 1940s, and developed in particular by his student Grothendieck, began to develop rapidly in the 1950s and in its first decade engendered hopes for a renewal of the machinery and stock of examples of spaces in functional analysis (nuclear, countably normed, and other new classes of spaces and operators on them, tensor products, a freer topological theory, duality, and other things). Indeed, some beautiful theorems were proved, for example, that nuclear spaces are more convenient than Banach spaces in a number of questions (the Minlos–Sazonov theorem, the Gel'fand–Kostyuchenko theorem). But the above hopes were not realized. Moreover, Banach analysis turned out to immeasurably more viable than it seemed at the time of the nuclear and locally-convex euphoria. The general theory of locally convex spaces is now almost totally forgotten.

5. The geometry of Banach spaces, which was long based on the examples of a few classical spaces (of the type of $C(X)$, L^p, and so on), gained new momentum during the 1950s and 1960s after the theorem of Dvoretzky and the example of Enflo mentioned above. It became clear how varied this geometry could be. Completely new Banach spaces, pathological at first sight, were discovered. But, unfortunately, these latest examples of spaces have not yet found their place in analysis, although there is no doubt (in my mind) that the basic ideas involved will be useful in other situations.

Dvoretzky's theorem was an especially important discovery. It formed the beginning of a true asymptotic geometric analysis; that is, the study of the geometric properties of spaces of high dimension. Asymptotic geometric analysis is an area that is certainly far from being perfected; it is a current topic today because of its connections with statistical physics, asymptotic representation theory, and other asymptotic problems. But amazingly, here also it turned out that functional analysis had by no means gotten an immediate grasp on a theme that seemingly ought to have preceded the study of actually infinite-dimensional analysis. Von Neumann, in a little-known paper from the 1930s on the asymptotic properties of matrices, called attention (in connection with factor theory) to the absence of research in this area, and indeed such research was very late in appearing.

6. The situation in the noncommutative theory was entirely different. It may be that, in terms of its consequences, this is one of the main results of the development of functional analysis. We remark first of all that, just as vector analysis arose out of linear algebra, the W^*-algebras discovered by von Neumann and the C^*-algebras discovered by Gel'fand and Naimark became a natural infinite-dimensional generalization of semi-simple finite-dimensional algebras. And, just as in the case of linear analysis, the transition to the infinite-dimensional case required entirely new ideas. Of course, the theory of algebras and modules over them is much deeper than the theory of vector spaces and operators on them. It is to the latter that the lion's share of publications on functional analysis in the 1940s and 1950s was devoted.

The discovery of factors and the theory of W^*-algebras by von Neumann, and the parallel theory of C^*-algebras begun by Gel'fand and continued in hundreds of papers, undoubtedly exerted a very strong influence on representation theory, the theory of dynamical systems (cross products), statistical physics, and quantum field theory. But here also it has turned out (at least up to now) that the language created in the framework of this theory plays a much greater role than the theories themselves.

7. Perhaps the most impressive success of pre-war functional analysis was its multiplicative (ring/algebra) version, developed by Gel'fand and his school. It began with a note of Kolmogorov and Gel'fand on what we would now call the functorial relation between compact spaces and rings of functions on them. The theory of normed commutative rings (commutative Banach algebras) gave an extraordinarily beautiful and efficient approach to many problems of complex and harmonic analysis (in particular, the Gel'fand transform). But, as it happens, the beauty of this approach could not overshadow the fact, which became apparent somewhat later, that no new theorems in commutative harmonic analysis had been obtained using this theory. It played a unifying role and corresponded perfectly to the spirit and tradition of all of functional analysis. At the same time, the Gel'fand theory of maximal ideals was a fruitful and timely borrowing from the arsenal of algebraic concepts.

8. The duality theory of Abelian groups, the general theory of unitary representations of locally compact groups and Lie groups (like harmonic analysis and the theory of integration on groups) were formed originally under the unmistakable influence of functional analysis and operator theory. The theory of representations of semi-simple Lie groups, which was begun before World War II mainly by Gel'fand in the USSR and Bargmann in the USA, has nowadays become an enormous area having connections with practically every part of mathematics. But the whole subsequent development of the theory of representations took it far from the original concepts of functional analysis, and it is now connected to a much greater degree with algebraic geometry, number theory, the differential topology of manifolds, singularity theory, complex analysis, partial differential equations, and so on. The same applies to the more recent theories — representations of infinite-dimensional algebras, Kac–Moody algebras, and quantum groups.

9. The interaction of probability theory and functional analysis seemingly ought to have begun simultaneously with the creation of the general theory of stochastic processes, that is, with the theory of measure and integration in function spaces. But this interaction was greatly retarded, even though Kolmogorov had formulated the basic concepts of measure theory in infinite-dimensional spaces as early as the 1930s (immediately after his famous monograph), and Wiener had given his abstract definition of measure already in the early 1920s. The recognition that the general theory of measure in function spaces is a special case of general measure theory (Lebesgue–Rokhlin spaces), and that the theory of stationary processes is a special case of the theory of dynamical systems with an invariant measure, came only much later, in the late 1950s. This point of view became especially important when physical applications required the development of a theory of integration in function spaces. It was not so much probability theory, which has always stood somewhat apart, or functional analysis itself, that stimulated this development; rather it was the needs of physics (the continuous integral, the Feynman–Kac formula, quasi-invariant measures, and other concepts). From that point on the theory of dynamical systems and, in particular, the entropic theory, developed completely independently of functional analysis.

10. The relationship of functional analysis with applied areas, mostly computational mathematics, is peculiar. Perhaps it was here that the influence of functional analysis was especially important. Beautiful examples of this are the Newton–Kantorovich method of solving equations in function spaces and the general theory of approximate methods. The explanation of this special role lies in the fact that it is especially important in the applications of mathematics to have an organizing concept, which functional analysis provided in this instance.

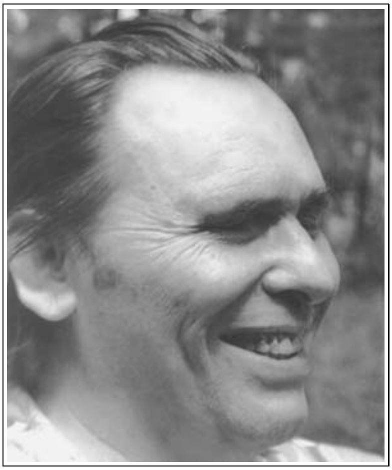
11. The most recent mighty achievements under the canopy of functional analysis are, of course, the index theory of elliptic operators and the related questions of topology, K-theory, and other areas. Here it is not just that the question of the connection between the topology of manifolds and the index of pseudo-differential operators was itself posed by one of the leading lights in functional analysis, but rather that this was the deepest connection between the theory of linear differential operators — such a familiar topic of functional analysis — and the theory of characteristic classes of manifolds. Perhaps it was this period of greatest triumph that began the final dissolution of the concepts of functional analysis in a gigantic unification of topics and ideas, to list which would be equivalent to listing nearly all the rubrics of the review journals.

Even from the rapid and superficial survey that I have given above, one can see how the development of functional analysis itself proceeded and how areas in which it was either a progenitor or a reformer gradually separated from it. No one can diminish the prominent role of functional analysis and the role of the papers written by its leading lights, but one can hardly deny that for some time now it has not existed as an integral area of mathematics.

There is a certain useful inertia in the development of mathematics, which does not allow sudden jumps, and thanks to which some researchers continue to study calcifying topics — this should not, and need not, be neglected. There is a monument to this vanishing subject — it is the journals that bear its name, the departments, majors, and even specialties. For many (myself, for example) it is difficult to state briefly what their specialty is otherwise than as “functional analysis.”

It is clear that this name will be preserved for some time to come and will be adequately understood. However, it will soon be necessary to explain to young people what functional analysis is and what, exactly, the people who call it their specialty are specializing in.

The fate of subjects is capricious and cruel. In contrast to people, they never die completely, and one cannot safely wager that functional analysis as a general concept will not appear again in some other form and under some other name. But indisputably, some very interesting pages of the history of mathematics of the twentieth century, which went out along with it, are connected with its name.



A. G. Vitushkin

Half a Century As One Day

Presented by the author

An abyss opened, stars there spread;
And neither count nor bound is laid.
M. V. Lomonosov

1. How to Study Mathematics?

In my first year at Moscow State University (1949) I was fortunate to attend an unusual seminar. It was an optional beginners seminar on function theory — "Circle of Freshmen," managed by Aleksandr Semenovich Kronrod, then a young Doctor of Science, witty and friendly. He at once became our favorite and we called him simply Sasha as he requested.

We knew that in 1941, when the war broke out, he had volunteered for the front line. He was then a fourth year student. In the army, Sasha was awarded with several medals: the order of the Red Star, the order of the Patriotic War and others. Severely wounded, after a year in hospital he was discharged from the army in 1944. During the following five years Sasha completed his undergraduate and post-graduate studies at the university. Simultaneously, he was working at the Kurchatov Institute. His Ph. D. thesis was recognized by the academic council as outstanding, and instead of a Ph. D., Kronrod received the degree of Doctor of Science. His advisor was Nikolai Nikolaevich Luzin.

A. S. Kronrod, the founder of a scientific school

At that time, seminars for younger students were supplementary to the curriculum: students were reporting on the topics of basic courses. Kronrod's seminar had a different style. It was a training seminar: during the first year no reports were presented, instead all material was offered in the form of exercises — we had to prove even the principal theorems ourselves. We met once a week, but the preparations required several hours every day. Therefore, the number of participants soon reduced very significantly. However, the seminar turned out to be very helpful for those who participated in it. After a year the circle grew into a full scale research seminar.

The first original result was presented by the end of the first seminar year. Its author was Robert Minlos. Kolmogorov recommended Robert's paper for publication in Doklady Akademii Nauk. A freshman's article in a leading journal — this was an event that did not occur frequently. I obtained my first result in the second year and gave a talk on it at the Moscow Mathematical Society meeting a year later.

These examples show that the seminar had been working successfully. To our regret, however, Kronrod did not hold a formal position at the university (his formal position was at the Institute for Problems of Physics). Because of that he had to ask someone else to sign up as our advisor every time we presented our yearly course papers. That did not encourage the enrollment of new participants. Alas, in 1954, after five years of activity, Kronrod's seminar disintegrated, reducing our relations to friendly meetings.

Once I happened to be a tutor in a freshmen class. I had to train backward, that is, lazy students. After one or two weeks they stopped coming to the tutorials for unknown reasons. Instead, the most active students came. This is how my first circle of freshmen appeared (1953).

Later on such circles and other training seminars for younger students were announced by my colleagues and myself every two or three years. Among the students who attended these seminars are Valya Arkharova, Slava Erokhin and Vitya Pan, Dima Arnold and Sasha Kirillov, Marik Mel'nikov and Gena Henkin, Buma Fridman and Shura Tumanov, Valera Beloshapka and Serezha Bychkov, Serezha Ivashkovich and Shura Loboda, Petya Paramonov and Kolya Shcherbina, Kolya Kruzhilin and Stepa Orevkov, Volodya Ezhov and Sanya Isaev, Misha Mishchenko and Misha Smirnov, Andrei Domrin and Stefan

Nemirovskii, Aleksei Bystrikov and Egor Egorov... Many of them, each in his time, came later to our main research seminar.

Perestroika dispersed a good portion of the seminar over various countries. However, the seminar is still active and is periodically replenished with new participants. Many seminar members often travel to international mathematical centers. On the other hand, many former participants working abroad occasionally come to Moscow and attend the seminar, or invite their friends to their home institutes.

I entered the university after graduating from the Tula Suvorov Military School. Such military schools were established during the war, in 1943, in order to revive the old traditions of the former Russian Cadet Corps. A cadet, training to be an army officer in future, was supposed to get a thorough education and a proper up-bringing. In addition to the usual high school curriculum, the Suvorov School offered such subjects as the history of diplomacy, dancing, and higher mathematics.

Here is an example of how we studied higher mathematics. Lieutenant Georgii Ivanovich Bobylev taught mathematics to our platoon. I see now that he was an excellent teacher. His lessons were fascinating and joyful. Once we were not able to differentiate an intricate expression. Calling us slackers, he started explaining the rules of differentiation once again. While he was writing on the blackboard and we were listening to him quietly, our platoon joker, Gena Emel'yanov, purposely whispered so that everyone in the class was able to hear: "Engels said that even a monkey can be trained to differentiate." "But I also can integrate," replied the teacher, thus accepting the joke. By the end of the semester, announcing the grades, he recalled this monkey. While reading out Gena's grades he said: "Emel'yanov — four, four, four," then with a smile he counted the grades: "One, two, three, hence Emel'yanov gets 'three'." Of course, the genuine grade was "four."

At the beginning of my first year at the university I became acquainted with Lev Semenovich Pontryagin. I had learned about him from the tales by Georgii Ivanovich who had attended the university during the same years as Pontryagin and knew him well. Lev Semenovich was an interesting interlocutor. Once I asked him *how to study mathematics*. He answered that it was very easy, *one should just study it* (*mathematics*) *incessantly*, while the choice of the particular subject was not of much importance to start with. This supported my decision to attend Kronrod's seminar. Such reassurance was necessary because the seminar was leaving little time for other studies, and there was a certain risk of failing the semester exams.

In the fifth year at the university, having listened to Pontryagin's course on continuous groups, I was attracted to topology. For a year I attended the Seminar of Pavel Sergeevich Aleksandrov. Then, on graduation from the university, I became a post-graduate student of Andrei Nikolaevich Kolmogorov.

2. Complexity Estimates for Algorithms

Kronrod's main studies were connected with heat engineering and computational mathematics. For this research he was awarded the Stalin Prize. His contribution to fundamental mathematics is the notion of set variations. The evolution of this concept has been treated in two books, namely my M. Sc. thesis *On Multidimensional Variations* (1955) and a book by Leonid Dmitrievich Ivanov *Variations of Sets and Functions* (1975).

For a subset of an n-dimensional space, one can define its variations of orders $k = 0, \ldots, n$. The k-th variation is the integral over the space of $(n-k)$-dimensional planes of the number of connected components in the intersection of this subset with each plane.

Let us consider a set in $\mathbb{R}^3$ looking like a rope, for instance like a clothesline. Four variations are defined in this case. The zero variation of the rope is the number of its pieces. The first variation is equal to its length in the everyday sense, rather than in the sense of Hausdorff. The second variation is equal to the Hausdorff area of the surface of this rope. The last variation is equal to its volume.

Variations are rather convenient because they characterize the "breadth" of the set in all dimensions simultaneously. This can be used to estimate the complexity of various mathematical objects. In particular, this property of variations was successfully applied to the study of compact sets in function spaces. Such expressions as "estimate of the complexity of a problem" and "estimate of the complexity of an algorithm," which are quite common now, were first adopted in this context.

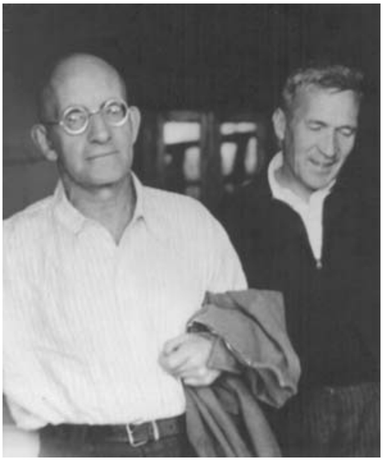
A complexity estimate for a compact set of functions can be obtained in the following way. We project this set onto $\mathbb{R}^n$ by restricting all functions to a sufficiently dense net, and approximate the image of the projection by an algebraic surface. The variations of the image set can be estimated in terms of the degree and dimension of this surface. Both parameters can be calculated for the classes of smooth or analytic functions, and this approach yields some interesting results.

It has been shown that if the complexity of a function is measured by the ratio of the number of variables to the order of smoothness, then *almost every function of a given complexity cannot be represented as a superposition of functions of lower complexity.*

In particular, for all positive integers n and s, there exists an s times differentiable function of n variables that is not a superposition of s times differentiable functions of fewer variables. This was proved in 1953.

Superpositions of functions constituted the subject of my talk at the International Congress of Mathematicians held in Moscow in 1966. In 1977, I gave a series of lectures on this topic at the University of California at Los Angeles, where I was invited as a Lecturer of the International Mathematical Union (such a lecturer is appointed on application by a leading university, and the lectures are published in a separate volume).

My first results on superpositions gave me an opportunity to become acquainted with Kolmogorov. I asked him to recommend my two communications on this work to *Doklady*. Having listened to the formulation of the result Kolmogorov uttered his usual long "er..." and added: "Yes, it is correct, and I see how it can be done." I was a bit discouraged. Pavel Sergeevich Aleksandrov, who took part in the conversation, comforted me: "Don't get upset, Tolya. Andrei Nikolaevich understands everything." A week later I came to learn Kolmogorov's decision and had another surprise. He had written both communications anew and even typed them up, explaining in this unusual way how an article should be prepared for publication.



P. S. Aleksandrov
and A. N. Kolmogorov

Andrei Nikolaevich advised me to take a post-graduate course at the Steklov Mathematical Institute so that later on I would be able to get a research position there. On Kolmogorov's recommendation, Ivan Matveevich Vinogradov invited me for an interview. The interview was going smoothly until Vinogradov learned that I was Kronrod's student. The conversation then took an undesirable turn. Sergei Mikhailovich Nikol'skii, who was also present there, tried to support me. However, Ivan Matveevich cut short my visit, giving his toothache as an excuse. When I left the room he said to Nikolskii: "Never mind. He has nowhere else to go if not to us." As the landowner Lasukov (a character from *Autumnal Bore* by

I. M. Vinogradov, Director of the Mathematical Institute

N. A. Nekrasov) used to say, it did turn out his way. I became a post-graduate at the university and got a position at the institute only eleven years later. This example shows that Vinogradov's position towards Jews left much to be desired. In other aspects he was an adequate head of the institute and much respected, especially for his straightforwardness and consistency. When a group of leading academicians was asked to sign an open letter condemning Sakharov for his dissident activities, only two of them refused to add their signatures, and these were Kapitsa and Vinogradov.

Complexity estimates for algorithms computing smooth and analytic functions were obtained by the same method that had been used to study superpositions (1957). Under suitable assumptions on the functional compact set F, it was proved that: if an algorithm achieves an ε-approximation of every function in F, if p is the number of parameters determining a function, and if the total degree of the formula computing the function does not exceed k, then $p\log_2(k+1) \geqslant cH_\varepsilon$. Here, c is a constant depending on the compact set F, and H_ε denotes the ε-entropy of F, i. e., $\log_2$ of the number of elements of a minimal ε-net for F. From the point of view of such complexity estimates the well-known approximation schemes for smooth and analytic functions are close to being optimal. This can be understood in the following way: *there does not exist a significantly better approximation method for these classes of functions*.

A. A. Lyapunov lecturing on cybernetics

This result was obtained when I was a post-graduate student and working at the same time at the institute now known as the Keldysh Institute of Applied Mathematics. I joined this institute with a group of engineers. We were constructing a reading device, and during intervals in this work I turned to mathematics.

The head of the Cybernetics Department at the institute was Aleksei Andreevich Lyapunov, gentle and amiable in communication, engaged and zealous in research. In those days (the 1950s) newspapers and even our textbooks did not call cybernetics (as well as genetics and some other sciences) anything other than a "bourgeois pseudo-science." Lyapunov did not spare himself in organizing this department and editing the journal *Osnovy Kibernetiki* (Principles of Cybernetics).

Mstislav Vsevolodovich Keldysh was the chairman of the Special Committee of the Presidium of the Academy of Sciences on man-made satellites, and together with Sergei Pavlovich Korolev he supervised the Outer Space Project. The Keldysh Institute performed the mathematical part of the Project. The institute carried out research in many other applications as well. At the same time, studies in pure mathematics were given due attention, and the institute was a good place to work. Keldysh and Lyapunov paid attention to my results. However, there was a great drawback, and that was the secrecy conditions. Everyone on the staff had one or another form of access to secret work, even if one had nothing to do with it. Such conditions limited severely professional contacts and I transferred to Mergelyan's Department at the Steklov Institute.

S. P. Korolev and M. V. Keldysh
are discussing the Space Project (1961)

The interest in superpositions is motivated by their applications. Let me give an example of how superpositions are used in computational mathematics. Suppose that an algorithm for computing a function of n variables is given, and it is necessary to produce another algorithm computing this function together with its gradient. This problem arises, for instance, when looking for the extremum of a function. The question is: By how much does the number of arithmetic operations increase? The usual guess is that the number of operations increases at least by a factor of n, but this is wrong.

Klim Vladimirovich Kim observed that this problem could be solved so that the number of operations increased only by 3 or 4 times (asymptotically for large n). The idea is as follows. Every formula corresponding to a sequence of arithmetic operations is a superposition of functions of 2 variables. Therefore, the formula computing a partial derivative of the initial function is a

K. V. Kim and V. V. Voevodin are mastering agriculture (the 1990s)

superposition of partial derivatives of representing functions. The complexity of computing a function and its derivative is approximately the same. Hence, computing the function together with its gradient increases the number of operations much less than by n times. In 1984 Kim designed a computer program based on this idea. This is the best result on superpositions.

Here is one more question of this type with an unexpected answer. How many arithmetic operations are needed to evaluate a polynomial of degree n in one variable? The classical Horner method requires n additions and n multiplications. Viktor Pan found a more efficient method using n additions and only $[n/2]+1$ multiplications, and proved that there is no algorithm containing fewer operations (1960).

3. Digital System of Sound Recording

An All-Union Congress on Communication Reconstruction and Low-Current Industry Development was to be held in 1933. Vladimir Aleksandrovich

Kotel'nikov prepared a talk "On Carrying Capacity of 'Air' and Wire in Electric Communication." The congress was not held but the proceedings were published.

The main thesis of Kotel'nikov's contribution was that *the amount of information received through a communication channel is proportional to the band width of the channel.*

In textbooks, a more rigorous formulation of this assertion is now called Kotel'nikov's theorem: *An entire function of type σ, square-summable on the real axis, can be represented in the form*

$$f(t) = \sum_{k=-\infty}^{\infty} f(t_k)q_k(t), \qquad \textit{where} \qquad t_k = \frac{k\pi}{\sigma} \quad \textit{and} \quad q_k(t) = \frac{\sin(\sigma(t-t_k))}{\sigma(t-t_k)}.$$

Indeed, it follows from this formula that the "amount of information," i. e., the number of independent numbers, carried by a signal of spectrum σ per unit time is equal to σ/π. Such formulas had been known before Kotel'nikov. His discovery consisted in a reasonable choice of the function class and the understanding of potential applications.

This was the first time that the informational aspect of communication problems was discussed, and his idea forms the basis for the current theory of information. Kotel'nikov's work has been awarded many prizes, including the Lomonosov Medal (the USSR, 1981) and the Bell Medal (the USA, 2000).

V. A. Kotel'nikov

Kotel'nikov's 1933 paper dealt with an important problem of radio communication which was quite complicated in those days. If a radio signal uses a narrow band width, the signal received does not have the quality needed owing to the lack of higher frequencies. When a wide band width is used, noise from various disturbances increases. Moreover, there had always existed the problem of signal compression on the air. Efforts to overcome these difficulties by a transformation of the emitted signal had been attempted. Kotel'nikov explained that such attempts had no prospect of success and thus he stopped the useless activity of inventors of an informational "perpetuum mobile."

Kotel'nikov's principle can be made more precise by using entropy estimates of the relevant function class. Kolmogorov and Tikhomirov showed (1959) that the ε-entropy (in the uniform norm) of the compact set of functions obtained as restrictions of entire functions of type σ to the interval $[-T,T]$ is asymptotically equal to $\frac{2T\sigma}{\pi}\log_2\frac{1}{\varepsilon}$ (for small ε and large T). It follows that if every function (from the specified class) transmitted through a communication channel can be recovered with accuracy ε, then the amount of information (i. e., the number of binary digits) that can be transmitted through this channel per unit time is asymptotically equal to $\frac{\sigma}{\pi}\log_2\frac{1}{\varepsilon}$.

Kotel'nikov's formula found various applications in the late 1970s when it became possible to replace the analog recording system with a digital one. The formula has become the main means of recording in all the instances when the signal spectrum is known. In particular, all digital devices for sound recording use this formula. This is due to the fact that the manufacturing of formula-based devices is low-cost, while the quality of the playback signal on a CD is almost perfect. But there always remains the problem of minimizing the algorithmic complexity. For instance, nowadays on the Internet the flow of the sound and video recordings is so large that it becomes necessary to save memory and time consumption. Thus, the quality of recovery we enjoy is not the best.

It is great to listen to music not only in concert halls, but at home as well. However, bad room acoustics change the frequency characteristics of the whole reproducing system, thus constraining significantly the playback quality. If a powerful computer is available, Kotel'nikov's formula gives the possibility of smoothing out these characteristics. This can be achieved by replacing the function $f(t)$ received from the record-player with another function $f^*(t)$ determined by the collection of values

$$f^*(t_k) = \sum_{n=-N}^{N} \varepsilon_n f(t_{k-n}), \quad t_k = \frac{k\pi}{\sigma},$$

with sufficiently large $N \in \mathbb{N}$ and appropriate $\{\varepsilon_n \in \mathbb{R}\}$. It is important that no preliminary processing of the function $f(t)$ is needed.

In sound recording the quality of the equipment is characterized by three parameters, namely: frequency response range, nonlinear signal distortion (calculated as a percentage of the mean square signal norm), and the device dynamic range (defined as the norm ratio of the maximum and the minimum signals with guaranteed small nonlinear distortions).

Sufficiently "good" values of the last two parameters may be obtained by improving the accuracy of approximation. If one uses Kotel'nikov's formula, taking the numbers $f(t_k)$ as the code, then the code length increases significantly. *It is more efficient to employ the Weierstrass formula* that restores a function from its zeroes, taking the code in this case to be the coordinates of the zeroes.

Quite unexpectedly, it turns out that *there exists a coding system that provides an arbitrarily wide dynamic range for a communication channel or a* *device, while the code length is independent of the dynamic range.* This fact explains the mechanism of noise-suppression systems which are used both in analog and in digital sound recording. This work was done by Viktor Ivanovich Buslaev and myself in 1972. Coding of signals and sound recording problems were the subject of my plenary talk at the International Congress in Vancouver (1974).

Similar estimates for the dependence of code length on the dynamic range of a communication channel for video signal have not been obtained as yet. Such a result would help to assess how close to optimal are the coding methods used in video-devices.

Viktor Ivanovich is pleased with the communication quality

4. On Hilbert's Thirteenth Problem

The general algebraic equation can be simplified by the Tschirnhausen transformation, which is written in terms of radicals. In particular, the general equation of degree 7 can be reduced to the form $f^7 + xf^3 + yf^2 + zf + 1 = 0$. Hence, the solution of the general equation of degree 7 is a superposition of arithmetic operations, radicals, and a function f of the three variables x, y, and z. Further simplification of this equation by means of algebraic transformations does not seem to be possible. In the famous list of Hilbert's problems (1900) this conjecture is stated under No. 13 in the following form: *A solution of the general equation of degree* 7 *cannot be represented as a superposition of continuous functions of* 2 *variables*. Here and below, in talking about a solution of an algebraic equation, we mean a univalent branch of the general solution. By the way, Hilbert's conjecture is no less exciting if one is interested in the general solution and multivalued continuous functions of two variables.

Many mathematicians found this problem appealing. The result on superpositions of smooth functions, mentioned above (see section "Complexity Estimates for Algorithms"), came about in relation with this problem. We tackled some problems on this subject in a freshmen circle (1954–1955). Dima Arnold and Sasha Kirillov were the best in that seminar. By the way, they were passionate hikers and often took other members of the circle with them. They did not forget me and I sometimes took part in their hiking or canoeing trips. Some of the girls of their class (their future wives included) attended the seminar as spectators. The presence of beautiful girls heated up their rivalry, and helped make the seminar so successful. At the same time I became interested in radio engineering. That took all my time, and to my regret I had to close this seminar after a year. Kirillov went to Gel'fand, while Arnold joined Kolmogorov.

Kolmogorov said that Hilbert's thirteenth problem was very good material for students. With time, it turned out that it was good not only for students but for Kolmogorov himself. Two years of collaboration between Kolmogorov and Arnold resulted in the proof that Hilbert's original conjecture was wrong. The breakthrough was Kolmogorov's result that a continuous function of several variables could be represented as a superposition of functions of three variables (1956).

Kolmogorov used to say that the idea of this construction originated while looking through outdated journals, as was his habit. He came across an article by Kronrod which treated, among other things, functional trees. The tree of a function is the space of its level components. A tree is one-dimensional and acyclic and hence can be homeomorphically embedded into the plane. The values of the function are naturally carried forward to its tree and, in this sense, a function of several variables depends on two variables only. Constructing superpositions required an additional variable, which resulted in superpositions of functions of three variables.

Kolmogorov was an excellent academic supervisor. When he worked with students he always created an opportunity for a gifted student to play a solo part, so to say. This was the case with Dima Arnold who in his third year at the university improved the Kolmogorov construction and showed that any continuous function of three variables is representable as a superposition of continuous functions of two variables, thus proving that *Hilbert's conjecture was incorrect* (1957). Then Kolmogorov found a new construction and proved the possibility of representing every continuous function of several variables as a superposition of continuous functions of one variable and the operation of addition (1957).

Dima Arnold is dreaming of solving Hilbert's 13th problem

This cycle of works by Kolmogorov and Arnold was a sensation. Soon after that, however, it was proved that the functions in their constructions could not be smooth even if the superposition represented an analytic function

(1959). Therefore, having in mind that almost every smooth function is not representable as a superposition of smooth functions of fewer variables, an affirmative answer to the problem may still be expected, i. e., it is still possible that *the solution of the equation of degree* 7 *cannot be represented as a superposition of functions of* 2 *variables* if these functions, of course, are assumed to be smooth or analytic. On the other hand, it is not excluded that every algebraic function is a superposition of algebraic functions of one variable and arithmetic operations. Thus, the *problem remains open* and the range of issues is, in fact, as wide as it was at the beginning of the century.

By the end of his post-graduate course Arnold submitted his work on Hilbert's thirteenth problem as his Ph. D. thesis. I was the official opponent during the thesis defence. Kolmogorov made an interesting remark about Arnold: "Vladimir Igorevich never eats, nor sleeps . . . well, of course, he eats and sleeps, but even then he is thinking of mathematics." Izrail Moiseevich Gel'fand suggested that Arnold should be given the degree of Doctor of Science for the solution of Hilbert's problem. Such pulling of the blanket was doing injustice to Kolmogorov. I said that to disprove a conjecture was not to solve the actual problem, however the Doctor of Science degree may be given to Academician Kolmogorov rather than to Arnold. The discussion was lively, but it did not hurt Arnold's reputation, only adding to his popularity. Very soon he received the Doctor of Science degree for his work on dynamical systems, while I have been reminded of my opposition more than once.

In the 1950s, the curriculum of the mathematics department at the university included a course of pedagogics followed by teaching practice at a school. Some pupils of the school to which I was assigned had no difficulties with mathematics. A few boys (Dima Arnold among them) were eager to solve problems from the university calculus course, which I was giving them during the breaks. However, my test lesson was a failure. I had to examine four pupils and to explain new material. The first boy got a "five" quickly. But I had no luck with another boy. He did not understand the simplest things and, being inexperienced, I decided to explain to him everything on the spot. When the bell for the break rang he was still struggling at the blackboard unable to solve a simple equation, while I, fully conscious of my mistake, was wondering if this nice guy should be tortured with mathematics . . . One could not help thinking of Mephistopheles:

> *Dear friend, all theory is grey,*
> *And green the golden tree of life.*
>
> (Goethe)

5. Analytic Capacity and Approximation of Functions

Sergei Nikitovich Mergelyan is another bright star in the constellation of Russian mathematicians. A child prodigy and the one chosen by fortune, he got the degree of Doctor of Science when he was 20, while at the age of 24 he was awarded a Stalin Prize and elected a Corresponding Member of the USSR Academy of Sciences (1952). His theorem on approximation of functions by polynomials is a classical one, along with the theorems of Weierstrass and Runge. His lectures attracted great attention, though female students were more interested in the lecturer himself, a young blue-eyed professor.

S. N. Mergelyan

The Mergelyan theorem answers a question about the possibility of polynomial approximation of functions of one complex variable. Every function continuous on a compact set $K \subset \mathbb{C}$ and holomorphic in its interior can be represented in K by a uniformly converging series of polynomials if, and only if, the complement $\mathbb{C} \setminus K$ is connected (1952).

This result concludes a long series of studies on polynomial approximation of functions of one complex variable (Walsh, Hartogs and Rosenthal, Lavrent'ev, Keldysh). In all these papers a function continuous on a compact set and holomorphic in its interior is approximated by a function holomorphic on the entire compact set (i. e., in a neighbourhood of this set). Polynomial approximation is then obtained by the Runge theorem (1885) that every function holomorphic on a compact set whose complement is connected can be represented in this set by a uniformly converging series of complex polynomials. Schemes of approximations had been improved, step-by-step completing the list of sets on which holomorphic approximations are possible. However, the criterion for holomorphic approximations was obtained only fifteen years later after Mergelyan's work.

Contrary to the case of polynomial approximation, there is no simple geometric criterion for approximation by rational functions or, equivalently, by functions holomorphic on a compact set, and we shall see that such a simple criterion cannot exist. To describe the compact sets on which holomorphic approximation is possible, a notion was introduced in 1958, which was called the *analytic capacity* of a set, following Slava Erokhin. The analytic capacity of a subset $E \subset \mathbb{C}$ is defined by the formula $\alpha(E) = \sup\left|\lim_{z\to\infty} zf(z)\right|$. The supremum is taken over the class of functions continuous in the complex plane, bounded by 1, holomorphic on the complement of E, and vanishing at infinity.

Approximation by holomorphic functions on a compact set is possible if there is enough room in its complement near the boundary to "push" the boundary off the compact set. The *approximation criterion* is formulated as follows (1966): every continuous function holomorphic in the interior E_0 of a compact set $E \subset \mathbb{C}$ can be uniformly approximated by functions holomorphic in a neighbourhood of E if, and only if, $\alpha(D \setminus E) = \alpha(D \setminus E_0)$ for every open disc $D \subset \mathbb{C}$. This condition is satisfied if, for instance, the interior boundary of the compact set is empty, i. e., if every boundary point of E lies in the boundary of some component of its complement. In particular, this holds if the complement is connected, and therefore the *Mergelyan theorem can be regarded as a corollary of this criterion and the Runge theorem.*

The result did not go unnoticed. I was invited to the International Congress held in Nice in 1970, but I was not permitted to go there for the "signing" (see the last section).

Let us mention some properties of the analytic capacity $\alpha(E)$. The analytic capacity of a set of finite length is zero. The capacity of a disc is equal to its radius. Furthermore, the only set of fixed area providing the minimum value of capacity is a disc. The capacity of a domain is at least one fourth of its diameter. A fairly unexpected and equally useful property is the *"instability of capacity"* (1958): if a set $E \subset \mathbb{C}$ is such that

$$\limsup_{r \to 0} r^{-2} \alpha(E \cap D(z,r)) > 0 \quad \text{for every point } z \in \mathbb{C}$$

(where $D(z,r)$ denotes the open disc of radius r centered at z), then $\alpha(E \cap D(z,r)) = r$ for all z and r.

Marik Mel'nikov,
the USSR bridge champion

A notable event was the following theorem, due to Mel'nikov (1966), providing an important *estimate of the Cauchy integral*. If a function $f(z)$ is continuous on the disc $\{|z| \leqslant 1\}$ and the set E where this function is not holomorphic stays off the boundary of the disc, then $\left|\int_{|z|=1} f(z)\,\mathrm{d}z\right| \leqslant c\alpha(E)$, where c is an absolute constant.

This subject still remains popular. From the point of view of possible applications, the most attractive unsolved problem is the following conjecture: *Analytic capacity is semi-additive, i. e., $\alpha(A \cup B) \leqslant \alpha(A) + \alpha(B)$ for every two compact sets $A, B \subset \mathbb{C}$.* This conjecture is more than forty years old, but the question remains open even if we multiply the sum on the right-hand side by an absolute constant. New hope that the problem might be solved in the affirmative was raised in 1998 when a young Spanish student of Mel'nikov, Xavier Tolsa, established the semi-additivity property for the so-called positive analytic capacity γ^+. This capacity is defined in the same way as above but the supremum is taken over the class of functions bounded by 1 and representable by the Cauchy potential of a positive measure supported on the given set.

6. Removable Singularities of Holomorphic Functions

The formulation of the problem about sets of removable singularities of holomorphic functions and early results on this subject are due to Painlevé (1888). A subset of the complex plane is said to be removable (for the class of bounded holomorphic functions) if every bounded holomorphic function defined in a deleted neighbourhood of this set can be extended holomorphically over this set. Guy David showed in 1999 that a compact set of finite Hausdorff length is removable if, and only if, its linear variation is zero — in other words, if the projection of this compact set to almost every line has measure zero. This statement came up as a conjecture in our seminar about four decades ago after an example of a removable set of nonzero length had been constructed. It was noticed that the linear variation of this exotic set is zero, and this suggested the formulation of the conjecture. Now the Painlevé problem may be regarded as solved, because he himself considered only sets of finite length and did not have any definite conjecture at all.

Evgenii Prokof'evich in the country

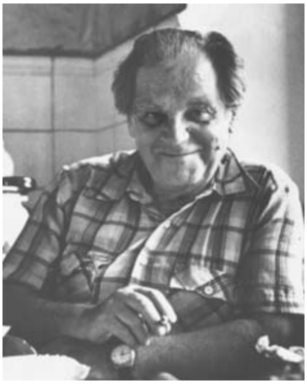
An attractive variant of the problem on removable sets was introduced by Evgenii Prokof'evich Dolzhenko. In 1963 he proved that a compact set is removable for the class of holomorphic functions satisfying a Lipschitz condition with exponent $0 < s < 1$ if, and only if, it has $(1+s)$-dimensional Hausdorff measure zero. A Vietnamese mathematician Nguen Xuan Uy (then a young student of John Garnett) showed in 1977 that the analogous statement is true for $s = 1$. The estimates of the Cauchy potential obtained in this work have found many applications and were used in approaching the original Painlevé problem.

David's theorem concludes a large series of works, some of which had been presented at International Congresses (Calderón (1978), Murai (1990), Mattila (1998)). Lately, the direction of research in this area has been determined by the work of Mel'nikov on the curvature of measure (1994). This notion suggests new problems, and further interesting developments can be expected, particularly in connection with the semi-additivity conjecture for analytic capacity.

7. Integral Representations of Functions

In 1964 the Ukrainian Academy of Sciences held a School on Complex Analysis and Topology in Uzhgorod. Discussions about the problems of complex analysis in several variables started there and continued at our seminar in Moscow.

Boris Vladimirovich Shabat gave several introductory survey lectures on the theory of functions of several complex variables. Later on, he and his students V. A. Zorich, E. M. Chirka, S. I. Pinchuk, A. Sadullaev and others determined in many respects the subject of the seminar. At that time Vasilii Sergeevich Vladimirov gave several survey talks on various aspects of complex analysis related to mathematical physics. At present, A. G. Sergeev and A. V. Domrin are in charge of this field at the seminar.



B. V. Shabat

Vladimirov and Shabat were both former front-line soldiers during the war. This fact could be omitted because almost everyone of their generation took part in the war, but there is a story worth telling. Shabat had badly hurt his foot in an accident. But he was so dextrous with a prosthesis that he could walk well and even run. He went to the army as a volunteer and therefore no medical examination was required. The prosthesis was not noticed and he was sent to the artillery corps. Many years later Boris Vladimirovich applied to a military registration office for a war participant certificate. This time the prosthesis was discovered and no certificate was given because, as he was told, a person in such a state could not have been drafted to the army and sent to the front line, especially not to the artillery corps.

Approximation problems for holomorphic functions of several variables were a principal subject of our seminar. The intricacy of these problems lies in the fact that, contrary to the case of one variable, it is very hard to obtain a convenient integral representation of functions of several variables. There is the

Gena Henkin
in the village of Laptevo

Runge theorem analogue for polynomial polyhedra in $\mathbb{C}^n$, which is a consequence of the integral formula discovered by A. Weil in 1931. There were also formulas for some other particular classes of domains, but a general formula was still missing. In 1956 Leray found such a general formula by embedding the domain into higher-dimensional spaces and obtaining the integral kernel as the restriction of a special differential form common to all domains. This explained diversity in notation and, in some cases, helped to choose a kernel appropriate for domains of a given type.

The *integral representation of functions on strictly pseudoconvex domains* with a sufficiently smooth boundary obtained by G. M. Henkin (1968) was an important accomplishment of the seminar. For domains of this type he established the analogue of the Mergelyan theorem. Henkin was an invited speaker at the Warsaw Congress in 1983. However, many well-known old problems still remain open. In order to characterize the state of the art, let us mention two unsolved problems.

Bishop's conjecture: The union of finitely many pairwise disjoint closed balls in $\mathbb{C}^n$ is polynomially convex, i. e., for every point in the complement of the set, there is a polynomial equal to 1 at that point and less in modulus than 1 on the set. Even Eva Kallin, who is a great master of this art, can prove it only if the number of balls is less than four (1964).

Wermer's conjecture: If an arc in $\mathbb{C}^n$ is polynomially convex, then every continuous function on this arc can be uniformly approximated by polynomials. Chirka proved this for arcs of Hausdorff 2-measure zero (1966).

8. Continuation of Analytic Elements

In 1907, while working on the classification problem for domains in $\mathbb{C}^n$ and trying to reduce it to the classification of real hypersurfaces, Poincaré proved the following model assertion. If a holomorphic map defined in a neighbourhood of a point on the $(2n-1)$-dimensional sphere maps the points of this sphere into a sphere of the same dimension, then this map extends holomorphically onto the entire sphere and then onto the ball bounded by it, and so is fractional-linear.

Much later, in 1974, Alexander proved this statement anew, without knowing about the work of Poincaré. This result was noticed, and the papers of Poincaré, É. Cartan (1935), Tanaka (1962), and others on this subject were recalled from oblivion. Chern and Moser published their influential work on the invariants and normal forms of real-analytic hypersurfaces (1974).

This set of problems came to our seminar in due time. A remarkable result was obtained by Sergei Pinchuk in 1978. Every holomorphic map defined in a neighbourhood of a point on a compact strictly pseudoconvex nonspherical real-analytic hypersurface in $\mathbb{C}^n$, and mapping this hypersurface into another hypersurface of this type, extends holomorphically along every path in the first hypersurface (a hypersurface is called nonspherical if it is nowhere locally biholomorphic to the quadric). The proof used Fefferman's metric adapted to hypersurfaces in $\mathbb{C}^n$, and therefore worked only in this case.

S. I. Pinchuk

The proof of the analogous statement for real hypersurfaces in general complex manifolds required other methods. This *"mapping germ theorem"* was established in 1985.

It was obtained after a series of preliminary studies on nonspherical hypersurfaces with nondegenerate Levi form. For such hypersurfaces, Beloshapka and Loboda proved that every automorphism fixing a point is completely determined by the restriction of its linear part onto the complex tangent plane at this point (1981). Kruzhilin and Loboda showed that the local automorphism group of a hypersurface with positive-definite Levi form is linearizable, i. e., all the automorphisms can be simultaneously made linear by a special choice of coordinates (1983). For the same class of hypersurfaces, Beloshapka obtained an estimate for the convergence radius of the power series defining a local automorphism (1985).

Huckleberry and Kruzhilin (Moscow, 1998)

These works have resulted in many invitations. Talks have been given at the seminars of Lelong and Dolbeault (Institute Henri Poincaré), Carleson (Mittag-Leffler Institute), Grauert (Göttingen University), Huckleberry (Ruhr University of Bochum), Manin (Max-Planck Institute, Bonn) and, of course, at seminars of several Russian universities. Kruzhilin was an invited speaker on this subject at the International Congress in Berkeley (1986).

On this day of 1994 Ivashkovich and Paramonov became Doctors of Science
(Ivashkovich, Paramonov, the Beloshapkas, Sergeev)

This subject found further evolution in the work of Sergei Ivashkovich on analytic continuation of holomorphic mappings of complex spaces. In particular, he proved the *conjecture of Griffiths* that a meromorphic mapping from a domain in a Stein manifold to a Kähler manifold extends meromorphically onto the envelope of holomorphy of this domain (1992).

9. The Jacobian Conjecture

This is the following assertion: If the Jacobian of a pair of polynomials P, Q in two variables x,y is a nonzero constant, then for all a and b the system $P(x,y)=a$, $Q(x,y)=b$ has a unique solution. More often this is stated in a different way: If a polynomial endomorphism of $\mathbb{C}^2$ is locally invertible, then it is globally invertible (and hence it is an automorphism). This problem was posed by Keller in 1939.

The polynomiality assumption is necessary. For instance, the holomorphic map $x' = \mathrm{e}^x$, $y' = y\mathrm{e}^{-x}$ is not invertible, although its Jacobian is identically unity. There exists a more subtle example (Fatou, 1922) of a strict holomorphic embedding of $\mathbb{C}^2$ into itself with constant Jacobian. This map is not an automorphism because its image does not cover the entire plane.

The group of polynomial automorphisms of $\mathbb{C}^2$ is characterized by *Jung's theorem* (1942): every polynomial automorphism of $\mathbb{C}^2$ is a composition of affine maps and triangular transformations, i. e., automorphisms of the form $x' = x + cy^n$, $y' = y$ ($c \in \mathbb{C}$, $n \in \mathbb{N}$).

The invertibility question for polynomial endomorphisms of $\mathbb{R}^2$ can be formulated in another way. Let us weaken the assumption that the Jacobian is constant and assume only that the map is locally invertible. The answer in this case turns out to be negative. In 1994, Sergei Pinchuk constructed an example of a polynomial map of $\mathbb{R}^2$ into itself that is invertible locally but not globally. (Pinchuk was an invited speaker at the International Congress in Berlin, 1998.) Nonetheless, there is not much hope that there exists a counterexample to the classical variant of the conjecture. Furthermore, it is known that the Jacobian conjecture holds true for polynomial maps of $\mathbb{C}^2$ of algebraic degree less than 100 (Moh, 1983).

Let f be a polynomial endomorphism of $\mathbb{C}^2$. The following procedure is helpful for the understanding of the geometry of f. Consider $\mathbb{C}^2$ as part of the projective plane $\mathbb{C}\mathrm{P}^2$. In a neighbourhood of the projective line at infinity, the map f is given by rational functions which have a finite number of ambiguous points. Let us blow up each of these points by replacing it with a Riemann sphere via a coordinate change of the form $x \to xy$, $y \to y$. For instance, this coordinate change transforms the rational function x/y into the linear function x. In general, new ambiguous points may appear on the exceptional spheres of blow-ups so that we must blow up these as well. However, after a finite number of blow-ups we obtain a compact complex manifold consisting of $\mathbb{C}^2$ and a tree of spheres, together with a holomorphic map of this manifold into $\mathbb{C}\mathrm{P}^1 \times \mathbb{C}\mathrm{P}^1$ that coincides with f on $\mathbb{C}^2$. Deleting some of these spheres, we can get a manifold M consisting of $\mathbb{C}^2$ and a finite union of pairwise disjoint complex lines $\{L_j\}$. This can be done so that for every j, each of the two components of the map is identically equal to infinity on L_j, or does not attain the value infinity on L_j at all. We delete the minimal number of spheres and in this sense obtain a complete set of exceptional lines.

For example, a triangular automorphism of $\mathbb{C}^2$ corresponds to the so-called triangular chain of blow-ups. The tree of spheres $\gamma_0, \gamma, \gamma_1, \gamma_2, \ldots, \gamma_{2n}$ in this case possesses a very simple structure. The sphere γ is obtained by blowing up a point in the projective line $\gamma_0 = \mathbb{C}\mathrm{P}^2 \setminus \mathbb{C}^2$. The sphere γ_k $(k = 1, \ldots, n)$ is obtained by blowing up a point $p_k = \gamma \cap \gamma_{k-1}$. For $k = n+1, \ldots, 2n$, the sphere γ_k is obtained by blowing up a point that lies only in the previous sphere γ_{k-1}. A characteristic property of triangular chains is that, by blowing down the spheres $\gamma_0, \ldots, \gamma_{2n-1}$, we retrieve the projective plane $\mathbb{C}\mathrm{P}^2 = \mathbb{C}^2 \cup \gamma_{2n}$. So we obtain the map from $\mathbb{C}\mathrm{P}^2$ onto itself. Its restriction to $\mathbb{C}^2$ is the original triangular automorphism.

The first homology group of the manifold M mentioned above is trivial, whereas the second homology group is nontrivial. Each complex line L_j defines a nontrivial cycle represented by a smoothly immersed two-sphere in M having intersection index 1 with this line. This sphere can be obtained, for instance, by gluing a disc contained in $\mathbb{C}^2 \subset M$ to the boundary of a small disc transversal to that line. In our seminar we are used to call such a surface a *test sphere*. This terminology is justified by the fact that the topological characteristics of test spheres determine, to a great extent, the properties of the manifold M. For instance, sometimes the invertibility of the corresponding map can be examined using such characteristics.

If the Jacobian conjecture is false, then there exists a locally invertible polynomial endomorphism $f\colon \mathbb{C}^2 \to \mathbb{C}^2$ that is not globally invertible. Let us remove from the corresponding manifold M all the lines L_j on which at least one of the components of our map equals infinity. This gives us another manifold M^* consisting of $\mathbb{C}^2$ and a finite number of complex lines. The extended map f^* sends M^* holomorphically onto $\mathbb{C}^2$. This map is proper and locally invertible outside of the remaining lines L_j — and, since it is not invertible globally, it has a ramification along some of these lines. In this way we obtain a rather exotic holomorphic ramified covering $f^*\colon M^* \to \mathbb{C}^2$, and the problem is to show that this cannot exist.

It turned out, however, that removing the holomorphicity assumption allowed one to construct such a covering (1970). Namely, *there exists a ramified covering of three sheets over $\mathbb{C}^2$, glued together of $\mathbb{R}^4$ and $\mathbb{R}^2$, and such that the projection is locally invertible on $\mathbb{R}^4$ and has two sheets near $\mathbb{R}^2$.*

Stepa Orevkov is meditating on the Jacobian conjecture

Unfortunately, no counterexample to the Jacobian conjecture came out of it. In 1986 Stepa Orevkov showed that there exists no such ramified covering of three sheets over $\mathbb{C}^2$ with holomorphic structure. Nevertheless this example still works in those instances when one more "proof" of the conjecture has to be checked for a mistake. For over 15 years (1955–1970) the conjecture had been considered proved, and the disproving arguments were found only with the construction of this example.

An important success of our seminar was the work of Stefan Nemirovskii (1998). He answered several questions about the envelopes of holomorphy of test spheres raised in our seminar in the 1980s. In particular, using the Seiberg–Witten invariants of smooth four-manifolds, he proved that if a real two-surface smoothly embedded into $\mathbb{CP}^2$ is not homologically trivial and its envelope of holomorphy is also not trivial (i. e., there exists a nonconstant function holomorphic in a neighbourhood of the surface), then the genus of this surface is at least 3, and this estimate is sharp. This implies that there are no nonconstant holomorphic functions on a homologically nontrivial smoothly embedded two-sphere in $\mathbb{C}P^2$ and, in particular, on a test sphere.

His other result is that if a domain in $\mathbb{C}^2$ is biholomorphically equivalent to the ball, then there is no analytic disc in its complement such that the boundary of the disc is contained in the boundary of the domain. If a similar statement were true not only in $\mathbb{C}^2$ but on algebraic ramified coverings over $\mathbb{C}^2$, it would give a proof of the Jacobian conjecture. For these works Nemirovskii was awarded the European Mathematical Society Prize for young mathematicians (Barcelona, 2000).

Let M be a complex manifold consisting of $\mathbb{C}^2$ and a complex line, obtained from $\mathbb{C}P^2$ by performing a chain of blow-ups and then removing all the spheres (including the line at infinity in $\mathbb{C}P^2$) except the last one. We assume that the tree of blow-ups is minimal, that is to say no blow-up can be omitted from the construction of M. How are the topological properties of the test sphere S of M related to the structure of the chain of blow-ups? In some cases, to answer this question it is useful to apply the following *criterion of triangularity* (1999): a chain of blow-ups defining the manifold M is a composition of triangular chains if and only if the intersection index $S^2 = 1$ and the value of the canonical class $K_M \cdot S = -3$. Let us note that, since a triangular chain defines an automorphism, we may not take such chains into account in invertibility problems.

Suppose that $S^2 = -3$ and $K_M \cdot S = 1$. (Notice that these values are attained by the test sphere of the exotic example discussed above.) It follows from the criterion (although not so easily) that the chain of blow-ups generating the manifold M should consist of a composition of triangular chains and a simple chain of four blow-ups (a chain is called simple if the point blown up at each step belongs only to the sphere obtained at the previous step). This example is interesting because it prompts the simplest partial case of the Jacobian conjecture that still remains unsolved.

Exercise: Is it true that if a pair of polynomials P, Q is such that the functions $P(xy^3+y^2,\frac{1}{y})$ and $Q(xy^3+y^2,\frac{1}{y})$ are also polynomial in x,y, then the Jacobian of P,Q vanishes somewhere in $\mathbb{C}^2$? (The change of variables used here is obtained by passing to a coordinate chart at infinity and then performing a simple chain of four blow-ups. If the number of blow-ups is less than four, the analogous statement is known to be true.)

10. On Migration

The Jacobian conjecture was the subject of my talk at the conference "Manifolds-73" held in Tokyo. It was a major conference where I made several acquaintances that were followed by visits to other countries. Thus, having been introduced to Sh. Kobayashi and H. Hironaka in Tokyo, three years later I visited them in the USA (at Berkeley and Harvard). The conference in Tokyo was especially memorable because it was my first trip abroad. Since then I have been to many countries and lectured at about fifty universities.

In the 1970s we seldom traveled abroad. The reasons are known. Here is a typical example. Our colleague, the mathematician Esenin-Vol'pin, had been put in a mental institution for his too emotional response to political events. A letter signed by over a hundred mathematicians was sent to the authorities, insisting on his release, and he was freed. A few years later he emigrated to the USA, whereas those who signed this letter were not permitted to take trips abroad for a long time.

The participants in these events have handled their destinies in different ways. The signatories may remain proud: our Bright Future has arrived thanks to their activities. Many of them have left the country, but not everyone, of course. Tatiana Velikanova, an excellent teacher of mathematics, who was imprisoned and exiled for intensive dissident activity, is now doing what she likes most of all. She teaches Moscow children and finds in it, in her own words, a great pleasure and satisfaction. On graduation from the university in 1954, she was recommended to take a post-graduate course. She refused, because she wanted to work in schools. However, for various reasons her dream of becoming a school teacher came true only in the late 1980s.

Tatiana Mikhailovna Velikanova
and future mathematicians

Besides limitations on our trips abroad there were other difficulties that interfered with communication, such as the language barrier. Let me give as an example a story about the meeting of Kolmogorov and Shannon in Moscow. Kolmogorov was fluent in French and German, and read English well, though his spoken English was not as good. During their conversation Shannon said with some sympathy that he wished they had understood each other better. Kolmogorov replied that there are five international languages, and he knew three of them, and had his interlocutor also known three languages they would not have had problems in communicating.

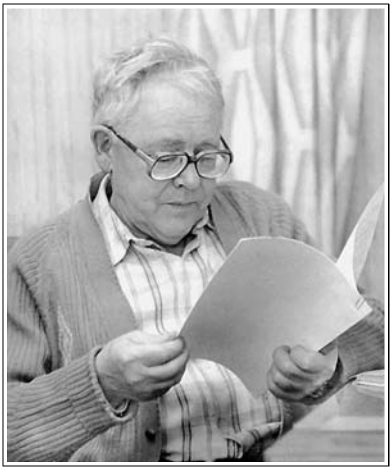
Perestroika has brought a lot of changes: one can go anywhere, those who work in state-controlled institutes earn ridiculously small salaries... If you are clever, why are you poor? Many mathematicians have left for other countries: Manin is in Bonn, Arnold and Henkin in Paris, ..., Nemirovskii is to leave for Bochum. Some of them have left apparently for good, others work half a year here, half a year there. And their seminars now are not the same as they used to be. Before Perestroika, Soviet mathematicians had 30 to 50 out of 150–170 section talks and 2 to 4 plenary talks out of 15–17 at several international congresses. But at the Berlin Congress, Russia was given only one section talk and no plenary talks at all. At this congress the Russians that had left the country received more talks, but much fewer than we had before. They appear to lose shape from hunting for jobs. Not all of them, certainly. Manin is always Manin, and Arnold as well...

Nemirovskii and Domrin in Brussels: Time to go home? (1999)

They say, emigration will save our science. Allegedly, many scientists will come back when the economy improves. Who knows? Maybe they really believe it...

* * *

An acute reader will have certainly perceived that our objective was to discuss three fascinating problems of the twenty-first century, the solutions to which one can now only dream to find...



V. S. Vladimirov

Nikolai Nikolaevich Bogolyubov — Mathematician by the Grace of God

Translated by R. Cooke

Il libro della natura è scritto in lingua mathematica.[1]
Galileo Galilei

When discussing twentieth-century mathematics one cannot avoid mentioning the mathematical legacy of N. N. Bogolyubov, his influence on the development of modern theoretical and mathematical physics, his career, his personality, and his powerful intellect, which bore the stamp of genius. Nikolai Nikolaevich Bogolyubov — mathematician by the grace of God[2] — belongs rightfully among the Pleiad of great Russian scholars whose works enriched the boisterous blossoming of mathematics in the twentieth century. Bogolyubov was the founder of schools in mathematics, theoretical and mathematical physics, and mechanics in Kiev, Moscow, and Dubna. His students in turn have founded schools of their own in Moscow, Kiev, Dubna, Protvino, Novosibirsk, Tbilisi, Kishinev, and Erevan.

[1] The book of nature is written in mathematical language.

[2] The name *Bogolyubov* consists of two Russian words: *Bog* — God, and *lyubov* — love. — *Transl.*

N. N. Bogolyubov

N. N. Bogolyubov was born on 21 August 1909 (8 August on the Julian calendar) in Nizhnii Novgorod. His father, Nikolai Mikhailovich Bogolyubov, was a prominent clergyman, famous for his work on theology, philosophy, and history of religion. The Bogolyubovs soon moved to Kiev. Soon after World War II, N. N. Bogolyubov moved to Moscow. From that time on he was closely connected with the Steklov Mathematical Institute and the Moscow State University.

Year by year we find ourselves more distant from that day (13 February 1992) when Nikolai Nikolaevich Bogolyubov, the great Laborer and Master of science, passed away. But it remains just as difficult to reconcile the fact of his death with the magnanimity that was characteristic of him, with those brilliant and lively impressions that we had of his whole life, which was so rich in ideas and deeds. And it appears that Nikolai Nikolaevich did not leave us forever, that he left us the most valuable and enduring part of himself — ideas, methods, and results — over which death has no dominion.

1. The Kiev Period

Bogolyubov began his career in Kiev under the guidance of N. M. Krylov. At the age of 15 he published his first mathematical work. This early (Kiev) period of his work was devoted to both purely theoretical and applied mathematical problems — direct methods of the calculus of variations, the theory of almost-periodic functions, approximation theory, approximate solution methods for differential equations, dynamical systems, ergodic theory, asymptotic methods, nonlinear mechanics, statistical mechanics, and kinetic equations. He was the first to prove the existence of an invariant measure in dynamical systems; he introduced the important concept of an ergodic set, and established theorems on the decomposition of an invariant measure into "irreducible" invariant measures that are "localized" in ergodic sets. He gave a new construction of the theory of almost-periodic functions and proved the necessary artihmetical theorems. In response to the current needs of technology and construction he wrote important applied papers, many of them jointly with Krylov.

The outstanding research of the young Bogolyubov soon gained him wide recognition. The paper he wrote as a graduate student, "On some new methods in the calculus of variations" ([1]; see also [2]), which was devoted to direct methods of variational calculus for nonregular functionals

$$\int_a^b f(x,y,y')\,\mathrm{d}x,$$

contained a number of original approaches developing the ideas of the Italian mathematician L. Tonelli. This paper was awarded a prize by the Bologna Academy of Sciences in 1930; it was presented by Tonelli, and the author was awarded the degree of doctor of mathematics *honoris causa*.

2. Nonlinear Mechanics

From 1932 on, while developing the research of their famous predecessors Rayleigh, Poincaré, Lyapunov, and Van der Pol, Bogolyubov jointly with Krylov created a completely new branch of mathematical physics — the theory of nonlinear oscillations, which they called *nonlinear mechanics*. In nonlinear mechanics we should mention first the fundamental areas that they created and developed: asymptotic methods, the method of integral manifolds, and the method of averaging. [3]

Asymptotic methods of integrating nonlinear differential equations containing a "small" or "large" parameter. The idea of the method will become clear from the example of the equation

$$\frac{\mathrm{d}^2x}{\mathrm{d}t^2}+\omega^2x=\varepsilon f\left(x,\frac{\mathrm{d}x}{\mathrm{d}t},\varepsilon\right), \tag{1}$$

which describes the oscillations in a system having one degree of freedom. When $\varepsilon = 0$, the oscillations will obviously be harmonic:

$$x_0(t)=a\cos\theta, \quad \text{where} \quad \frac{\mathrm{d}a}{\mathrm{d}t}=0, \quad \frac{\mathrm{d}\theta}{\mathrm{d}t}=\omega. \tag{2}$$

In the case $\varepsilon \neq 0$ the solution of Eq. (1) is sought in the form of a power series in the small parameter ε,

$$x(t,\varepsilon)=a\cos\theta+\varepsilon u_1(a,\theta)+\varepsilon^2u_2(a,\theta)+\cdots, \tag{3}$$

[3] The principal papers from this period, selected by Nikolai Nikolaevich himself, have been translated into English and formed part of Volume III of the prestigious edition of his selected works [3]; see also Volume I of [4].

where $u_1(a,\theta)$, $u_2(a,\theta)$, $\ldots$ are periodic functions of θ, and a and θ are determined from the equations

$$\frac{\mathrm{d}a}{\mathrm{d}t} = \varepsilon A_1(a) + \varepsilon^2 A_2(a) + \cdots,$$
$$\frac{\mathrm{d}\theta}{\mathrm{d}t} = \omega + \varepsilon B_1(a) + \varepsilon^2 B_2(a) + \cdots.$$

This method admits different versions and can be extended to more general systems [5]. Nowadays it is known as the *Krylov–Bogolyubov method*. Yu. A. Mitropol'skii made a substantial contribution to its development.

The method of integral manifolds. The idea of this method is due to Bogolyubov (1945). Suppose that for an n-dimensional system of differential equations

$$\frac{\mathrm{d}x}{\mathrm{d}t} = X(t,x), \quad x = (x_1,x_2,\ldots,x_n), \quad X = (X_1,X_2,\ldots,X_n), \tag{4}$$

there exists, for each $t \in \mathbb{R}$, some manifold (s-dimensional hypersurface)

$$S_t = \big[x : x = f(t,c_1,c_2,\ldots,c_s)\big],$$

where the function $f(t,c_1,c_2,\ldots,c_s)$ satisfies a Lipschitz condition with respect to variables $(c_1,c_2,\ldots,c_s)$, $s \leqslant n$. The surface S_t is an *s-dimensional integral manifold* for Eq. (4) if for every solution $x = x(t)$ of the equation the validity of the relation

$$x(t_0) \in S_{t_0}$$

for some t_0 implies its validity for all t.

In problems of nonlinear mechanics one is interested not only in finding stable integral manifolds, but also in studying integral curves on those manifolds. The method proposed by Bogolyubov for studying one-frequency oscillations is a brilliant example of a way to study the behavior of solutions on an integral manifold, and it finds application in constructing the solutions of a large variety of oscillatory systems with many degrees of freedom, and also in providing rigorous foundations for them.

The method of averaging. The essence of the method of averaging proposed and developed by Bogolyubov is as follows. Consider a system of differential equations in standard form

$$\frac{\mathrm{d}x}{\mathrm{d}t} = \varepsilon X(t,x,\varepsilon) \tag{5}$$

and the corresponding averaged system

$$\frac{d\bar{x}}{dt} = \varepsilon \bar{X}(\bar{x}). \tag{6}$$

Here ε is a small parameter, and

$$\bar{X}(\bar{x}) = \lim_{T \to \infty} \frac{1}{T} \int_0^T X(t, \bar{x}, 0)\, dt. \tag{7}$$

Under rather general hypotheses concerning the right-hand side of (5), one can establish an estimate for the norm

$$\|x(\cdot, \varepsilon) - \bar{x}(\cdot, \varepsilon)\|$$

on a time interval of length L/ε, where $L \to \infty$ as $\varepsilon \to 0$.

In later papers on the method of averaging, Bogolyubov presented a solution of (5) under the assumption (7) in the form of a power series in ε,

$$x = \xi + \varepsilon x_1(t, \xi) + \varepsilon^2 x_2(t, \xi) + \cdots, \tag{8}$$

in which the variable ξ is determined from the equation

$$\frac{d\xi}{dt} = \varepsilon P_1(\xi) + \varepsilon^2 P_2(\xi) + \cdots \equiv \varepsilon P(\xi, \varepsilon). \tag{9}$$

The question of reducing (5) to the "integrable" system (9) is connected with the convergence of the series (8). However, (8) diverges even for elementary systems due to the appearance of "small denominators." To overcome this difficulty, Bogolyubov, after familiarizing himself with the papers of Kolmogorov and Arnold on accelerated convergence, combined their ideas with the method of integral manifolds and proposed a new method in nonlinear mechanics — his own method of accelerated convergence.

By using these results, Bogolyubov succeeded in significantly extending the range of application of asymptotic methods of nonlinear mechanics and at the same time solving the problem of the existence of quasi-periodic solutions for the case $n > 2$. He expounded these results in his 1963 lectures at the Summer Mathematical School in Kanev [6].

Bogolyubov's groundbreaking ideas and fundamental results in nonlinear mechanics form the foundation of many modern investigations in general mechanics, continuum mechanics, celestial mechanics, solid state mechanics and gyroscopic systems, stability of motion, control theory, regulation and stabilization, space flight mechanics, mathematical ecology, and other branches of science and technology. A detailed discussion of Bogolyubov's papers in nonlinear mechanics can be found in the survey of Mitropol'skii [7].

Asymptotic methods and the theory of dynamical systems were further developed in the work of many mathematicians, both here in Russia and abroad (A. N. Kolmogorov, S. Lefschetz, L. S. Pontryagin, A. N. Tikhonov, Yu. A. Mitropol'skii, V. I. Arnold, J. Moser, D. V. Anosov, V. V. Kozlov, and others).

3. Bogolyubov and Mathematical Physics

Some remarkable papers of Bogolyubov on modern mathematics appeared in the postwar period, in connection with the creation of the mathematical machinery for solving new problems of theoretical physics and atomic energy. Bogolyubov never proved theorems simply for their own sake. Every theorem that he proved had some purpose and immediately "went to work." It is an impressive fact that, not having any systematic mathematical education, Nikolai Nikolaevich could nevertheless quickly grasp the mathematical essence of a physical problem, thanks to his talent and intuition, rediscovering where necessary the facts and formulas that he did not know, introducing new and surprising ideas, creating a powerful new method with a large supply of "robustness," leaving the further generalizations and perfecting to be done by his students ... We, his students, observed such a surge of creativity many times and were enchanted by the work of the Master. We saw in practice how physics affects mathematics. On the other hand, he did not regard mathematics as a mere tool of computation, but rather as a method of obtaining new knowledge — as a way of deriving new regularities[4] from obvious propositions (axioms) "at the tip of a pen," as they say, using mathematics.

Bogolyubov's contribution to physics was not limited to just theorems that rigorously confirmed results that were already basically known or understood by physicists (see [9]). He obtained a number of new outstanding results in fundamental physics, such as the theory of a nonideal Bose gas, a chain of kinetic equations, a new method in the theory of superconductivity, and others.

[4] In essence, this was the difference between the schools of Bogolyubov and L. D. Landau in the 1940s and 1950s. For more details see, for example, [8]. Time has adjudicated this dispute — theoretical physicists have been using the most abstract branches of mathematics, from the Turing machine to p-adic numbers, for a long time now.

Here is what Nikolai Nikolaevich's students said of him on the occasion of his eightieth birthday [10]: "The principal feature of Bogolyubov's style lies in knowing how to estimate the key character of a problem and then, undeterred by difficulties, creating an adequate method of solving the problem (this is a manifestation of Hilbert's 'Wir müssen wissen, wir werden wissen'),[5] in which the influence of mathematics and physics forces everyone who studies the works of Bogolyubov to recall the times when the representatives of the exact sciences were simply called natural philosophers."

Oral tradition has even preserved a question posed by the "father of cybernetics" Norbert Wiener: "Is it possible that there are several Bogolyubovs, each the greatest specialist in his field?"

The organic fusion of mathematics and physics in the work of Bogolyubov enabled him to make a decisive contribution to the development of theoretical physics, and in fact to lay the foundations of modern mathematical physics, continuing the tradition of Hilbert, Poincaré, and Einstein.

By 1963 Bogolyubov had solid grounds for proclaiming: "The fundamental concepts and methods of quantum field theory are becoming ever more mathematical" [11]. One can now say more: "Theoretical physics is more and more becoming mathematical physics." A genuine need for a new journal in theoretical and modern mathematical physics[6] had arisen, and such a journal was founded in 1969 on Bogolyubov's initiative. That journal is *Teoreticheskaya i Matematicheskaya Fizika*, and is now known worldwide. A need soon arose for regular international conferences on current problems of theoretical and mathematical physics,[7] and the first such conference was held on Bogolyubov's initiative in Moscow in December 1972. Later conferences were held in Warsaw (1974), Kyoto (1975), Rome (1977), Lausanne (1979), West Berlin (1981), Boulder (1983), Marseille (1986), Swansea (1988), Leipzig (1991), Paris (1994), Brisbane (1997), London (2000), and Lisbon (2003). These last two were called the Thirteenth and Fourteenth International Congress on Mathematical Physics (ICMP 2000 and ICMP 2003 respectively).

[5] We must know; we will know.

[6] There had been cases in which the *Journal of Experimental and Theoretical Physics* rejected papers by members of Bogolyubov's school on the grounds that they were too "mathematical," and contained such "controversial" words as "theorem," "proof," "necessary and sufficient," and others.

[7] For example, at the Rochester conference on high-energy physics (held in Chicago in September 1972) some papers on mathematical physics were not accepted on the grounds that they were too "mathematized."

In accordance with the original principles of the Bogolyubov school, both the journal and the congress now bear the generally recognized emblem

$$\mathrm{M} \cap \Phi,$$

which symbolizes what is in common between mathematicians and physicists and between mathematics and physics.

In an invited address at the opening of the international conference on problems of quantum field theory (Alushta, May 1981), Bogolyubov gave the following assessment of the situation in modern mathematical physics [12]:

> A completely new branch of science has taken shape before our very eyes in recent years, a branch that should properly be called modern mathematical physics.
>
> It has the same genetic origins as classical mathematical physics. But whereas the theory of partial differential equations was generated by problems of classical physics (potential theory, theory of propagation of electromagnetic waves and such), it turns out that modern theoretical physics — quantum field theory with an infinite number of degrees of freedom — requires different, more abstract and modern mathematical methods. These methods consist primarily of the theory of distributions, functional analysis and operator theory, the representation theory of groups and algebras, topological algebra, and the like.
>
> The solution of the new physical problems of quantum field theory was first sought through perfecting the usual methods of quantum mechanics. At that time physicists managed to realize that in order to obtain reasonable answers to their questions they needed a deeper understanding of the mathematical nature of the objects they were studying, such as distributions or unbounded operators, and they needed to raise the standard of proof in their arguments.
>
> Subsequently, to liberate themselves from excessive and sometimes meaningless details, they began to seek out axiomatic routes for constructing theories. It then became obvious that modern mathematical methods sometimes make it possible to obtain very strong results. In this connection we might mention the theory of functions of several complex variables or the concept of weak equivalence of representations.
>
> We note, finally, that several specific quantum phenomena provide a direct physical illustration of von Neumann's famous theorem

on the existence of inequivalent representations in the case of an infinite number of degrees of freedom.

The examples just mentioned come from quantum electrodynamics, the theory of strong interactions at high energies, and problems of statistical physics. In particular, in the physics of strong interactions, due to the complexity of the dynamical picture, dispersion methods based on the general analytic properties of the amplitude of the process turned out to be especially useful. They now have immediate applications to the needs of experimental research.

We are at the very beginning of the route. It suffices to recall that as not a single nontrivial example of quantum field theory has so far been constructed outside of perturbation theory that is in any way close to the real physical world of four dimensions.

The attention of physicists to the methods of modern mathematics and the interest of mathematicians in the problems of quantum physics are mutually productive. [8]

4. Statistical Physics

The methods that Bogolyubov developed for studying dynamical systems and nonlinear mechanics enabled him to approach the problems of statistical physics (and mechanics) of systems consisting of a large (or infinite) number of particles in a fundamentally new way. He was the first to introduce the concept of a hierarchy of times in nonequilibrium statistical physics, which turned out to be decisive in the subsequent development of the statistical theory of irreversible processes.

As early as in his 1945 paper [13], we find the first applications of asymptotic methods to problems of statistical mechanics; in particular, he considered problems involving the effect of a random force on a harmonic oscillator and the establishment of statistical equilibrium in a system connected to a thermostat.

Bogolyubov's greatest contribution to the statistical mechanics of nonideal classical systems is his well-known monograph [14], in which he developed the method of *Bogolyubov chains of equations* for equilibrium and nonequilibrium

[8] It is not coincidental that Bogolyubov actively opposed the innovations in the teaching of mathematics at the high-school level that were based on intensive use of set theory and mathematical logic, which had the effect that neither the teachers nor the pupils nor the pupils' parents could make any sense out of the proposed textbooks [2].

multi-particle distribution functions

$$F(t) = \big(F_s(t,(x)_s)\big), \quad s = 1,2,\ldots, \quad (x)_s = (x_1,x_2,\ldots,x_s),$$

where $x_i = (p_i,q_i)$ is a point of the phase space $\mathbb{R}^6$. The state function $F(t)$ is defined as the solution of the Cauchy problem for the Bogolyubov chain of equations

$$\frac{\partial F_s(t,(x)_s)}{\partial t} = \big\{H_s, F_s(t,(x)_s)\big\} + \int \Big\{ \sum_{i=1}^{s} |\Phi(q_i - q_{s+1})|, F_{s+1}(t,(x)_s,x_{s+1}) \Big\}\, \mathrm{d}x_{s+1}, \qquad s = 1,2,\ldots, \quad (10)$$

with the initial conditions

$$F_s(t,(x)_s)\big|_{t=0} = F_s(0,(x)_s), \quad s = 1,2,\ldots \qquad (11)$$

Here Φ is the interaction potential,

$$H_s = \sum_{i=1}^{s} \frac{p_i^2}{2m} + \sum_{i<j} \Phi(q_i - q_j)$$

is the Hamiltonian of an s-particle subsystem, and $\{a,b\}$ is the Poisson bracket.

In studying the Cauchy problem (10)–(11), Bogolyubov made extensive use of the methods of nonlinear mechanics he had developed previously and the fundamental physical concept he had established — the existence of different time scales. In 1948 he was awarded a Stalin Prize for the paper [14].

In his lectures on quantum statistics [15] Bogolyubov extended these results to systems of quantum statistical mechanics.

Bogolyubov's address at the 1946 meeting of the Physics and Mathematics Division of the USSR Academy of Sciences and his subsequent 1947 paper [16] were devoted to the construction of the microscopic theory of superfluidity. In these papers he applied the canonical transformation now widely known as the *Bogolyubov transformation* to diagonalize the quantum part of the Hamiltonian. For Bose systems this transformation has the form

$$\begin{aligned} b_k &= u_k \xi_k - v_k \xi_{-k}^{+}, \\ b_k^{+} &= u_k \xi_k^{+} + v_k \xi_{-k}, \end{aligned} \qquad (12)$$

where b_k^+ and b_k are the creation and annihilation operators of the particles, ξ_k^+ and ξ_k are the creation and annihilation operators of new quasi-particles, and u_k and v_k are real-valued functions of the momentum k connected by the relation $u_k^2 - v_k^2 = 1$.

In 1957, using a canonical transformation of the type (12) for Fermi systems, Bogolyubov independently of Bardeen, Cooper, and Schrieffer constructed a microscopic theory of superconductivity [17]. For this outstanding work and works on quantum field theory he was awarded the Lenin Prize in 1958.

The development of the concept of superconductivity as superfluidity of Fermi systems led Bogolyubov in 1958 to the discovery of a new fundamental effect — superfluidity of nuclear matter. This discovery became the foundation of the modern theory of the nucleus.

It is difficult to enumerate all of Bogolyubov's outstanding achievements in statistical physics. We note in addition the widely known *method of Bogolyubov quasi-means* [18]. The main achievement of this method is the fundamental *Bogolyubov theorem on the singularity of* $1/q^2$ [18] for Bose and Fermi systems with gauge-invariant interaction between particles. The gist of this theorem is that the Fourier components of the Green's functions corresponding to energy $E=0$ satisfy the inequality

$$\left|\left\langle\langle a_q, a_q^+\rangle\right\rangle_{E=0}\right| \geqslant \frac{\text{const}}{q^2} \quad \text{as } q^2 \to 0.$$

We thus see that Bogolyubov made a number of fundamental discoveries in statistical physics as well.

These directions in statistical physics were further developed by Bogolyubov's students: S. V. Tyablikov, D. N. Zubarev, Yu. A. Tserkovnikov, N. N. Bogolyubov junior, E. E. Tareeva, D. Ya. Petrina, N. M. Plakida, V. A. Moskalenko, and others.

5. The Bogolyubov Axioms for Quantum Field Theory

A brilliant example of the creation and application of new mathematical methods in physics was the development of an axiomatic approach to quantum field theory, undertaken by Bogolyubov in the 1950s. He always strove to work on the latest and hottest topics of theoretical physics. At that time, the ultraviolet divergence was an important problem in quantum field theory when using the Hamiltonian formalism.

Bogolyubov proposed a new approach to this problem. First of all, he abandoned the Hamiltonian formalism and took as a basis of the theory the scattering matrix S introduced by Heisenberg. For the Lagrangian of the interaction

$$\mathscr{L}(x) \equiv \mathscr{L}\left(u_\alpha, \frac{\partial u_\alpha}{\partial x_\beta}\right)$$

he regarded the S-matrix as an operator-valued functional

$$S[g] = I + \sum_{n=1}^{\infty} \frac{1}{n!} \int S_n(x_1, \ldots, x_n) g(x_1) \cdots g(x_n) \, \mathrm{d}x_1 \cdots \mathrm{d}x_n \tag{13}$$

on "good" functions $g(x)$ (the "inclusions" of the interaction), where

$$\begin{aligned} S[g] &\to I \quad \text{as } g \to 0 \quad \text{(a free field)}, \\ S[g] &\to S \quad \text{as } g \to 1 \quad \text{(a physical } S\text{-matrix)}. \end{aligned}$$

In the representation (13) the functions $S_n(x_1, \ldots, x_n)$, $n = 1, 2, \ldots$, are operator-valued tempered (slowly increasing) distributions (in the sense of Sobolev–Schwartz) depending on the fields u_α and their derivatives $\partial u_\alpha / \partial x_\beta$ at the points $(x_1, \ldots, x_n)$. It is required that the S-matrix satisfies the following fundamental physical postulates (axioms) [19], [20]: *it must be relativistically covariant, unitary, causal* (*local*)*, and spectral.*

The greatest difficulty arises in formulating the condition of causality. The problem is that in the theory of the S-matrix the local operators are initially absent, and for that reason the formulation of the space-time relations — in particular, the requirement of causality — is not obvious. So Bogolyubov introduced local Heisenberg operators as the variational derivatives of the S-matrix with respect to the interaction inclusion functions $g(x)$:

$$\mathrm{i} \frac{\delta S[g]}{\delta g(x)} S^+[g], \tag{14}$$

and stated the condition of causality, nowadays well-known as *Bogolyubov's microcausality condition*, in the form

$$\frac{\delta}{\delta g(x)} \left\{ \frac{\delta S[g]}{\delta g(y)} S^+[g] \right\} = 0 \quad \text{for } y - x \in V^+, \tag{15}$$

where V^+ is the future light cone.

It follows from the above axioms, that S_n are symmetric in all arguments and commute when all the x_i are space-like to all y_j,

$$\left[S_n(x_1, \ldots, x_n), S_m(y_1, \ldots, y_m) \right] \quad \text{for} \quad (x_i - y_j)^2 < 0, \quad \forall i, j, \tag{16}$$

where $[a, b] = ab - ba$ is the commutator of the operators a and b.

In the proposed approach one can represent the physical scattering matrix S in terms of the T-exponent

$$S[u] = T \left(\exp \mathrm{i} \int \mathscr{L}(x) \, \mathrm{d}x \right),$$

and the Bogolyubov microcausality condition can be rewritten in a manner similar to (15) as

$$\frac{\delta}{\delta u_\alpha(x)}\left\{\frac{\delta S[u]}{\delta u_\beta(y)}S^+[u]\right\}=0 \quad \text{for } y-x\in V^+.$$

The axiom system proposed by Bogolyubov was the first attempt at a nontrivial application of the axiomatic method in physics. [9]

In mathematics (especially in geometry and set theory) the axiomatic method had long been known. Like every axiom system, Bogolyubov's axioms had to satisfy the requirements of consistency, independence, realizability, and completeness.

To this day, the deep question remains whether there exists a nontrivial (non-identity) S-matrix in the context of these axioms in 4-dimensional spacetime and outside the framework of perturbation theory. [10] In any case, under the assumption that the coefficient functions S_n in the S-matrix are ordinary locally integrable functions, this system leads only to the trivial S-matrix. Extension of the class of objects to distributions, on which the axiom system can be realized — the decisive qualitative requirement at the time — was far out of the ordinary for physicists. It led to a change in the very style of physical thought and an elevation in the requirements on the demonstrative power of arguments.

What has been said cries out for the conclusion that Bogolyubov actually made the first steps toward the solution of the sixth problem on Hilbert's 1900 list: "Axiomatize those physical sciences in which mathematics plays an important role" [21]. It should be noted that somewhat later other axiom systems for quantum field theory were constructed, connected with the names of A. Wightman, H. Lehmann, K. Symanzik, W. Zimmermann, R. Haag, H. Araki, D. Kastler, and others.

6. Bogolyubov's "Edge of the Wedge" Theorem

In proving the dispersion relations in the context of axiomatic quantum field theory, Bogolyubov encountered a number of new purely mathematical problems lying on the cusp between the theory of functions of several complex variables and the theory of distributions — questions of the theory of analytic continuation

[9] Here we have in mind systems of axioms that cannot be reduced to boundary-value problems for differential equations, as happens, for example, in the case of electrodynamics (the Maxwell equations).

[10] For the 2- and 3-dimensional theories the existence of such an S-matrix has been proved.

of distributions. The first thing he discovered and proved was the very important theorem now known as

Bogolyubov's "edge of the wedge" theorem. *Suppose two functions f_+ and f_- of n complex variables $z = x + \mathrm{i}y = (z_0, z_1, \dots, z_{n-1})$ are the Laplace transforms of tempered distributions with supports in the closed future light cone $\bar{V}^+$ and the past light cone $\bar{V}^-$ (where $V^- = -V^+$) respectively (and therefore holomorphic in the future tube $T^+ = \mathbb{R}^n + \mathrm{i}V^+$ and in the past tube $T^- = \mathbb{R}^n + \mathrm{i}V^-$ respectively), and that their boundary values*

$$f^{\pm}(x \pm \mathrm{i}0) \quad \text{as} \quad y \to 0,\ y \in V^{\pm},$$

which exist as distributions, coincide in some domain $D \subset \mathbb{R}^n$:

$$f^+(x + \mathrm{i}0) = f^-(x - \mathrm{i}0), \quad x \in D. \tag{17}$$

Then there exists a function $f(z)$ holomorphic in the domain

$$T^+ \cup T^- \cup \widetilde{D} \tag{18}$$

that coinsides with $f^{\pm}$ in the domains $T^{\pm}$ respectively, where

$$\widetilde{D} = \bigcup_{x \in D} \big[z \in \mathbb{C}^n : |z - x| < \theta \Delta_D(x) \big]$$

is a complex neighborhood of D, the constant θ $(0 < \theta < 1)$ is independent of $f^{\pm}$, and $\Delta_D(x)$ is the distance from a point x of D to the boundary of D.

It was at first difficult to verify that this theorem is true for all $n \geqslant 2$. After all, the "wedge-shaped" $2n$-dimensional domains T^+ and T^- meet each other only along the n-dimensional "edge" $\mathbb{R}^n$, and the agreement of the boundary values (17) on a part of the "edge" D seemed to be an insufficient condition for the function $f(z)$ to be holomorphic in an entire $2n$-dimensional neighborhood $\widetilde{D}$ of this "wedge" when $n \geqslant 2$. But Nikolai Nikolaevich's powerful intuition overcame this doubt, and he constructed the first proof of this theorem in 1956 (see [20]). It was communicated at an international conference in Seattle that year.

Here is how Abdus Salam remembers Bogolyubov's presentation [22]: "I first saw Professor N. N. Bogolyubov at the Seattle Conference where he gave lectures on complex variables in more than one dimension and displayed the proofs of the dispersion relations... It was quite clear that the man was fully in charge of the complex variable theory at that time."

Unfortunately, Nikolai Nikolaevich did not take the trouble to publish his proof of the "edge of the wedge" theorem immediately in English; this omission gave rise to the ambiguous priority assessments connected with this theorem (see, for example, [23] and [24]).

Here is what the authors of the famous book *Introduction to Quantum Field Theory* — Bogolyubov and Shirkov — have to say on this score (second edition, Nauka, 1973, p. 391):

> This technique was first developed by Bogolyubov in the mid-1950s. The most general and classical result in this area is the theorem that it is possible to define the advanced and retarded functions as a single analytic function (see the 1958 monograph of Bogolyubov, Medvedev, and Polivanov, Appendix A, Theorem 1). This theorem later came to be known as the "edge of the wedge" theorem. One can prove the dispersion relations for different cases with reasoning on the basis of this theorem ...
>
> These methods received further development in papers of Bogolyubov and Vladimirov (1958), Bremermann, Oehme and Taylor (1958), Lehmann (1959), Vladimirov and Logunov (1959), Oehme and Taylor (1959), Todorov (1960), and others.

The next stage in this topic was the construction of the envelope of holomorphy (or lower bounds for them) in $\mathbb{C}^n$ for domains of the form (18),

$$H(T^+ \cup T^- \cup \widetilde{D}), \tag{19}$$

and the corresponding integral representations. This construction forms the content of Bogolyubov's global "edge of the wedge" theorem. This research was developed by Bogolyubov and his students. It was remarked that under the hypotheses of the "edge of the wedge" theorem the envelope of holomorphy (19) contains the convex hull $V(D)$ ($V = V^+ \cup V^-$) of the domain D relative to time-like curves — the *"V-convex hull" theorem* (Vladimirov, 1961; see [25]).

Bogolyubov's "edge of the wedge" theorem and its corollaries have now become an established part of mathematics; they have profound generalizations and many applications, and comprise a new chapter in the theory of functions of several complex variables [25]–[27]. The significance of the theorem goes far beyond the requirements of physics. This is a salient example of the influence of physics on mathematics!

7. Dispersion Relations and Related Questions

The Bogolyubov axioms, the "edge of the wedge" theorem, and the "V-convex hull" theorem have many corollaries (theorems) that contain new physical knowledge hidden in the axioms. This applies primarily to the dispersion relations in quantum field theory. From the mathematical point of view the dispersion relations are simply Cauchy's formula connecting (on the real axis) the real and imaginary parts of a function $T(s)$ that is holomorphic in the plane of the complex variable s cut along $[0,\infty)$. The simplest formula (without subtractions) has the form

$$\Re T(s) = \frac{1}{\pi}\mathscr{P}\int_0^\infty \frac{\Im T(s')}{s'-s}\,\mathrm{d}s'. \tag{20}$$

For physical (scattering) amplitudes a relation of the type (20) can be verified experimentally.

The matrix elements, for which the dispersion relations are derived, reduce to the vacuum means of the *radiation operators*

$$H(x_1,\dots,x_n) = \frac{\delta^n S[u]}{\delta u_1(x_1)\cdots\delta u_n(x_n)} S^+[u]. \tag{21}$$

A very profound, virtuoso proof of the dispersion relations for the scattering of π-mesons at nucleons ($p+k=p'+k'$) on the energy square $s=(p+k)^2$ with a fixed momentum transfer $-t=-(p-p')^2$ (here s and t are the Mandel'shtam variables) was constructed and communicated (to great acclaim) by Bogolyubov at the International Conference in Seattle in 1956. Plenary addresses were devoted to these same questions at the International Congress of Mathematicians in Edinburgh in 1958 [28] and at the Rochester Conference on high-energy physics in Kiev in 1959 [29].

At the time this area — the method of dispersion relations — was undergoing a period of vigorous development and for that reason a rigorous proof of the relations elicited great interest. The proof of the formula (20) is reduced to the analytic properties of the scattering amplitude

$$T_{p,p'}(q) = \int \theta(x_0) g_{p,p'}(x)\exp\mathrm{i}(qx)\,\mathrm{d}x, \quad q=\frac{k+k'}{2}=(q_0,\vec{q}), \tag{22}$$

where the commutator

$$g_{p,p'}(x) = \left\langle p'\middle|\left[A\left(\frac{x}{2}\right),B\left(-\frac{x}{2}\right)\right]\middle|p\right\rangle \tag{23}$$

vanishes for $x^2 < 0$ and its Fourier transform $\tilde{g}_{p,p'}(q) = 0$ in the domain

$$D = \left[q \in \mathbb{R}^4 : a - \sqrt{|\vec{q}|^2 + b^2} < q_0 < -a + \sqrt{|\vec{q}|^2 + c^2}\right].$$

Here a, b, and c ($a \geqslant 0$, $0 \leqslant b \leqslant c$) are certain numbers determined by the masses of the interacting particles. In (23), A and B are field operators and $\langle p'|$ and $|p\rangle$ are single-particle states with momenta p' and p respectively.

To prove dispersion relations of the type (20) using Bogolyubov's "edge of the wedge" theorem, it remained only to construct the envelope of holomorphy (19) of the domain (18). Bogolyubov did so without using the famous Jost–Lehmann–Dyson representation, which was not discovered until 1957–1958 (see [25]).

The dispersion relation approach to quantum field theory opened a new phase in the theory of strong interactions. It was not merely a matter of constructing a systematic mathematical machinery unconnected with the assumption that the interaction of the elementary particles is weak. Rather, the circle of ideas introduced into physics in the proof of the dispersion relations became the basis of a new language of strong interactions. Physicists got a picture of the scattering amplitude as a unified analytic function of the momentum variables, and this became decisive for the later development of the theory of strong interactions. This result, which seems purely mathematical at first glance, reflected the deep connections that exist between seemingly different physical processes. It became clear that even if one could not find the amplitude of a given process, one could find its connection with the amplitudes of other processes. The idea of a connection between different reaction channels became the point of departure for numerous heuristic considerations on the structure of the scattering amplitude. The subsequent development of these ideas, together with duality considerations, led to the creation of the modern theory of strings and superstrings (I. Ya. Aref'eva, I. V. Volovich).

Many other applications of the axiomatic method in quantum field theory, both inside and outside the context of perturbation theory, were developed in the papers of Bogolyubov and his students. Bogolyubov proved that the scattering matrix can be determined sequentially from the axioms (up to quasi-local singular operators) in all orders of perturbation theory. This analysis of the singularities led to the construction of a sequential recipe for removing the ultraviolet divergences in the S-matrix, which came to be known as the *Bogolyubov–Parasyuk R-operation* [19]. Later this approach was developed in works of O. I. Zav'yalov, K. Hepp, W. Zimmermann, K. Nakanishi, and others.

Another important achievement of Bogolyubov and his students Shirkov and Logunov in perturbation theory is the development of the *method of renormalization groups* [19].

Asymptotic estimates at high energies were established, a description of the low-energy regions was given by invoking the unitary condition, and the problem of scale invariance was studied. Study of the self-similar behavior of the form-factors of deeply nonelastic hadron-nucleon scattering processes at high energies and large momentum transfer (and the connection with the behavior of their Fourier images in a neighborhood of the light cone) [30] were intended to prove that such experimentally observable behavior does not contradict the Bogolyubov axioms for quantum field theory. This research laid the foundation for a new developing branch of modern mathematics – Tauberian theory for distributions of several variables [31].

Bogolyubov and Vladimirov established the *"finite covariance" theorem* ([32], [33]), which asserts that translation invariance, spectrality (at the Jost points), and causality imply finite relativistic covariance. This result, which indicates a partial interdependence of the Bogolyubov axioms, was obtained under the additional assumption that the N-point extended tube τ_N is a domain of holomorphy in $\mathbb{C}^{4N}$. (For $N = 1$ and $N = 2$ this was well known; for $N \geqslant 3$ this difficult problem was solved only in 1997 by the Chinese mathematician Zhou – who received his doctorate from the Mathematical Institute of the Russian Academy of Sciences under the direction of A. G. Sergeev, see [34].)

D. Ya. Petrina (1961), using the "V-convex hull" theorem, established that it is impossible to construct nonlocal field theories with an energy-momentum operator having a positive spectrum and elementary length $l > 0$ under the assumption of translation invariance (see [25]).

These examples show how mathematics helps physics to obtain new knowledge which is hidden in the axioms!

8. Bogolyubov in Arzamas-16

The picture of Bogolyubov's mathematical activity would be far from complete if we omitted his work and life in Arzamas-16 (now known as the All-Russian Nuclear Center and All-Russian Scientific Research Institute of Experimental Physics). Early in 1950, by decree of the Soviet government, Bogolyubov was drafted to work on a supersecret project in *Privolzskaya kontora Glavgorstroya SSSR*, KB-11 (Design Bureau No. 11) [11] to provide the mathematical support for a group of theoretical physicists headed by I. E. Tamm and A. D. Sakharov.

[11] Located in Sarov monastery – one of the holiest places of the Russian Orthodox Church. The Sarov monastery was known for its miraculous springs, where Saint Serafim of Sarov spent his life in prayers, silence, and asceticism.

At the time they were working on the first version of a hydrogen bomb, the so-called "puff" bomb of Sakharov (RDS-6). [12]

In 1950 Bogolyubov organized a mathematical division in KB-11, whose core originally consisted of D. N. Zubarev, V. N. Klimov, D. V. Shirkov, V. S. Vladimirov, E. V. Malinovskaya, and Yu. A. Tserkovnikov. Most of the staff in the division, about 50 people, were young women calculators, brought in to work on the project mainly from institutions of geodesy. They used electro-mechanical calculators of Mercedes type. They were our "live" computer — the basis of the pre-computer period of computations for the project. At the time there were no high-speed computers in our country, and the technical institutes were not producing the necessary computing specialists, technicians, and engineers. Everything had to be done from scratch, in a hurry, under the watchful eye of the security forces. In the beginning of this work, Bogolyubov placed me in charge of this computation group.

The main attention was paid to computing versions of Sakharov's "puff." However, in the process it was necessary to carry out a great deal of routine computational work on almost all versions of new models of atomic weaponry.

Here is where the enormous erudition and talent of Nikolai Nikolaevich were of use! Sensing his intellectual prowess, the research staff — mathematicians and physicists — took every opportunity of discussing their problems with him and making use of his extensive knowledge, wide experience, and advice. [13]

Bogolyubov wrote a number of brilliant papers on peaceful (magnetically confined) thermonuclear synthesis. Together with his colleagues he worked on the problems of stability of a plasma in a magnetic field and the interaction of the plasma with the walls of the container.

He devoted a great deal of attention to the development of approximate methods of solving the problems of nuclear and neutron kinetics on both the theoretical and applied levels. For more precise computations on the "puff," I created, under his direction, a new method for numerical solution of the kinetic equation for multi-layer spherically symmetric systems — the *method of characteristics* (1950–1951), which was adapted for hand computation (see [36]–[40]).

Mathematically, the problem reduces (in the simplest cases) to a boundary-value problem for an integro-differential equation (the linearized Boltzmann

[12] The atomic and hydrogen bombs were given sequential ordinal numbers RDS-1, RDS-2, and so-forth. The abbreviation RDS at the time was decoded as the Russian phrase "Stalin's Jet Engine."

[13] In his memoirs [35] Sakharov, the "father of the Soviet hydrogen bomb," used the epithets "unusually talented" and "passing out ideas right and left" in speaking of Bogolyubov.

equation)

$$\mu\frac{\partial\varphi}{\partial r}+\frac{1-\mu^2}{r}\frac{\partial\varphi}{\partial\mu}+\alpha(r)\varphi=\beta(r)\frac{1}{2}\int_{-1}^{1}\varphi(r,\mu')\,\mathrm{d}\mu'+f(r), \tag{24}$$

$$\varphi(R,\mu)=0, \qquad -1\leqslant\mu\leqslant 0 \tag{25}$$

for the density of neutrons $\varphi(r,\mu)$ in the phase domain of the variables (r,μ) given by $|x|=r<R$, $\mu=\cos\theta$, $|\mu|<1$. The quantity

$$\frac{1}{2}\int_{-1}^{1}\varphi(r,\mu')\,\mathrm{d}\mu'=n(r) \tag{26}$$

in Eq. (24) is the average density of neutrons at the point r.

Two variants of the problem (24)–(25) are distinguished: (i) the problem with a source $f\neq 0$ under the assumption that the system is in a subcritical state (the smallest eigenvalue λ_0 of the homogeneous problem is less than 1), and (ii) the problem of critical parameters and the computation of the corresponding eigenfunction ($\lambda_0=1$).

The idea of the method is that on the characteristic of Eq. (24)

$$y=r\sqrt{1-\mu^2}=\text{const}, \quad |x|<\sqrt{R^2-y^2},$$

the boundary-value problem (24)–(25) reduces to the Cauchy problem

$$\frac{\mathrm{d}\psi}{\mathrm{d}x}+\alpha\big(\sqrt{x^2+y^2}\big)=Q\big(\sqrt{x^2+y^2}\big), \quad Q=\beta n+f, \tag{27}$$

$$\psi\big(-\sqrt{R^2-y^2},y\big)=0, \quad 0\leqslant y\leqslant R \tag{28}$$

for the function

$$\psi(x,y)=\varphi\left(\sqrt{x^2+y^2},\frac{x}{\sqrt{x^2+y^2}}\right)\equiv\varphi(r,\mu).$$

For the approximate solution of the problem (27)– (28) we take the grid (see the figure below)

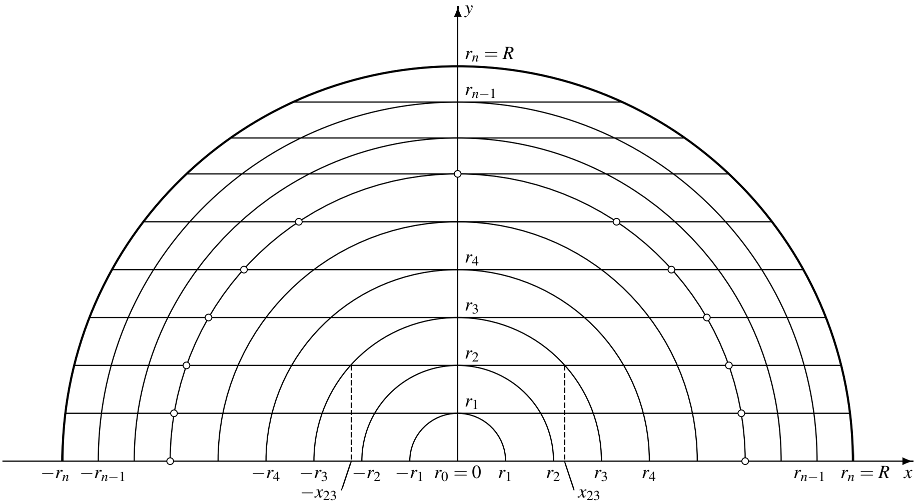
$$\{y_i=r_i,\ i=0,1,\dots,n\}, \quad \text{where } 0=r_0<r_1<\cdots<r_n=R;$$

$$\{\pm x_{ki},\ k=i,i+1,\dots,n\}, \quad \text{where } x_{ki}=\sqrt{r_k^2-r_i^2}.$$

From the values that are found

$$\psi(\pm x_{ki},y_i)=\varphi(r_k,\pm\mu_{ki}), \quad k=0,1,\dots,n, \quad i=0,1,\dots,k,$$

where $\mu_{ki}=x_{ki}/r_k$, one can compute $n(r_k)$ for $k=0,1,\dots,n$, using formula (26).



This method is easy to program on an electronic computer and can be carried over without difficulty to multi-group and multi-dimensional problems of neutron and radiation transport. Nowadays the method of characteristics, being the most stable and accurate, is also used in nuclear power engineering ([36], [37]).

In addition to current physical and computational problems, purely theoretical questions of modern and prospective mathematics and physics were discussed with Nikolai Nikolaevich — the axiomatization of quantum field theory [19], the ideas of cybernetics, distributions, Monte Carlo methods [41], functional integration, quadrature formulas (including functional integrals) [42], the Wiener–Hopf method [43], variational principles for differential and integral equations and kinetic equations [44], factorization methods [45], and many others. The published papers of the Los Alamos physicists and mathematicians were also studied: those of R. E. Marshak, R. Peierls, H. A. Bethe, K. Fuchs, R. P. Feynman, B. Davison, G. Wick, G. Placzek, C. Mark, J. LeCaine, H. Hurwitz, and others.

The successful test of RDS-6 took place on 12 August 1953. Nikolai Nikolaevich was sent to the Kazakhstan steppe to participate personally in the tests. He returned in an elated mood. Bogolyubov's work was highly appreciated by his country. For his active participation in the construction of the first hydrogen bomb he was awarded a Stalin Prize in 1953. In the autumn of that same year he was elected an academician of the USSR Academy of Sciences.

In the preface to the book *In the Intermissions*... [36] Yu. B. Khariton, the head of the project at the time, called Nikolai Nikolaevich one of the "superstars" of Soviet physics and mathematics: "The establishment of the

Arzamas school of theoretical physics and its creative style owe their origins to the ‘superstars’ of Soviet physics and mathematics, such as N. N. Bogolyubov, N. A. Dmitriev, D. A. Frank-Kamenetskii, M. A. Lavrent’ev, L. V. Ovsyannikov, I. Ya. Pomeranchuk, A. D. Sakharov (Nobel Prize winner, 1975), I. E. Tamm (Nobel Prize winner, 1958), V. S. Vladimirov, Ya. B. Zel’dovich.”

Nikolai Nikolaevich worked at the project for three and a half years; at the time he was a little over 40 years old. The last preliminary report, approved by him in October 1953, was on RDS-41, a small size atomic bomb. M. A. Lavrent’ev was in charge of this project.

This was a heroic and quite productive period in the life of Nikolai Nikolaevich; on the one hand, it was life behind barbed wire in holy places with all the confusion and disorder of the time and harsh discipline.[14] On the other hand, it was an enormous responsibility to the country and its people for the cause entrusted to him.

One can now say with satisfaction that the intense labor that Bogolyubov performed together with the whole Soviet people in that period was not in vain: the country obtained a new and awesome weapon; nuclear restraint was established, and a third world war was avoided. This restraint continues to be effective right down to the present.

* * *

Nikolai Nikolaevich examined with great interest and a certain sadness a photograph of Sarov that I brought to him in May 1991, and asked detailed questions about the colleagues with whom he had worked or whom he had encountered there nearly 40 years earlier. As a religious man, he showed special interest in the parts of Sarov monastery that had been preserved, the famous 70-meter bell tower, the cathedrals of the Diveev Monastery, the relics of Serafim of Sarov . . . Unfortunately, he was no longer able to revisit these holy places.

Bibliography

[1] N. N. Bogoliubov. Sur quelques méthodes nouvelles dans le Calcul des Variations. *Ann. Mat. Pura Appl.* (4), 1929–1930, **7–8**, 249–271.

[2] A. N. Bogolyubov. *N. N. Bogolyubov. Life and Work.* Dubna, 1996 (Russian).

[14] I remember a naive comment by a young officer of the secret service, who instructed me before my first trip to Arzamas: “You will live in communism surrounded by socialism.” On such occasions Bogolyubov used to quote M. E. Saltykov-Shchedrin or M. A. Bulgakov. We were impressed by his enormous erudition in history, literature, and linguistics. His wise and humorous remarks brightened up our secluded life in Arzamas.

[3] N. N. Bogolyubov. *Selected Works*. Part I. Dynamical Theory, 1990; Part II. Quantum and Classical Statistical Mechanics, 1991; Part III. Nonlinear Mechanics and Pure Mathematics, 1995; Part IV. Quantum Field Theory, 1995. London: Gordon & Breach.

[4] N. N. Bogolyubov. *Selected Works in Three Volumes*. Kiev: Naukova Dumka, 1969–1971 (Russian).

[5] N. N. Bogolyubov, Yu. A. Mitropol'skii. *Asymptotic Methods in the Theory of Nonlinear Oscillations*, 2nd edition. New York: Gordon & Breach, 1962.

[6] N. N. Bogolyubov, Yu. A. Mitropol'skii, A. M. Samoilenko. *The Method of Accelerated Convergence in Nonlinear Mechanics*. Kiev: Naukova Dumka, 1969 (Russian).

[7] Yu. A. Mitropol'skii. On Bogolyubov's work in the field of nonlinear mechanics. *Proc. Steklov Inst. Math.*, 2000, **228**, 11–17.

[8] D. V. Shirkov. Reminiscences of N. N. In: *Nikolai Nikolaevich Bogolyubov. Mathematician and Physicist*. Dubna, 1994, 180–197 (Russian).

[9] L. D. Faddeev. What is modern mathematical physics. *Proc. Steklov Inst. Math.*, 1999, **226**, 1–4.

[10] V. S. Vladimirov, A. A. Logunov. A brief essay on scientific activity. In: *Nikolai Nikolaevich Bogolyubov*. Dubna, 1989, 7–21 (Russian).

[11] N. N. Bogolyubov, M. K. Polivanov. Fields and quanta. Quantum field theory — the science of elementary particles and their interactions. In: *Through the Eyes of a Scientist*. Moscow: USSR Academy of Sciences, 1963, 158–173 (Russian).

[12] Introductory speech of Academician N. N. Bogolyubov, Chairman of the Organizing Committee. In: *Proceedings of the Sixth International Conference on Problems of Quantum Field Theory* (Alushta, 1981). Dubna, 1981, 5–6 (Russian).

[13] N. N. Bogolyubov. *On Some Statistical Methods in Mathematical Physics*. Kiev: Ukrainian SSR Academy of Sciences, 1945 (Russian).

[14] N. N. Bogolyubov. *Problems of Dynamical Theory in Statistical Physics*. Moscow–Leningrad: GITTL (State Publishing House for Technical and Theoretical Literature), 1946 (Russian).

[15] N. N. Bogolyubov. Lectures on quantum statistics. In: *Selected Works in Three Volumes*, Vol. II. Kiev: Naukova Dumka, 1970, 287–493 (Russian).

[16] N. N. Bogolyubov. On the theory of superfluidity. *Izv. Akad. Nauk SSSR, Ser. Fiz.*, 1948, **11**(1), 77–90 (Russian).

[17] N. N. Bogolyubov. On a new method in the theory of superconductivity. I; III. *Zh. Eksp. Teor. Fiz.*, 1958, **34**(1), 58–65; 73–79 (Russian).

[18] N. N. Bogolyubov. Quasi-means in problems of statistical mechanics. In: *Selected Works in Three Volumes*, Vol. III. Kiev: Naukova Dumka, 1970, 174–243 (Russian).

[19] N. N. Bogolyubov, D. V. Shirkov. *Introduction to Quantum Field Theory*. Moscow: Gostekhizdat, 1957; 2nd, corrected edition, Moscow: Nauka, 1973 (Russian).

[20] N. N. Bogolyubov, B. V. Medvedev, M. K. Polivanov. *Questions of the Theory of Dispersion Relations.* Moscow: GIFML (State Publishing House for Physico-Mathematical Literature), 1958 (Russian); English translation: Laurence Laboratory, 1959.

[21] *The Hilbert Problems*. Moscow: Nauka, 1969 (Russian).

[22] A. Salam. My meetings with N. N. Bogolyubov. In: *Nikolai Nikolaevich Bogolyubov. Mathematician and Physicist*. Dubna, 1994, 110–111.

[23] H.-J. Bremermann, R. Oehme, J. G. Taylor. Proof of dispersion relations in quantum field theories. *Phys. Rev.*, 1959, **109**(6), 2178–2190.

[24] R. Oehme. Dispersion relations in gauge theories with confinement. In: *Quanta, Relativity, Gravitation. Proceedings of the Eighteenth Workshop on High Energy Physics and Field Theory*. Protvino, 1996, 275–282.

[25] V. S. Vladimirov. *Methods in the Theory of Functions of Several Complex Variables*. Moscow: Nauka, 1964 (Russian); English translation: MIT Press, 1966.

[26] V. S. Vladimirov, V. V. Zharinov, A. G. Sergeev. Bogolyubov's "edge of the wedge" theorem, its development and applications. *Russ. Math. Surveys*, 1994, **49**(5), 51–65.

[27] A. A. Gonchar. On Bogolyubov's "edge of the wedge" theorem. *Proc. Steklov Inst. Math.*, 2000, **228**, 18–24.

[28] N. N. Bogolyubov, V. S. Vladimirov. On some mathematical problems of quantum field theory. In: *Proceedings of the International Congress of Mathematicians* (Edinburgh, 1958). Cambridge University Press, 1960, 19–32.

[29] D. V. Shirkov, V. S. Vladimirov, A. A. Logunov. Theoretical studies of dispersion relations. In: *Ninth International Conference on High-Energy Physics* (Kiev, July 1959). Moscow, 1961, 453–464 (Russian).

[30] N. N. Bogolyubov, V. S. Vladimirov, A. N. Tavkhelidze. On self-similar asymptotics in quantum field theory. *Teoret. Matem. Fiz.*, 1972, **12**(3), 305–330 (Russian).

[31] V. S. Vladimirov, Yu. N. Drozhzhinov, B. I. Zav'yalov. *Multidimensional Tauberian Theorems for Generalized Functions*. Moscow: Nauka, 1986 (Russian); English translation: Kluwer Acad. Publ., 1988.

[32] N. N. Bogolyubov, V. S. Vladimirov. A theorem on analytic continuation of generalized functions. *Nauchnye Dokl. Vysshei Shkoly. Fiz.-Mat. Nauki*, 1958, No. 3, 26–35 (Russian).

[33] N. N. Bogolyubov, V. S. Vladimirov. Representation of n-point functions. *Proc. Steklov Inst. Math.*, 1971, **112**, 1–18.

[34] A. G. Sergeev, Xiang-Yu Zhou. Extended future tube conjecture. *Proc. Steklov Inst. Math.*, 2000, **228**, 25–42.

[35] A. D. Sakharov. *Reminiscences*, two vols. Moscow: Human Rights, 1996 (Russian).

[36] *In the Intermissions... Collected Works on Research into the Essentials of Theoretical Physics in the Russian Federal Nuclear Center, Arsamas-16* (ed. Yu. A. Trutnev). Singapore: World Scientific, 1998.

[37] V. I. Lebedev, V. S. Vladimirov. The nuclear power and mathematics. *Russ. J. Numer. Anal. Math. Modelling*, 2000, **15**(3–4), 257–283.

[38] V. S. Vladimirov. Numerical solution of kinetic equation for spherical symmetry. *Vychisl. Mat.*, 1958, **3**, 3–33 (Russian).

[39] G. A. Goncharov. The 50th anniversary of the beginning of research in the USSR on the potential creation of a nuclear fusion reactor. *Physics–Uspekhi*, 2001, **44**(8), 851–858.

[40] I. V. Potugina, I. D. Sofronov. The development of basic mathematical methods and software at the All-Russian Research Institute of Experimental Physics. *Atom*, 2002, 2–11 (Russian).

[41] V. S. Vladimirov. On the application of the Monte Carlo method to find the smallest eigenvalue and the corresponding eigenfunction of a linear integral equation. *Teor. Veroyatn. Primen.*, 1956, **1**(1), 113–130 (Russian).

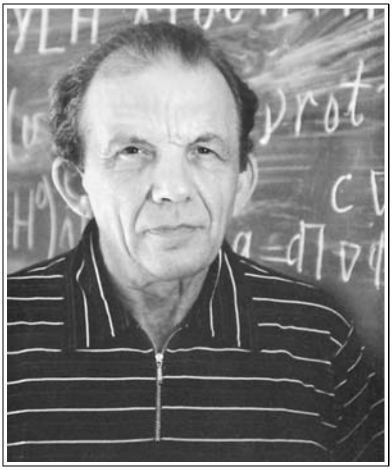
[42] V. S. Vladimirov. On the approximate calculation of Wiener integrals. *Uspekhi Mat. Nauk*, 1960, **15**(4), 129–135 (Russian); *AMS Transl.* (2), 1963, **34**, 405–412.

[43] V. S. Vladimirov, I. V. Volovich. The Wiener–Hopf equation, the Riemann–Hilbert problem, and orthogonal polynomials. *Sov. Math. Dokl.*, 1982, **26**, 415–419.

[44] V. S. Vladimirov. Mathematical problems of the one-speed theory of particle transport. *Trudy Matem. Inst. im. Steklova*, 1961, **61**, 158 pp. (Russian); English translation: Chalk River: Atomic Energy of Canada, 1963.

[45] V. S. Vladimirov. Approximate solution of a boundary-value problem for differential equation of the second order. *Prikl. Mat. Mekh.*, 1955, **19**(3), 315–324 (Russian).

The article has been slightly updated by the author for the English edition.



V. I. Yudovich

Global Solvability Versus Collapse in the Dynamics of an Incompressible Fluid

Translated by R. Cooke

This mathematical event has still not happened, even though the scientific world has been waiting impatiently for it throughout the twentieth century. The question involves the problem of global solvability of the fundamental initial/boundary-value problems of the dynamics of an incompressible fluid, both ideal and viscous. The unceasing efforts of many mathematicians have given a considerable number of first-rate results and stimulated the development and improvement of a number of mathematical theories, but the main problems remain inaccessible. A large prize has been offered for the proof of global regularity of the solutions of the Navier–Stokes system (Clay Mathematics Institute, `http://www.claymath.org`). The danger even arises that the Fermatists will now fasten onto this problem. To be sure, it is a comfort that its statement is much more difficult to understand than the statement of the Fermat theorem.

In this article I shall discuss my (unavoidably subjective) views and ideas about the state of this field, achievements in it, and the prospects for further advance. This article is not a survey. Detailed references (as usual, far from being just on the score of priority) can be found in the articles and books [1]–[4] cited below.

In 1956, when I was in my fourth year at Rostov State University, my advisor I. I. Vorovich, who was at the time *kandidat* in engineering and is now a member of the Russian Academy of Sciences, once said to me, "Look at the article of Kiselev and Ladyzhenskaya in the *Doklady*. They are applying finite differences to obtain 'local' existence theorems either for small t or for small initial velocities. But if you apply Galerkin's method, you'll probably get 'global' existence." Vorovich also directed my attention to an article of S. N. Bernshtein, in which a global existence theorem was proved for the solution of an initial/boundary-value problem for a second-order nonlinear parabolic equation. It was then that my long reflections on this problem began.

During these years the theory of nonlinear differential equations was undergoing rapid development brought about by the new ideas of functional analysis, topology, and modern calculus of variations. This solid rock — the problem of global solvability in fluid dynamics — seemed to be yielding to the pressure of researchers, and significant chunks of it were flaking off. Ultimate success seemed not too distant. How could anyone have thought at the time that it would remain an unsolved and burning question nearly half a century later?

From Bernshtein's article alone it was not difficult to discern a "general formula":

$$\text{global existence theorem} \\ = \text{local existence theorem} + \textit{a priori} \text{ estimate of the solution.}$$

Of course, this formula is also well-known in the theory of ordinary differential equations, in which the alternative to global continuation of the solution turns out to be collapse: A solution of the Cauchy problem

$$\dot{x} = f(x,t), \qquad x(0) = x_0$$

for a smooth differential equation in $\mathbb{R}^n$ is either defined for all $t > 0$ or such that $|x(t)| \to \infty$ as $t \to t_*$ for some $t_* > 0$. For infinite-dimensional systems — in particular, for systems that are described by partial differential equations — this result can be guaranteed, but not by just any *a priori* estimates of the unknown functions, only by rather strong ones; say, not just estimates in L_p but estimates with sufficiently large p, and it may not be possible to get by without estimates on the derivatives. Here is where the drama of mathematical fluid dynamics begins. However, the same is true of many other problems in the physics of continuous media.

Global Solvability as a Problem of Physics

Many physicists regard existence proofs as bureaucratic formalities and have a hostile attitude toward them, finding even the words themselves odious. In this attitude they seem to be imitating L. D. Landau — who is difficult to imitate in other respects. Landau once wrote that existence theorems occupy too much space in the course of differential equations and criticized lecturers for first making a separate study of scalar equations, then repeating the proofs in great detail for equations of higher order, then for systems of equations, and so on. The correct conclusion from this criticism would seem to be that after the scalar case one should briefly discuss the vector-valued existence theorem, after explaining that all the ideas also work beautifully in this general case. But that is not what happened. Many drew the conclusion that existence theorems should be removed entirely from courses in mathematics for physicists. Whenever I was invited to lecture to physicists, the invitation was always followed by a request: "Just remember, we don't need any existence theorems." In reply I tried to explain the physical significance of existence and uniqueness theorems. Sometimes that even seemed to work. The point was that in contrast to the local existence theorems for the Cauchy problem, which are very general in nature and rest on just the general idea of Picard's method of successive approximations, *global* existence theorems may apply only to certain exceptional classes of equations. The possibility of continuing a solution of the Cauchy problem to all times is an essentially physical property — the absence of breaks in the system. A proof of it can be based on fundamental physical laws and properties of the system alone, such as the law of increasing entropy, the laws of energy and momentum conservation, the presence of symmetry and co-symmetry, and so on.

On the other hand, the violation of global existence of a solution due to the appearance of discontinuities (which is interpreted as being the result of the solution tending to infinity within a finite time in a suitable Banach space), indicates as a rule that the model is incomplete and that it is necessary to take account of some additional factors.

In mathematical physics the following principle applies, usually implicitly: of the possible solutions, the correct one is deemed to be the one that has the greatest smoothness, or in general is the most regular in some sense. Only the nonexistence of a smooth solution compels us to pass to discontinuous solutions. The situation in the dynamics of an ideal gas is exactly that: even with C^∞-smooth initial and other data, the flow generally has discontinuities — shock waves arise (which are now unfortunately so well known to everyone). After this happens the subsequent evolution can be studied only by taking account of additional information. One must take account of viscosity and thermal conductivity properties, at least in asymptotic form, and that leads to the Hugoniot conditions at a jump.

As it happens, analogous questions also arise in celestial mechanics, which has a marvelous ability to describe the motions of large and small planets, but only under the assumption that they do not collide. If a collision occurs, obviously the original model fails, and for subsequent predictions one needs information as to the nature of the collision. And in the mechanics of continuous media the collision of particles is one of the main reasons for the appearance of discontinuities. The particles of an ideal compressible gas may indeed collide, but the answer is unknown in the case of an ideal incompressible fluid. Somewhat roughly, one may say that a proof that collisions cannot occur would yield global existence of the main initial/boundary-value problem for the Euler equations.

In physical explanations of phenomena, conservation laws are the final word. The question as to why the law of energy conservation holds has no answer. (R. Feynman: "One interesting question is whether there is a deeper basis for these conservation laws, or whether we have to take them as they are.") To be sure, for dynamical systems subject to Hamilton's principle this law follows from the invariance of the Lagrangian under a time shift. But first of all, Hamilton's principle by no means holds in all systems, and second, "Our problem is to explain where symmetry comes from. Why is nature so nearly symmetrical? No one has any idea why" (Feynman).

However, the most fundamental property of physical objects is that they exist. The requirement of global existence of a solution (for all time, or at least for all positive times) for all values of the parameters of the problem, for arbitrary forces, and so on, would obviously be a useful thing to take into account even at the stage of deriving the fundamental evolution equations. I venture to guess that many physical equations, or at least the basic conservation and dissipation laws to which they are subject, can be defined by the requirement of global solvability. Only the underdeveloped state of the corresponding mathematical theory can explain the fact that at present solvability questions arise only *post factum*, after the model has been constructed.

Perhaps it is now time to take up the investigation of a fundamental question: What can be said about a universe, given that it exists and can be described by differential equations, and that human beings live in it? In physics the "anthropic principle" has already led to some achievements. The somewhat fantastic thought arises that if we take an "arbitrary" system of differential equations as a beginning, say a polynomial system, then "after everything that can blow up has blown up" there remains a system in which conservation laws hold and which has symmetries and other good properties.

How strong the requirement of global solvability is can be seen from just the example of a scalar equation $\dot{x} = p(x)$, where $p(x) = \sum_{k=1}^{n} c_k x^k$ is a polynomial with constant coefficients. For this equation the Cauchy problem with initial condition $x(0) = x_0$ has a solution defined for all t if and only if the polynomial p is linear: $p(x) = c_1 x$. All solutions are defined for all $t > 0$ only "for one quarter of the equations": when p is a polynomial of odd degree with negative leading coefficient. It would be interesting to study the condition of global solvability (for all t) of polynomial differential equations of degree k in $\mathbb{R}^n$. One may conjecture that they lie on a submanifold of positive codimension in the space of all such equations. (For homogeneous fields of even degree the analogous result is not difficult to prove.)

The Basic Initial/Boundary-Value Problems

Ideal incompressible fluid. Leonhard Euler [1755] derived the equations that describe the evolution of the velocity vector field $v(x,t)$ of a fluid and the scalar pressure field $p(x,t)$ and density field $\rho(x,t)$. He neglected viscosity. If we also neglect compressibility and assume density constant ($\rho = 1$), these equations assume the form

$$\frac{\partial v}{\partial t} + (v,\nabla)v = -\nabla p, \tag{1}$$

$$\operatorname{div} v = 0. \tag{2}$$

Here t is time, $x = (x_1,\dots,x_n)$ is a point in $\mathbb{R}^n$, and $v = (v_1,\dots,v_n)$. Only the cases $n = 2$ and $n = 3$ have an immediate physical interpretation. The first equation here is obtained by applying Newton's second law to an element of volume of the fluid. If a force is acting on the fluid, the right-hand side of (1) will also contain the force density $F(x,t)$. The left-hand side of (1), which is also denoted $\mathrm{d}v/\mathrm{d}t$, is the acceleration of the particle of fluid that is at the point $x \in \mathbb{R}^n$ at time t. In general, the left-hand side of (1) should contain $\rho\, \mathrm{d}v/\mathrm{d}t$, where $\rho = \rho(x,t)$ is the density of the fluid. Equation (2) expresses the conservation of volume as the fluid moves.

When we consider a fluid located in a domain $D \subset \mathbb{R}^n$ with a boundary that is rigid (or, more precisely, impermeable to the fluid), the following boundary condition is also required:

$$v_n\big|_{\partial D} = 0, \tag{3}$$

where v_n is the normal component of the velocity. It would be of theoretical interest to study the more general case of a container D_t that deforms over time. Then, instead of (3) we would have to assume the condition $v_n = q_n$ on D_t, where q_n is the naturally defined normal component of the velocity of the boundary. I shall not say any more about the case when the entire boundary or some part of it is free: a boundary condition then arises for the pressure.

If we further add the initial condition

$$v\big|_{t=0} = v_0(x), \qquad x \in D, \tag{4}$$

we arrive at the basic initial/boundary-value problem in the dynamics of an ideal fluid. The field v_0 is assumed known, and it is required to satisfy the conditions $\operatorname{div} v_0 = 0$ and $v_{0n}\big|_{\partial D} = 0$.

Of course, even to write down relations (1)–(4) it is necessary to impose certain restrictions on the smoothness (regularity) of v and p as well as on the domain D. At first sight it seems natural to consider classical solutions, that is, to assume that v and p are continuously differentiable with respect to x and t in the cylinder $D \times \mathbb{R}$, and that for each t the field v is continuous up to the boundary ∂D, which should be assumed either C^1-smooth or piecewise-smooth, so that a unit normal vector $n = n(x)$ is defined on it everywhere or at least almost everywhere.

The first formulation of the problem of global unique solvability for the problem (1)–(4) now arises: *Does there exist a classical solution v, p defined for all $t \in \mathbb{R}$ under the hypothesis that the boundary ∂D and the initial field v_0 are smooth, and is that solution unique*?

The Euler equations (1)–(2) are invariant under the simultaneous substitutions $t \mapsto -t$, $v \mapsto -v$, $p \mapsto p$. This reversibility property indicates that it is reasonable to seek solutions for both $t > 0$ and $t < 0$.

In fluid dynamics a fluid is regarded as a continuous collection of fluid particles. That is why, in order to get a complete description of a flow, one must also solve the Cauchy problem

$$\dot{x} = v(x,t), \qquad x(0) = a \tag{5}$$

for all points $a \in \overline{D}$. We will then know the position $x(t) = x(t,a)$ at time t of the fluid particle that was at the point $a \in \overline{D}$ at time $t = 0$. In the Lagrangian description of the motion of the fluid the equations are written for the unknown $x(a,t)$. When this is done, the condition of incompressibility has the form

$$\det \frac{\partial x}{\partial a} = 1. \tag{6}$$

From the point of view of mechanics, Eq. (6) is an ideal holonomic constraint. Being an ideal constraint means that the reaction corresponding to it is orthogonal to the corresponding "hypersurface," which accounts for its form $(-\nabla p)$ in (1).

If D is an unbounded domain, some conditions are required at infinity. An interesting case is the problem of the motion of a fluid filling all of space when the velocity field is spatially periodic and given by the period vector $\overline{\ell} = (\ell_1, \ldots, \ell_n)$. Then D can be taken to be the period parallelepiped (a fundamental domain for the corresponding discrete subgroup of the translational group of $\mathbb{R}^n$). If the pressure p is also assumed spatially periodic, we arrive at an initial-value problem for the Euler equations (1)–(2) on the torus T^n with initial condition (4). (In general, only ∇p is spatially periodic, but the pressure p itself consists of a linear function plus a periodic function: $p(x,t) = c_i(t)x_i + \tilde{p}(x,t)$. New unknowns c_i arise, and the statement of the problem must be correspondingly generalized.)

Viscous fluid. By generalizing suitably the law of viscous friction discovered by Newton for elementary translational flows, Louis-Marie Henri Navier [1822] and George Stokes [1845] obtained the general laws of motion of a *viscous fluid*. Many scholars were so carried away by this achievement that began to call them the equations of motion of a *real* fluid, even though it is not given to us to reflect all the manifold properties of real water, air, or alcohol in a single set of equations. In the simplest case of an incompressible homogeneous fluid and for the law of friction that is (or seems to be?) the simplest, the Navier–Stokes equations can be written as follows:

$$\frac{\partial v}{\partial t} + (v, \nabla)v = -\nabla p + \nu \Delta v, \tag{7}$$

$$\operatorname{div} v = 0, \tag{8}$$

where $\nu > 0$ is the kinematic viscosity coefficient. In rectangular Cartesian coordinates the Laplacian Δ can be applied componentwise to the vector field v. The invariant definition of the Laplacian suitable for arbitrary smooth manifolds is somewhat more complicated: $\Delta = \operatorname{grad}\operatorname{div} - \operatorname{curl}\operatorname{curl}$. For general information on the dynamics of a fluid on a smooth manifold see [4].

It was not an easy task for fluid dynamicists to obtain the correct boundary condition at a rigid boundary of the domain occupied by a fluid. Naturally, one must retain the impermeability condition (3). It would seem natural to add to it a condition on the tangential component of the velocity by assuming that the fluid slides along a rigid boundary and that an external frictional force proportional to this velocity develops in the process. (More precisely, the force is proportional to the difference between the tangential velocities of the fluid and the rigid body at the given point.) However, since the time of Stokes it has been customary in fluid dynamics to assume that these two tangential velocities are the same. Doubts arose about this *adhesion* (or *no-slip*) *condition* from time to time, even in the 1960s. For example, experiments were performed to verify the adhesion condition in cases when the fluid does not wet the rigid surface, just as mercury does not wet glass. Experiments, however, showed that the coefficient of external friction must be regarded as infinitely large, and that the adhesion condition holds in every case up to cosmic velocities of mercury flow, of the order of several kilometers per second. In the case of a motionless rigid wall the adhesion boundary condition has the form

$$v\big|_{\partial D} = 0. \tag{9}$$

The inhomogeneous condition $v\big|_{\partial D} = w$ is also frequently encountered. If we integrate Eq. (8) over the domain D, we arrive at a necessary condition that the field w must satisfy:

$$\int_{\partial D} w_n \, \mathrm{d}s = 0. \tag{10}$$

If we add the initial condition, which has the same form (4), we arrive at the basic initial/boundary-value problem of the dynamics of a viscous incompressible fluid. The presence of the diffusion term $\nu \Delta v$ makes the problem irreversible, and we now have the right to ask only about the future flow of the fluid.

Demonstrating the local solvability of this problem involves proving that there exists a unique solution (v, p) satisfying conditions (7)–(9) and (4) for x and t belonging to a finite cylinder $D \times [0, T]$ for some $T > 0$. Correspondingly, the problem of global solvability consists of proving that T can be taken arbitrarily or immediately set equal to $+\infty$.

Of course, there are other versions of the boundary conditions. It is interesting to consider the motion of a fluid on the torus T^n (which has no boundaries, so that there are no boundary conditions). Of especial importance is the case when part of the boundary of the domain of flow remains unknown. This may mean a free boundary, on which one must prescribe the tension or, for example, an elastic membrane. In the latter case the role of the boundary condition, together with the adhesion condition, is played by the equation of motion of the membrane.

There are other formulations that are not always justified physically, but are useful as auxiliaries or "models." Such, for example, are conditions on an undeformable but free boundary (in which the normal velocity is zero and the tangential tensions are prescribed). The planar problem with zero normal component of velocity and a prescribed vorticity on the boundary has proved "technically" useful. (In this case it was possible to pass to the limit as $\nu \to 0$.)

The problem of passing to the limit as the viscosity disappears ($\nu \to 0$), which must be regarded as a central problem of fluid dynamics, arises along with the problem of solvability of boundary and initial/boundary-value problems for the Navier–Stokes system. The reason is that in the overwhelming majority of interesting cases the quantity ν is very small. (More precisely, one ought to speak of the corresponding dimensionless quantity $1/\mathrm{Re}$ as being small, where the Reynolds number $\mathrm{Re} = Vd/\nu$, V is a characteristic velocity of the flow, say $\max|w|$, and d is a characteristic length, say the diameter of the domain D.)

Despite the significant successes achieved by the Prandtl boundary-layer theory (especially in application to the computation of the forces acting on flying machines), this theory is by no means a theory of asymptotic integration of the Navier–Stokes equations as $\nu \to 0$. Here it is not a question of mathematical formalities, but of the fact that up to now no one has any idea what $v(x,t)$ tends to as $\nu \to 0$. It is not even clear how regular this limit is, to say nothing of the fact that the existence of the limit is also not proved. Although we believe that the limit does exist (in some sense) and satisfies the Euler equation (also in a sense not yet determined) inside the domain D.

It is clear that even in the best-possible case this convergence can be uniform only on compact subdomains of D. That can be seen immediately by comparing the boundary conditions (3) and (9): If, for example, $w = 0$, then for every $\nu > 0$ the tangential component of the velocity on ∂D is zero, while for $\nu = 0$ it is not at all necessary that it be zero; and, as a rule, it isn't. In fact, in general one cannot expect even that convergence — it is quite possible that surfaces or lines of discontinuity may form inside the domain of flow D.

This circumstance greatly complicates the understanding of the physical essence of the model of an ideal fluid. After all, fluid mechanicists believe fervently that ideal fluid flows have meaning only because they are good approximations to the flow of a viscous fluid for small ν. But there is no guarantee at all that these ideal flows satisfy the smoothness conditions usually assumed. Moreover, it may happen that in some subdomains the convergence as $\nu \to 0$ is only weak convergence. An interesting study of effects of this type was carried out by Peter Lax for the Korteweg – de Vries equation and the equations of gas dynamics. In such cases, as $\nu \to 0$ the flow oscillates strongly over space (as for, say, the equation $\nu y'' + y = 0$) and it may well happen that the weak limit does not satisfy the Euler equations in pure form, but instead the Euler equations with an additional "vibrogenic" force that arises due to the nonlinear interaction of the oscillations, such as occurs when the function $\sin^2 x/\varepsilon$ is averaged over x to yield not 0 but $1/2$.

One cannot even exclude the possibility that singular solutions of the Euler equation result in the limit as $\nu \to 0$, for example, with a vorticity that is a measure. For the two-dimensional case an existence theorem has been proved in the class of such solutions by J.-M. Delort [1990–1991].

Local Existence Theorems

Local existence and uniqueness theorems for both the Euler and Navier–Stokes equations were proved as early as the 1920–1930s. The case of a viscous fluid is conceptually simpler. Here existence theorems are based on results for the linear problem (with the term $(v, \nabla)v$ discarded). This was also not a simple matter, since it required the development of potential-theory methods. This work was to a certain extent carried out by Lichtenstein, Oseen, and Odqvist. But then the passage to the nonlinear equation did not involve any essentially new ideas and depended only to a very small degree on the specifics of the fluid-dynamic nonlinearity. The essential thing was only that the nonlinearity was subordinate to the linear viscous term Δv, was in some sense of lower order in comparison with it. The classical solutions were studied, revealing that for both stationary and nonstationary problems $C^{k,\lambda}$-smooth functions and fields (having kth partial derivatives with respect to x that satisfy a Hölder condition with exponent λ, $0 < \lambda < 1$) were better adapted than C^k-smooth functions. As a matter of fact, for the convergence of the standard method of successive approximations, starting from the linear problem, it was essential here that the nonlinearity be smooth (for example, polynomial, although exponential functions can also be admitted — the rate of increase for large values of the arguments is of no consequence) and depend only on the field v itself and its first partial derivatives with respect to x. (One can also allow dependence on the second derivatives, but only very weak dependence.) I note that it is these existence theorems that physicists especially disdain. Here one can understand their point of view, and it is not worthwhile recommending that physicists study the details of such proofs.

As Poincaré used to say, no mathematical problem is ever solved completely, so it is not surprising that Jean Leray [1933] and O. A. Ladyzhenskaya, K. K. Golovkin, and V. A. Solonnikov in the 1960s had to continue the difficult technical work on the study of fluid dynamic potentials.

Even the proof of local existence theorems for an ideal fluid, carried out in large cycles of papers by N. M. Gyunter and L. Lichtenstein in the 1920s required a deeper penetration into the fluid-dynamical essence of the problem. Judging from the dates, these two authors worked in parallel. A number of results (for example, on deformable containers) can be found only in the work of Gyunter, while others (for example, the theory of thin vortex rings) occur only in the work of Lichtenstein. And some of the papers repeat each other almost word for word. This may be because there is only one truth on earth — at least in mathematics.

At the basis of the approach of Gyunter and Lichtenstein lies a method of successive approximations that they developed, based on the intermediate use of

Lagrangian variables and the so-called Cauchy formulas, according to which the vorticity $\omega = \operatorname{curl} v$ is carried along by the current as a passive vector admixture. Let us discuss this in more detail.

The basic law of motion of an ideal incompressible fluid was discovered by Helmholtz and Thomson. Helmholtz established that the flow of vorticity across any liquid surface does not change over time. Thomson realized that this result is a consequence of *Thomson's theorem*, which says that the circulation of the velocity along any fluid contour is conserved over time.

If we assume that a smooth solution $v(x,t)$ of the Euler equations exists, then by solving the Cauchy problem (5) we find a smooth family of diffeomorphisms of the closed domain $\overline{D}$, $a \mapsto X(a,t)$, and $x(t) = X(a,t)$ is a solution of the problem (5).

From the Euler equations one can derive what are probably the most beautiful relations in fluid dynamics, the Cauchy formulas for the vorticity $\omega = \operatorname{curl} v$

$$\omega_i = \omega_{0k} \frac{\partial x_i}{\partial a_k}. \tag{11}$$

In fact these formulas can be obtained by applying exterior differentiation to Weber's formula

$$\begin{aligned} v_i \, \mathrm{d}x_i &= v_{0k} \, \mathrm{d}a_k - \mathrm{d}\mathscr{X}, \\ \mathscr{X}(a,t) &= \int_0^t \left(p - \frac{v^2}{2} \right) \mathrm{d}\tau. \end{aligned} \tag{12}$$

Let us take some closed contour γ_0 at the initial time $t = 0$ and follow the motion of the fluid particles that make up the contour. At time t they form a fluid contour $\gamma_t = X(\gamma_0, t)$. Integrating Eq. (12) over the fluid contour gives Thomson's theorem on the conservation of circulation:

$$\int_{\gamma_t} v \cdot \mathrm{d}s = \int_{\gamma_0} v_0 \cdot \mathrm{d}s_0. \tag{13}$$

If we analogously define a fluid area S_t with boundary $\partial S_t = \gamma_t$, then by applying Stokes' theorem or by directly integrating Eq. (11) we obtain Helmholtz' theorem on the conservation of the flux of vorticity

$$\int_{S_t} \operatorname{curl} v \cdot n \, \mathrm{d}S = \int_{S_{t_0}} \operatorname{curl} v_0 \cdot n_0 \, \mathrm{d}S_0. \tag{14}$$

It is obvious that conservation of circulation (13) can be obtained from (14) only in the case when the contour γ_t is the boundary of some area S_t, so that Helmholtz' theorem can be regarded as a special case of Thomson's theorem.

The Gyunter–Lichtenstein method of successive approximations is based on a certain combination of the Lagrange and Euler approaches. If we know the m-th approximation $v^{(m)}(x,t)$ for the velocity field ($m = 0, 1, \dots$ — usually one sets $v^{(0)}(x,t) = v_0(x)$), then by solving the Cauchy problem

$$\dot{x} = v^{(m)}(x,t), \qquad x\big|_{t=0} = a, \tag{15}$$

we obtain $x(t) = X^{(m)}(a,t)$. Then from Cauchy's formula (11) we can find $\omega^{(m)}(x,t)$:

$$\omega_i^{(m)}(x,t) = \omega_{0k}(a)\frac{\partial X^{(m-1)}(a,t)}{\partial a_k}, \quad a = X^{(m-1)^{-1}}.$$

Here $\left(X^{(m)}\right)^{-1} : \overline{D} \to \overline{D}$ is the diffeomorphism inverse to $X^{(m)}$. The next approximation $v^{(m+1)}$ of the velocity field can now be determined by solving the basic boundary-value problem of vector analysis:

$$\operatorname{div} v^{(m+1)} = 0, \tag{16}$$

$$\operatorname{curl} v^{(m+1)} = \omega^{(m)}, \tag{17}$$

$$v^{(m+1)} \cdot n\big|_{\partial D} = 0, \tag{18}$$

$$\oint_{\gamma_k} v^{(m+1)} \cdot \mathrm{d}s = \Gamma_k := \oint_{\gamma_k} v_0 \cdot \mathrm{d}s. \tag{19}$$

The equality (19) expresses the conservation of circulation over each of the one-dimensional homology basis contours $\gamma_1, \gamma_2, \dots, \gamma_r$ in the case of an $(r-1)$-connected domain. For a simply-connected domain these conditions drop out. For the two-dimensional problem one can take γ_k to be "internal contours" — the boundaries of holes in the domain D.

It was proved in the papers of Gyunter and Lichtenstein that for smooth data (the domain D and the initial field v_0) it is possible to construct the fields $v^{(m)}$ and prove that they are $C^{k,\lambda}$-smooth. One can then establish the convergence as $m \to \infty$ to an exact solution of the Euler equations on some time interval $[-T, T]$, where T is determined by the data of the problem, $T > 0$.

An analogous result was obtained for containers D_t that deform in a prescribed way over time. The same thing was done in the problem of motion of an unbounded fluid with an initial velocity field that is everywhere continuous and potential outside some bounded region whose boundary is the surface of discontinuity of the curl of the velocity (the so-called *weak discontinuity*). The planar problem of the same type, especially in the particular case of a constant vorticity in the domain D_0, has in recent years been the subject of rather numerous theoretical and computational studies. We shall give a separate discussion below of the dramatic events connected with this *vortex patch problem*.

Generalized Solutions

Let me repeat: the global solvability of the initial/boundary-value problems of fluid dynamics is a physical problem. And the characteristic feature of a problem of physics, even a problem of mathematical physics, is that it cannot be regarded as properly posed until it is solved. Hilbert seems to have been the first to recognize clearly that, no matter how nontrivial the concept of a solution of a partial differential equation or the problem of minimizing a functional may be, “Every problem in the calculus of variations has a solution, provided the word *solution* is properly interpreted.” Various kinds of generalized solutions of boundary-value or initial/boundary-value problems now play a multifaceted role in mathematical physics, continuum mechanics, and geometry.

In many cases, in particular, for linear and many nonlinear equations of elliptic and parabolic type, the introduction of generalized solutions is a powerful means of investigation, even when smooth solutions actually do exist. At the initial stage general existence and uniqueness theorems are proved for generalized solutions, making it possible to consider strongly discontinuous data (even generalized functions of *distributions*) as forces, sources, and physical parameters: elastic, thermal, and others. One can also consider large classes of “domains” occupied by a continuous medium, even arbitrary measurable sets. Under such general conditions there will be no solutions other than generalized ones. The problem for subsequent investigation is to determine how the differentiability properties of the generalized solution improve when the data become ever smoother. Let me explain this using the elementary example of the first boundary-value problem for Poisson’s equation

$$-\Delta u = f(x), \qquad u\big|_{\partial D} = 0 \tag{20}$$

in a “domain” D of $\mathbb{R}^m$. If the data are sufficiently regular, then if we assume that there exists a smooth solution, Eq. (20) implies the integral identity

$$\int_D \nabla u \cdot \nabla \varphi \, \mathrm{d}x = \int_D f \cdot \varphi \, \mathrm{d}x, \tag{21}$$

which holds for every smooth function φ of compact support contained in D. It is not difficult to prove a “weak converse”: if a C^2-smooth function u satisfies the identity (21), then it is a solution of Poisson’s equation. But the integral identity remains meaningful for functions of a much larger class. The left-hand side of (21) defines an inner product on the set of C^∞-smooth functions of compact support in D, and completion with respect to the metric defined by this inner product leads to a Hilbert space H_1 in which it is natural to seek generalized solutions. The functions in this space have only generalized first derivatives in $L_2(D)$. Existence and uniqueness theorems can be obtained immediately from the Riesz representation theorem for the dual of a Hilbert space, and moreover for all f that define a continuous functional $\ell_f\colon \varphi \mapsto \int_D f\cdot\varphi\,\mathrm{d}x$ on H_1. If, for example, D is a bounded measurable set (with no regularity conditions on its boundary!) we can establish by using Friedrichs' theorem and the Sobolev embedding theorem that it suffices for the power $2m/(m+2)$ of the function f to be integrable when $m>2$ and for some power $p>1$ when $m=2$. One can also take f to be a generalized function of type $\partial g/\partial x_i$, where $g\in L_2(D)$.

It is paradoxical but true that generalized solutions have an even clearer physical meaning than classical solutions. The most natural conclusion for the boundary-value problem just considered is based on the principle of minimum potential energy

$$E=\frac{1}{2}\int_D(\nabla u)^2\,\mathrm{d}x-\int_D f\cdot u\,\mathrm{d}x, \tag{22}$$

and leads directly to the definition (21) with no extra assumptions at all about the smoothness of the solution u. What emerges is that, making completely unrealistic assumptions about smoothness to begin with, we derive the Poisson equation, then we are compelled to move in the opposite direction. Of course, for the Poisson equation it is well known that when f and the boundary ∂D are sufficiently smooth, the solution u is also smooth.

In nonstationary problems of continuum mechanics, generalized solutions arise most naturally when Hamilton's principle, or the nonholonomic generalization of it taking account of frictional forces, is used. Once the equations of motion have been written down, one must again return to the integral identity using the inner product of, say, the Navier–Stokes equations (7) on an arbitrary smooth vector field $\Phi(x,t)$ and integration over the domain D occupied by the fluid and with respect to time t from $t=0$ to an arbitrarily chosen time $T>0$. As required in Hamilton's principle, the field Φ is subject to the condition $\Phi(x,T)=0$, and also the conditions $\Phi\big|_{\partial D}=0$, $\operatorname{div}\Phi=0$. This last equality is a restriction on the possible velocities arising from the incompressibility condition (6). Applying integration by parts, and taking account of the boundary and initial conditions, we obtain the integral identity in the form

$$\int_D v_0(x)\Phi(x,0)\,\mathrm{d}x+\int_0^T\int_D\left(v\frac{\partial\Phi}{\partial t}+(v,\nabla)\Phi\cdot v+\nu\nabla v\cdot\nabla\Phi\right)\mathrm{d}x\,\mathrm{d}t=0. \tag{23}$$

The pressure p has again dropped out of this equation, as always happens with an ideal constraint reaction. Moreover, under the assumption that the field v

satisfies the identity (23) and is sufficiently smooth, we find that it is a solution of the Navier–Stokes equation (7) with some pressure function $p(x,t)$.

A *generalized solution* is a field v satisfying the identity (23) whose behavior exhibits certain other regularities. These regularity requirements can be varied, yielding different definitions; one can temporarily forget about the pressure. In order for all the terms of the identity (23) to have meaning it is necessary (and sufficient) to assume that the field v and its first-order spatial derivatives belong to some $L_q(Q_T)$, where $Q_T = D \times (0,T)$. As a minimum, $v \in L_2(Q_T)$ and $\partial v/\partial x \in L_1(Q_T)$. Theoretically, one could even transfer all the derivatives from v onto Φ and introduce a generalized solution having no derivatives at all. (This is what should be called a weak solution.) In fact, the choice of the definitions only looks arbitrary. First of all, from the physical meaning of the problem it is necessary to require that for a solution, even a generalized solution, all functionals should be defined whose values are measured experimentally, most of all the kinetic energy E_k of the fluid and the dissipation energy W in time T:

$$E_k = \int_D \frac{v^2}{2}\,\mathrm{d}x, \qquad W = \nu \int_0^T \int_D (\nabla v)^2\,\mathrm{d}x\,\mathrm{d}t. \tag{24}$$

Moreover, the proof of uniqueness requires higher regularity properties of the solution. Here we find ourselves on the horns of a dilemma — if we require too much regularity, we cannot prove existence for all $T > 0$, while for those generalized solutions whose existence we can prove, we don't know how to prove uniqueness.

One of the most fundamental achievements of mathematical fluid dynamics, for which we are indebted to J. Leray [1933–1934] and E. Hopf [1950–1951], is the global existence theorem (for any bounded domains D, any $T > 0$ and arbitrary initial fields v_0 in $L_2(D)$) for a generalized solution (often called a *weak* solution) in the sense of the integral identity (23). The regularity assumptions in this case reduce to the existence of the quantities E_k and W. The initial condition for these generalized solutions is satisfied in the mean: $\|v(\cdot,t) - v_0\|_{L_2(D)} \to 0$ as $t \to 0$.

Hopf's paper is written in the classical style of modern functional analysis. It is not difficult to understand what a rush of optimism the author must have experienced when he finished work on such a remarkable result. The article ends with a promise to prove a uniqueness theorem in a subsequent paper and to establish the energy equation

$$\frac{\mathrm{d}}{\mathrm{d}t} \int_D \frac{v^2}{2}\,\mathrm{d}x = -\nu \int_D (\nabla v)^2\,\mathrm{d}x. \tag{25}$$

Alas, up to the present no one has been able to prove either of these claims. In the article only an inequality was established:

$$\int_D \frac{v^2(x,t)}{2}\,\mathrm{d}x \leqslant \int_D \frac{v_0^2(x)}{2}\,\mathrm{d}x - \nu \int_0^t \int_D (\nabla v)^2\,\mathrm{d}x\,\mathrm{d}\tau. \tag{26}$$

Any fluid mechanicist would probably say that equality actually holds in (26). And for smooth solutions this equality is indeed obvious and expresses the fact that the kinetic energy at time t is less than the initial kinetic energy by the amount dissipated through viscous forces. But the extraordinary difficulties that arise in attempts to prove that give us pause. The late Kirill Golovkin seems to have been the first [1968] to speak of the possibility of a strange mechanism of energy dissipation not connected with viscosity. The problem remains unsolved.

In infinite-dimensional systems "conservative dissipation" of energy and other quantities, for example, enstrophy — the integral of the square of the vorticity — is possible. This phenomenon is connected with the gradual loss of smoothness of the solution as time passes; energy "goes off into the higher harmonics," becomes unobservable, and is actually scattered by other, arbitrarily small dissipative forces (see [5], [6]).

In the case of an ideal incompressible fluid $\nu = 0$, even the integral identity (23) no longer contains any derivatives of the velocity. In the three-dimensional case essentially only one *a priori* estimate is known — an estimate of the kinetic energy (and also an estimate of the time derivative in a very weak sense). One might try to determine a weak solution with finite kinetic energy assuming nothing additional about regularity. But then it is not possible to prove an existence theorem even in the two-dimensional case. The reason is that the strongest known method of proving existence fails: Galerkin's method. According to this method, the solution is obtained as the limit of approximate solutions $v^m(x,t) = \sum_{k=1}^m c_k(t)\Psi_k(t)$, where $\Psi_1, \Psi_2, \dots$ are a basis in some space of solenoidal vector fields. If we require that the integral identity (23) holds for $\Phi = \Psi_1, \dots, \Psi_m$, we obtain a system of ordinary differential equations with quadratic nonlinearity for the unknown functions c_k. It is crucial that the energy equation (25) holds for this system together with the corresponding *a priori* estimate, which is uniform in m. From this estimate, global solvability of the equations for $c_1, \dots, c_m$ (these coefficients depend on m due to the nonlinearity) follows along with a certain type of weak compactness of the whole family of approximate solutions in $\mathbb{R}^n$. It remains to be proved that every weak limit point of the sequence v^m is a generalized solution. To do that one must pass to the limit in the integral identity (23) written for the approximate solutions v^m. In the case of a viscous fluid, Hopf was able to execute this plan. In the case of an ideal fluid, weak convergence in L_2 is insufficient. Strong convergence would suffice, but it is not known whether it occurs or not.

Thus there is no global existence theorem for weak solutions in the dynamics of an ideal fluid, even in the two-dimensional case. For that reason the common opinion of the favorable situation with two-dimensional flows is perhaps slightly exaggerated. The global existence theorems known here are based on an estimate of the vorticity that follows from Cauchy's formula (11). In the planar case there is only one non-zero component of the vorticity, namely ω_3, and it is preserved in every fluid particle. It results from this that for each t the function $\omega_3(x,t)$ is commensurable with $\omega_3(x,0)$. In particular, the integral

$$\int_D F\left(\omega_3(x,t)\right) \mathrm{d}x, \tag{27}$$

for an arbitrary function F, does not vary over time. This alone leads to *a priori* estimates that are fully sufficient to prove existence. The existence of weak solutions remains an important problem, however, since it may well happen that they are the ones that occur as the limits when the viscosity vanishes, even in the case of smooth data.

Smoothness of Generalized Solutions

Following the proof of the existence of a generalized solution (if that can be achieved) the question arises as to the extent to which its smoothness improves when the data of the problem become smooth. For an ideal fluid in the best-possible case one can establish only that at any time t the velocity field has as many derivatives (in L_p or Hölder-continuous) as the initial field. Actually this has been done only in two-dimensional problems.

In general, in nonlinear problems of this type one usually discovers a certain critical condition of initial smoothness such that, if one can reach it, one can trace the subsequent improvement in the differentiability properties, as the smoothness of the data increases, without any particular difficulty. In the dynamics of an ideal fluid having first-order derivatives $\partial v_i/\partial x_k$ in $L_\infty(D)$ is such a condition. The reason is that differentiating the Euler or Helmholtz equations for vorticity gives linear equations for the higher derivatives with coefficients and free term depending only on lower-order derivatives. Here the leading term contains only $\partial v_i/\partial x_k$ as coefficients.

At the same time, the conclusion that smoothness is conserved in the flow of an ideal fluid is to a certain extent purely formal. There is no reason why even the first derivatives of the vorticity should not increase without bound as

$t \to \pm\infty$, and in fact they really do increase in many cases. For those special three-dimensional flows that are defined for all t, the vorticity itself may increase without bound [6]. A more profound fact is that the Lagrangian distortions $\partial x_i/\partial a_k$ increase without bound.

In the two-dimensional case the known estimates say that the derivatives $\partial v_i/\partial x_k$ increase with time no faster than an iterated exponential $\exp(k\exp \ell t)$ with known constants $k, \ell > 0$. (It is fascinating that the recent increase in total computer power has obeyed an iterated exponential law. Such a law results for a scalar-valued variable $x(t)$ when it satisfies the differential equation $\dot{x} = u(t)x$, and the exponent of increase u obeys Malthus' law $\dot{u} = \ell u$, which means that the "population" is increasing without any environmental resistance.) Probably this rate of increase really is possible. This makes the computation of the flow of an ideal fluid a very delicate problem. Various authors from time to time announce that they have observed collapse in their computations — in a three-dimensional flow the vorticity becomes infinite in finite time. Such results, however, do not merit much confidence, since a computer considers the number 10^{20} infinitely large. Actually, it appears that the true time increase in the derivatives of the solution has been supplemented by what appears to be an even faster increase caused by the approximation. This happens because the numerical schemes violate the law of conservation of circulation and possibly other laws of fluid dynamics that we do not know about; and as a result they destroy the mechanisms that can prevent collapse.

Here it is also interesting to note the work of a number of computational experts on the numerical study of the evolution of a vortex patch — two-dimensional flow of a fluid without boundaries, with a vorticity that is nonzero only in a certain bounded region. For this problem there is an existence and uniqueness theorem in the class of generalized solutions, but the question of conservation of smoothness of the boundary of the vorticity region over time arises. Computations seem to show that strong singularities arise on it over a finite time and the computation has to be halted. Nevertheless, J.-Y. Chemin [1993], and after him A. Bertozzi and P. Constantin, established that the smoothness of the boundary of a vortex patch is conserved forever. A. B. Morgulis generalized this result to the more difficult case when the vortex patch moves in a bounded container. Of course it should in no case be thought that the computational work is wrong. On the contrary, it poses for mathematicians the problem of explaining how theory can describe the computational results and how one should modify the statement of the problem (which quantities to compute, which to average), in order to trace the subsequent evolution on a computer. It may even happen that the computational results describe experimental data better than the exact solutions — approximation errors and rounding mimic external noise. It is quite possible that the measuring devices may go out of range at exactly the time predicted in the computations.

Viscosity, like thermal conductivity, has a smoothing effect. For the heat equation or the linear Navier–Stokes equations (with the term $(v,\nabla)v$ removed) with C^∞-smooth free terms, it is well known that a generalized solution is C^∞-smooth for any $t>0$. In general, the smoothness for $t>0$ is determined by the differentiability properties of the free terms alone.

For the Navier–Stokes equations one may attempt to improve our knowledge about the smoothness of the solution by remarking that it is a solution (albeit generalized) of the linear Navier–Stokes equations with external force $-(v,\nabla)v$. Some information about this "external force" is known to us from the existence theorem for a generalized solution. Thus, in the three-dimensional case for the Leray–Hopf solutions it turns out that this force belongs to $L_{5/4}(Q_T)$, while in the two-dimensional case it belongs to $L_{4/3}(Q_T)$ for every $T>0$.

At the same time, it is known (see the references in [2]) that in the case of an external force $F\in L_\ell(Q_T)$, $\ell>1$, for sufficiently good initial data and C^2-smooth boundary, a generalized solution has generalized derivatives $\partial v/\partial t$ and $\partial^2 v/\partial x_i\partial x_k$ that also belong to $L_\ell(Q_T)$.

It also turns out that a vector-valued function v with these properties is integrable over the cylinder Q_T and, in addition, can be regarded as a continuous vector-valued function of t with values in $L_q(D)$ for some $q>1$. Similar results also exist for the spatial derivative $\partial v/\partial x$. More precisely, in the three-dimensional case the following propositions hold:

$$v\in\frac{5\ell}{5-2\ell}(Q_T),\;\; v\in\frac{3\ell}{5-2\ell}(D),\;\; \frac{\partial v}{\partial x}\in\frac{5\ell}{5-\ell}(Q_T),\;\; \frac{\partial v}{\partial x}\in\frac{3\ell}{5-\ell}(D). \tag{28}$$

(I hope that the abbreviated notations are clear.) If the denominator vanishes, the fraction can be replaced by any positive number. If the denominator is negative, it means that the corresponding function is continuous and even satisfies a Hölder condition.

Now using Hölder's inequality, we can establish that

$$(v,\nabla)v\in\ell_1(Q_T)=\frac{1}{\dfrac{5-2\ell}{5\ell}+\dfrac{5-\ell}{5\ell}}(Q_T)=\frac{5\ell}{10-3\ell}(Q_T). \tag{29}$$

If it happens that $\ell_1>\ell$, it means that the original assertion about the smoothness of the vector-valued function v is strengthened (and so it happens if $\ell>5/3$). By repeating the same procedure, we find that $(v,\nabla)v\in\ell_2(Q_T)$, and $\ell_2>\ell_1$. After a finite number of such iterations both denominators in (28) are negative, and that will mean that the Hölder continuity of v and $\partial v/\partial x$ is established. After that, one can analogously establish the existence of higher-order derivatives by applying known results on smooth solutions of the Navier–Stokes system (see [1], [2]).

I note that the value $\ell = 10/7$ would have corresponded to the initial data on the regularity of a generalized solution, but even for that value of ℓ it is not possible to establish a result for $(v, \nabla)v$. For the critical value $\ell = 5/3$ it turns out that $v \in 5(Q_T)$, $v \in 3(D)$, $v_x \in \frac{3}{2}(Q_T)$, $v_x \in \frac{5}{2}(Q_T)$. It is also good to work with generalized solutions of this class; in particular, theorems of Lyapunov type on the legality of linearization in the stability problem can be established [2]. Using more refined methods, one can even establish that the subsequent smoothness of such generalized solutions is limited only by the smoothness of the data. They are C^∞-smooth if the data are. Alas, no one knows how to obtain a global existence theorem for such regular generalized solutions.

The "bootstrap" process described above works quite successfully in the two-dimensional case and also in stationary problems. As a matter of fact, Ladyzhenskaya [1959] immediately established the existence of generalized solutions so good that the further investigation of their smoothness caused no difficulty.

Naturally, one can use different scales of functional spaces, but in each of them one discovers "critical parameters." I think it unlikely that the choice of new functional scales can lead to any fundamental advances.

Uniqueness

Let me begin by discussing the question of the uniqueness of the solution of the Cauchy problem for an ordinary differential equation in finite-dimensional space. It might appear that the available theorems of Lipschitz and Osgood, which guarantee uniqueness in the case when the field $f(x,t)$ is continuous with respect to x and t and satisfies the Lipschitz condition or the slightly weakened version of it due to Osgood, give a completely satisfactory solution of the problem. But is smoothness (even weak smoothness) with respect to x really needed? Would continuity not be enough? According to Peano, for equations $\dot{x} = f(x,t)$ in $\mathbb{R}^n$ with right-hand side f continuous with respect to x and t, a local existence theorem for the solution of the Cauchy problem holds. Simple examples show that, without additional assumptions, nonuniqueness is possible. Moreover, there exist continuous functions $f(x,t)$ on the (x,t)-plane such that the solution of the Cauchy problem is nonunique for every initial point (x_0, t_0). But to what extent is this phenomenon typical?

The opinion is widely held that nonuniqueness is typical. This prejudice appears to be supported by the example of the autonomous scalar differential equation $\dot{x} = a(x)$, for which (in the case of initial condition $x(t_0) = x_0$ with $a(x_0) = 0$) Osgood's conditions are absolutely necessary. At several seminars at Rostov University and a number of American universities, I have conducted a poll on the question whether uniqueness or nonuniqueness of the solution of the Cauchy problem for an equation with continuous right-hand side is typical. Only in the seminar at the Courant Institute did the correct answer – uniqueness is typical – receive a majority, though a slim one. Incidentally, W. Orlicz and his followers already realized that uniqueness is typical in the sense of category. In particular, Kisielewicz [1975] proved that the set of continuous functions $f(x,t)$ for which the equation $\dot{x} = f(x,t)$ has even one (!) point of nonuniqueness (x_0, t_0) is a set of first category in the sense of the natural topology of uniform convergence on compact sets. As a result, the known uniqueness theorems encompass only a rather small set (of first category!) of all the equations for which this result holds. As one should expect, in a discussion of this question, V. I. Arnold posed the question of the measure of the set of equations with uniqueness in a typical finite-parameter family. I do not know the answer.

Another thought-provoking example is the system $\dot{x} = 0$, $\dot{y} = u(x)$ for which the unique solution $x(t) = x_0$, $y(t) = y_0 + tu(x_0)$ with initial data $x(0) = x_0$, $y(0) = y_0$ is obtained without any regularity restrictions at all.

It emerges from this that the use of smoothness in the proof of uniqueness is not essential! Incidentally, all proofs of uniqueness in fluid dynamics depend on some requirements on the smoothness of the velocity field.

In essence, there is only one method of proving uniqueness, which we shall call *energetic*. Assuming that there are two solutions v and v', we form their difference $u = v - v'$ for which we obtain a linear equation with coefficients that contain v and v'. Forming the inner product of this equation with u and integrating over D, we arrive at the relation

$$\frac{1}{2}\frac{\mathrm{d}}{\mathrm{d}t}\int_D u^2(x,t)\,\mathrm{d}x = \int_D \varepsilon_{ik} u_i u_k \,\mathrm{d}x, \tag{30}$$

where $\varepsilon_{ik} = \partial v_i/\partial x_k + \partial v_k/\partial x_i$ are the deformation velocities. The relation (30) can be obtained immediately for smooth solutions. For generalized solutions the same is obtained in weakened form, integrated with respect to t with the sign $\leqslant$ instead of equality. In the case of a viscous fluid one obtains an analogous relation

$$\frac{1}{2}\frac{\mathrm{d}}{\mathrm{d}t}\int_D u^2(x,t)\,\mathrm{d}x = \int_D \varepsilon_{ik}u_i u_k\,\mathrm{d}x - \nu\int_D (\nabla u)^2\,\mathrm{d}x. \tag{31}$$

If the ε_{ik} are bounded functions, we obtain a linear differential inequality for the integral $I(t) = \int_D u^2(x,t)\,\mathrm{d}x$:

$$\dot I \leqslant c(t)I, \qquad c(t) = \max_x \sqrt{\sum_{i,k}\varepsilon_{ik}^2(x,t)}. \tag{32}$$

It follows immediately from it that $I(t) \leqslant I(0)\exp\left(\int_0^t c(\tau)\,\mathrm{d}\tau\right)$. Since $I(0) = 0$, it follows that $I(t) = 0$ for all $t \geqslant 0$. Thus there is no difficulty in proving uniqueness when the solution is sufficiently regular. As it happens, it is a peculiarity of such uniqueness theorems that it suffices to require regularity of one of the solutions. One can then prove that there is no second solution under much weaker regularity requirements.

As long as it is unknown just which solutions of the Euler equations arise as the limits of solutions of the Navier–Stokes system as $\nu \to 0$, the question of enlarging the classes of generalized solutions while retaining the property of uniqueness remains important. In any case, it is necessary to consider flows with weak discontinuities (that is, continuous velocity and bounded but discontinuous vorticity). But it does not follow from the boundedness of the vorticity that the first derivatives $\partial v_i/\partial x_k$, or even the quantities ε_{ik}, are bounded. That is why it is necessary to improve the methods of estimating the integrals on the right-hand side of Eq. (19). In the papers [7]–[9], for this purpose one applies the estimate

$$\left\|\frac{\partial v_i}{\partial x_k}\right\|_{L_p(D)} \leqslant C\frac{p^2}{p-1}\|\operatorname{curl} v\|_{L_p(D)}, \tag{33}$$

which is valid for any p ($1 < p < \infty$) with a constant C that may depend on D, but not on p. Even when $\operatorname{curl} v$ is bounded, the result is a linear increase of $\partial v/\partial x$ as $p \to \infty$, and examples show that this estimate is optimal.

Of course, it is not simply a matter of estimating the integral (30) — its convergence for fields v of class $W_p^{(1)}(D)$, say for $p > 3/2$, is obvious. The snag is that this estimate yielded a differential inequality $\dot I \leqslant f(I)$ such that uniqueness followed from it under the condition $I(0) = 0$. For that it was necessary that $f(I)$ tend to zero only a bit worse than linearly as $I \to 0$, so that the integral $\int_{+0}\frac{\mathrm{d}I}{f(I)}$ diverged. In the work [7] uniqueness was obtained for flows with a bounded vorticity; this result was extended in [8].

Let us assume that for the curl of the velocity we know an estimate

$$\|\operatorname{curl} v\|_{L_p(D)} \leqslant \theta(p) \tag{34}$$

for all $p \geqslant p_0 > 1$. For sufficiently large a, say for $a \geqslant 1$, we define the function

$$\psi(a) = \inf\left\{\frac{a^{\varepsilon}}{\varepsilon}\theta\left(\frac{1}{\varepsilon}\right) : 0 < \varepsilon < \frac{1}{p_0}\right\}. \tag{35}$$

Then (30) implies the estimate

$$I(t) \leqslant \int_0^t \beta\big(I(s)\big)\,\mathrm{d}s, \qquad \beta(I) = I\psi\left(\frac{M}{L}\right), \tag{36}$$

where $M = \max u^2(x,t)$, the maximum being taken over $x \in D$, $t \in [0,\tau]$. Uniqueness, that is, the equality $I(t) = 0$ for $t \in [0,\tau]$, holds if the following integral-divergence condition holds:

$$\int^{+\infty} \frac{\mathrm{d}a}{a\psi(a)} = \infty. \tag{37}$$

For example, if $\theta = \text{const}$, then $\psi(a) = \ln a$, and (37) holds. However, one may allow the function θ to increase without bound as $t \to +\infty$. For example, if $\theta(p) = \ln\ln p$, then $\psi(a) = \ln a$, and (37) is violated. I note that for the vorticity itself, the preceding means, in particular, that uniqueness is preserved for initial fields with a point singularity of type $\ln\ln r$, but if the vorticity has a singularity of type $\ln r$, the question of uniqueness remains open.

It is remarkable that these results on uniqueness are essentially independent of the dimension n. It is further worthwhile to note the amazing coincidence of the conditions for uniqueness of the solution with initial data in two seemingly different problems. The condition (37) leads to a unique solution both for the Euler equations (uniqueness of the vector field) and for the system (5) (uniqueness of the motion of a particle).

The condition (37) seems to be the strongest result on uniqueness that can be obtained by the "force" energetic method. In [8] there are counterexamples showing that when this condition is violated there may be nonzero fields u for which relation (30) holds. (I emphasize that these are counterexamples to the method, not to the Euler equations themselves.)

The conjecture also arises that *the solution of an initial/boundary-value problem for the Euler equations is unique if and only if the problem of particles for the initial velocity field has a unique solution*. I am not particularly confident that this conjecture is true in its literal formulation, but it seems rather likely that it may become true after suitable reworking. For example, it may be necessary to require uniqueness for almost all initial points rather than for all, or to interpret the solution of this problem in some generalized sense. What is intriguing here is the possibility of eliminating Banach spaces of functions and fields and passing to completely unstudied sets of fields for which the uniqueness theorem holds.

Viscous Fluid

In the case of a viscous fluid again no better route to proving uniqueness has been found than the application of the energetic method based on Eq. (31), or on Eq. (31) integrated with respect to t with the equal sign replaced by $\leqslant$. The situation here is essentially different from the one we face when $\nu = 0$. Of course, under the former regularity conditions for the solution one can prove uniqueness for the Euler equations, since the viscous term has a "good" sign, and can be eliminated. But the drama lies in the fact that no proof of existence of a sufficiently regular solution is known, so that one cannot avoid using the viscous term. The main obstacle to be overcome is the derivation of a suitable estimate for the integral on the right-hand side of (30). I shall not go into the details of all this — they are well-enough known. I shall say only that the situation is harmonious only for $n = 2$, where the result is achieved using Ladyzhenskaya's inequality

$$\int_{\mathbb{R}^2} u^4 \,\mathrm{d}x \leqslant 2 \int_{\mathbb{R}^2} u^2 \,\mathrm{d}x \cdot \int_{\mathbb{R}^2} (\nabla u)^2 \,\mathrm{d}x. \tag{38}$$

Here is a curious fact: To obtain the necessary estimate in $\mathbb{R}^n$ it suffices to know how to estimate the norm $\|v\|_{L_n(D)}$, and the kinetic energy, independently of dimension, is a quadratic form.

That is how mathematical fluid dynamics lives — there is a uniqueness theorem for regular solutions, but it is not known whether they exist globally, and there is a global Leray–Hopf existence theorem in the case $\nu > 0$, but it is not known whether the solution is unique. For an ideal fluid the situation is even more complex — there is no global existence theorem, even for a weak solution. But there are (Scheffer [1993], Shnirel'man [1997]) examples of nonuniqueness of the weak solution. From the point of view of fluid mechanics these last examples indicate that the definition of a generalized solution itself is incorrect. It is completely clear that if the fluid is initially at rest, and then begins to move at some later time (as in these examples) it means that in the standard definition of a weak solution this force is "invisible," that its effect on given velocity fields cannot be represented as a Lebesgue integral, and that it vanishes on all smooth fields. Functional analysis suggests that such a "force" may be, for example, an element of the second dual of the space C, which contains functionals that are differences of the limiting values of functions from the left and right.

Conclusion

Thus, what is the net result: global regularity or collapse? The methods known today have clearly reached a dead end. Charles Fefferman is right: "Profound new ideas are needed." The search will be extended into the twenty-first century, and so the problem will no doubt satisfy Smale's criterion [S. Smale, *Mathematical Intelligencer*, 1998, **20**(2)]: "A belief that the question, its solution, partial results or even attempts at its solution are likely to have great importance for mathematics and its development in the next century."

At some point all those who work on global solvability recognize the extraordinary difficulty of the problem and begin to divide their time between attempts to prove global solvability and attempts to find an example of a collapsing flow. And it is here that the effect that can be called the "resistance" of the equations of fluid dynamics to collapse reveals itself (of course, in the case of an incompressible fluid). For various kinds of symmetric modes it is possible to prove that collapse is impossible. Sometimes for flows with special symmetry (and those that demonstrably have a singularity) it happens that the corresponding quotient-system is simply unsolvable — the equation possesses symmetry, but has no solutions with that symmetry. Sometimes the collapse of symmetric flows results from the fact that the assumptions on the pressure are too strong. The pressure determines a constraint reaction and is completely determined by the velocity field; no additional assumptions about it are needed. Such a "collapse" disappears after a suitable enlargement of the class of functions in which the pressure is sought.

A brilliant example of resistance to collapse is provided by three-dimensional inviscid flows with constant pressure. If we set $p = \text{const}$, and eliminate the condition of incompressibility, the result is the equation of motion of a dusty medium. Its solution is expressed by the resolvent $(I - tA)^{-1}$ of a certain matrix $A = A(a)$, where a is the initial position of a particle of fluid. Collapse occurs whenever the matrix has real eigenvalues. Thus, it follows from incompressibility that the matrix A is nilpotent, and therefore $(I - tA)^{-1} = I + tA + t^2A^2$, and there is no collapse.

Of course, the search for collapsing flows should be continued both because symmetric flows are interesting in their own right and because the study of them helps us to get a better feeling for the nature of nonlinear interactions in fluid dynamics.

The usual course of reasoning "by contradiction" in mathematics has led to a number of important results on the possible structure of a hypothetical set E of singularities of the solution in (x,t)-space. In the case of an ideal fluid the strongest result up to now (Beale–Kato–Majda) is that

$$\int_0^T \|\operatorname{curl} v(\cdot,t)\|_{L_\infty}\, \mathrm{d}t = +\infty,$$

where T is the time at which the singularity appears. For a viscous fluid Caffarelli, Kohn, and Nirenberg [1982] established that a certain Hausdorff measure of the set E equals zero. It seems difficult to improve these results.

It is difficult to say in which direction decisive advances will be made. It may yet be, although it is very unlikely, that the problem will be solved "ingloriously" by the simple exhibition of good Lyapunov functionals. Perhaps a breakthrough will occur along the route of studying the linearized equations of fluid dynamics with nonsmooth coefficients. It may be that searches in the domain of "set-theoretic fluid dynamics" (Shnirel'man, Brenier) and the methods of measure theory will lead to a proof of global existence of a weak solution of the Euler equations.

Even so, I have no doubt that in this problem the methods of functional analysis, measure theory, potential theory, and so on, are secondary. They will probably be developed when we achieve a new understanding of the geometric properties of fluid flows. This applies especially to an ideal fluid.

When a continuous medium is deprived of its physical properties (elasticity, thermal and electrical conductivity, and so on) its property of occupying a definite position in space remains, as do elementary interactions through the mutual pressure of its parts, due to Aristotle's principle that it is impossible for two bodies to occupy the same space. It is amazing that it is these elementary interactions that cause the most complicated effects, including turbulence (viscosity of course plays an essential role in generating it).

Arnold has exhibited a beautiful example of what Pólya called "generalization by consolidation." He established that the fluid dynamical Euler equations, like Euler's equation of motion of a rigid body, are special cases of the Euler–Arnold equations for geodesics on a Lie group with a one-sided invariant Riemannian metric. These are equations on a Lie algebra. It turned out (this is where the consolidation comes in!) that all the fundamental laws of fluid dynamics (the Helmholtz–Thomson theorems on vortices, the Bernoulli equation, and so on) are special cases of the corresponding general relations. On the other hand, the equations of magnetohydrodynamics without dissipation and a number of other equations of mathematical physics, as it turned out, have the same structure, only with a different Lie group.

Arnold's intuitive constructions were partially complemented with rigorous mathematical analysis in a paper of Ebin and Marsden [1970]. Further, the Hamiltonian nature of the Lagrange equations of fluid dynamics is, of course, always obvious. Arnold [1969], however, discovered that the Euler equations also possess a particular Hamiltonian structure. The equations of fluid dynamics, like other fundamental equations of mathematical physics, admit a marvelous variety of forms (this impressed Richard Feynman long ago). But it must be admitted that up to now none of these forms has been able to solve the important problems of existence, uniqueness, and strong *a priori* estimates of the solutions. It may be that new forms of the equations will be discovered that will help to deal with these problems.

If the existence of weak solutions of the Euler equations is proved in the near future, it is still completely unclear what it will be possible to say about their uniqueness, even after suitable improvements in the definitions. Most likely they will be quite irregular. Optimal control theory and minimal surface theory seem to have gone farthest in generalizing the concept of a solution. Possibly the achievements of these areas will be of use in fluid dynamics as well. I hope that new methods of proving uniqueness of the solution of the Cauchy problem will be developed, which will also lead to new results for ordinary differential equations.

Thus, the problem of global unique solvability is not fully posed. Whether it is proved that collapse is impossible, or whether collapsing flows are found, in either case the matter must be settled by determining a solution and proving its uniqueness. If collapse is possible, additional conditions will have to be imposed.

Doubts are often expressed about the Euler and Navier–Stokes equations themselves. If these equations are slightly altered, all problems would be immediately solved. For example, it suffices to replace $(v,\nabla)v$ in (7) by $(v_h,\nabla)v$ (Leray), where v_h is the spatial average of v over a sphere of radius h, or to add $-\varepsilon\Delta^2 v$ or $\varepsilon|v|^k v$ to the right-hand side of (7). Here ε and h may be taken arbitrarily small, but the main question remains: What happens as $\varepsilon \to 0$ and $h \to 0$? It would take too much self-confidence on our part to dispense with the Navier–Stokes equations on the grounds that we do not know how to solve the problems associated with them. After all, whenever a solution has been found, it is beautifully confirmed by experiment.

After a talk devoted to weak solutions of the equations of fluid dynamics, N. N. Meiman remarked: “One must be a great optimist to undertake such pessimistic problems.” Well, let us try to hold onto our optimism.

Bibliography

[1] O. A. Ladyzhenskaya. *The Mathematical Theory of Viscous Incompressible Flow*. New York: Gordon and Breach, 1969.

[2] V. I. Yudovich. *The Linearization Method in Hydrodynamic Stability Theory*. Providence, RI: Amer. Math. Soc., 1989.

[3] R. Temam. *Navier–Stokes Equations. Theory and Numerical Analysis*, 3rd edition. Amsterdam: North-Holland, 1984.

[4] V. I. Arnold, V. A. Khesin. *Topological Methods in Hydrodynamics*. New York: Springer, 1998 (Appl. Math. Sci., **125**).

[5] V. I. Yudovich. On the gradual loss of smoothness and instability that are intrinsic to flows of an ideal fluid. *Russ. Acad. Sci. Dokl. Phys.*, 2000, **45**(2), 88–91.

[6] V. I. Yudovich. On loss of smoothness of solutions to the Euler equations and intrinsic instability of ideal fluid flows. *Chaos*, 2000, **10**(3), 705–719.

[7] V. I. Yudovich. Uniqueness theorem for the basic nonstationary problem in the dynamics of an ideal incompressible fluid. *Math. Res. Lett.*, 1995, **2**, 27–38.

[8] V. I. Yudovich. Plane unsteady motion of an ideal incompressible fluid. *Sov. Phys. Dokl.*, 1961, **6**, 18–20.

[9] V. I. Yudovich. Some bounds for the solutions of elliptic equations. *Matem. Sb.*, 1962, **59**, 229–244 (Russian); *AMS Transl.* (2), 1962, **56**, 1–18.

Name Index

C

D

G

L

M

N

O

P

Q

R

S

T

U

V