# ARNOLD: Suimming Against the Tide

Boris A. Khesin Serge L. Tabachnikov Editors



# **ARNOLD:**

Swimming Against the Tide



Vladimir Igorevich Arnold June 12, 1937–June 3, 2010



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## Epigraph

Development of mathematics resembles a fast revolution of a wheel: sprinkles of water are flying in all directions. Fashion – it is the stream that leaves the main trajectory in the tangential direction. These streams of epigone works attract most attention, and they constitute the main mass, but they inevitably disappear after a while because they parted with the wheel. To remain on the wheel, one must apply the effort in the direction perpendicular to the main stream.

-V. I. Arnold, translated from "Arnold in His Own Words," an interview with the mathematician originally published in *Kvant Magazine*, 1990 and republished in the *Notices of the American Mathematical Society*, 2012.

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# Preface

Vladimir Igorevich Arnold is one of the most influential mathematicians of our era. Arnold launched several mathematical domains (such as modern geometric mechanics, symplectic topology, and topological fluid dynamics) and contributed, in a fundamental way, to the foundations and methods in many subjects, from ordinary differential equations and celestial mechanics to singularity theory and real algebraic geometry. Even a quick look at a (certainly incomplete) list of notions and results named after Arnold is telling:

- KAM (Kolmogorov–Arnold–Moser) theory
- The Arnold conjectures in symplectic topology
- The Hilbert–Arnold problem for the number of zeros of abelian integrals
- Arnold's inequality, comparison, and complexification method in real algebraic geometry
- Arnold–Kolmogorov solution of Hilbert's 13th problem
- Arnold's spectral sequence in singularity theory
- Arnold diffusion
- The Euler–Poincaré–Arnold equations for geodesics on Lie groups
- Arnold's stability criterion in hydrodynamics
- ABC (Arnold–Beltrami–Childress) flows in fluid dynamics
- Arnold–Korkina dynamo
- Arnold's cat map
- The Liouville–Arnold theorem in integrable systems
- Arnold's continued fractions
- Arnold's interpretation of the Maslov index
- Arnold's relation in cohomology of braid groups
- Arnold tongues in bifurcation theory
- The Jordan–Arnold normal forms for families of matrices
- Arnold's invariants of plane curves

Arnold wrote several hundreds of papers, and many books, including 10 university textbooks. He is known for his lucid writing style which combines mathematical rigor with physical and geometric intuition. Arnold's books *Ordinary Differential Equations* and *Mathematical Methods of Classical Mechanics* have become mathematical bestsellers and integral parts of the mathematical education throughout the world.

Here is a brief biography and a list of distinctions of V. I. Arnold.<sup>1</sup>

V. I. Arnold was born on June 12, 1937, in Odessa, USSR. The family lived in Moscow, and Arnold graduated from the Moscow school #59. Later in life, on

 $<sup>^1\</sup>mathrm{Adapted}$  from Arnold's own CV, the Preface to his "Collected Works", Springer, 2009, and the website of the MCCME.

numerous occasions, he warmly recalled his mathematics teacher Ivan Vassilievich Morozkin. From 1954–1959, he was a student at the Department of Mechanics and Mathematics of the Moscow State University.

His M.Sc. diploma work was entitled "On mappings of a circle to itself." The degree of a "candidate of physical-mathematical sciences," an analogue of the Ph.D. degree in the West, was conferred on him in 1961 by the Keldysh Applied Mathematics Institute, Moscow; his thesis advisor was A.N. Kolmogorov. Arnold's thesis described the representation of continuous functions of three variables as superpositions of continuous functions of two variables, thus completing the solution of Hilbert's 13th problem. Arnold obtained this result back in 1957, being a third year undergraduate student (by then, A.N. Kolmogorov had shown that continuous functions of more variables could be represented as superpositions of continuous functions of only three variables).

The degree of "doctor of physical-mathematical sciences," an analogue of the Habilitation degree, was awarded to him in 1963 by the Keldysh Applied Mathematics Institute, Moscow. (The same Institute where Arnold completed his thesis on the stability of Hamiltonian systems, which subsequently became a part of what is now known as KAM theory.)

After graduating from Moscow State University in 1961, Arnold worked there until 1986. He then worked at the Steklov Mathematical Institute and later at the Paris Dauphine University.

Arnold became a corresponding member of the USSR Academy of Sciences in 1986 and a full member in 1990. He was an honorary member of the London Mathematical Society (1976), a member of the National Academy of Sciences of the Unites States (1983), the French Academy (1984), the American Academy Arts and Sciences (1987), the Royal Society (1988), the Accademia dei Lincei (1989), the American Philosophical Society (1990), the Russian Academy of Natural Sciences (1991), and the European Academy of Sciences (1991).

Arnold received a degree of Doctor Honoris Causa from the following universities: P. et M. Curie, Paris (1979), Warwick (1988), Utrecht (1991), Bologna (1991), Madrid (1994), and Toronto (1997). Arnold served as a vice-president of the International Mathematical Union from 1995–1998.

Arnold was a recipient of many awards, among them the Lenin Prize (1965, jointly with A. N. Kolmogorov), the Crafoord Prize (1982), the Lobachevsky Prize of the Russian Academy of Sciences (1992), the Harvey Prize (1994), the Dannie Heineman Prize for Mathematical Physics (2001), the Wolf Prize in Mathematics (2001), the State Prize of the Russian Federation (2007), and the Shaw Prize in Mathematical Sciences (2008).

One of Arnold's most unusual distinctions is that there is a small planet, Vladarnolda, discovered in 1981 and registered under #10031, named after him, Vladimir Arnold.

V. Arnold died suddenly in Paris on June 3, 2010, and he was buried in Moscow.

This book is a tribute to Vladimir Arnold, the mathematician, the teacher, and the person. Most of the memory articles included in this book were published in two issues of the *Notices of American Mathematical Society* in 2012. The reader will also find here three additional memories, by L. Polterovich, A. Vershik, and S. Yakovenko.

#### PREFACE

The book begins with a full translation into English of the interview that Arnold gave to the Russian magazine "Kvant" in 1990 (to the best of our knowledge, only excerpts from the full interview have appeared in English before). This is followed by reprints of Arnold's lecture at the Fields Institute in 1997 (at a conference in honor of his 60th birthday) and his article "Polymathematics". We also include a reprint of his "Mathematical Trivium", a collection of 100 mathematical problems that, in Arnold's opinion, delineate standards of undergraduate mathematical education. The problems are commented upon by the editors of this book. This commentary is followed by Arnold's article swritten by Arnold's colleagues, students, and friends.

A few words about the front and back covers. The front cover illustrates Arnold's love for outdoor activities. Some articles in the second half of the book describe this side of his personality in detail. The back cover reproduces the napkin Arnold wrote on at a meal with Emmanuel Ferrand. This is what Ferrand says about this: "Arnold wrote on this napkin at the occasion of a private meal at IHES (Institut des Hautes Etudes Scientifiques), Bures sur Yvette near Paris, France. As far as I remember, it was in February of 2006... Most of what is written here is related to the question of the enumeration of the topological types of Morse functions on surfaces. The two drawings with the letters A, B, C correspond to two of his favorite examples: the height functions of the Stromboli and Etna, two famous volcanos in Italy."

Vladimir Arnold has made a deep and lasting impression on everyone who knew him, and his impact on mathematics is there to stay. We hope that the reader will share our admiration of this remarkable man of science.

Boris A. Khesin and Serge L. Tabachnikov

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### PHOTOGRAPHS

Courtesy of Ilya Zakharevich: The Frontispiece portrait of V. I. Arnold.

**Courtesy of MCCME:** At the Globus seminar, Independent University of Moscow; At the Mathematical Olympiad on "Arnold's problem for all ages", with V. Kleptsyn, S. Gusein-Zade, S. Lando.

**Courtesy of Svetlana Tretyakova:** Arnold Lecturing; In Paris; With a participant of the Dubna school; With B. Khesin; Four portraits.

**Courtesy of Masha Khesin:** Speakers at Arnoldfest, Fields Institute 1997; With his wife Elya; Arnold sitting at Fields Institute, 1997; With Yu. Chekanov, V. Zakalyukin, A. Khovanskii; With Ya. Eliashberg; Arnold's son Igor and wife Yulya.

**Courtesy of Boris Khesin:** Course lecture on ordinary differential equations, Moscow State University, 1983; Mid-1985, at Moscow State University; In the woods near New Haven, CT, 1993; Swimming in November 1993, New Haven, CT; With B. Khesin.

**Courtesy of Tanya Belokrinitskaya:** With A. Khovanskii and I. Scherbak; With Yu. Ilyashenko; On Niagara Falls with A. Gabrielov, V. Zakalyukin and A. Khovanskii.

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**Courtesy of Marina Ratner:** On the Golden Gate Bridge, 1989; In Yosemite, California, 1989.

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Courtesy of Ya. Eliashberg: Lecturing in Syktyvkar, 1976.

©Victor V. Akhlomov, reprinted with permission from Olga Varshavskaya: I. G. Petrovsky with A. Kirillov and V. I. Arnold, around 1960; Arnold with students at Kolmogorov's mathematical boarding school, the 1960s.

©Frost Photo, used with permission: Receiving the Doctor Honoris Causa Degree from University of Toronto, June 1997; With Elya at ceremony; With Miles Reed at ceremony.

Courtesy of Francesca Aicardi: Front cover image: Walking in "Nature".

**Courtesy of Emmanuel Ferrand:** Back cover image: V. I. Arnold's handwriting on a napkin.

# Part 1

# By Arnold

### CHAPTER 1

### Arnold in His Own Words

### V. I. Arnold

In 1990, one of us (S.T.) interviewed V. Arnold for the Russian magazine "Kvant" (Quantum). The readership of this monthly magazine for physics and mathematics consisted mostly of high school students, high school teachers, and undergraduate students; the magazine had circulation of about 200,000. As far as we know, the interview was never translated into English in full. We translate this interview;<sup>1</sup> the footnotes are ours.

**Q**: How did you become a mathematician? What was the role played by your family, school, mathematical circles, olympiads? Please tell us about your teachers.

A: I always hated learning by rote. For that reason, my elementary school teacher told my parents that a moron, like myself, would never manage to master the multiplication table.

My first mathematical revelation was when I met my first real teacher of mathematics, Ivan Vassilievich Morozkin. I remember the problem about two old ladies, who started simultaneously from two towns toward each other, met at noon, and who reached the opposite towns at 4 pm and 9 pm, respectively. The question was when they started their trip.

We did not have algebra yet. I invented an "arithmetic" solution (based on a scaling—or similarity—argument) and experienced the joy of discovery; the desire to experience this joy again was what made me a mathematician.

The first mathematical book for me, at the age of twelve, was "Von Zahlen und Figuren"<sup>2</sup> by Rademacher and Toeplitz. I worked through a few pages a day. A year later, my uncle, an engineer-driller N. B. Zhitkov, told me in one evening what mathematical analysis was. His story ended with determining the shape of water in a revolving glass. After that I found and read the analysis textbook by Granville and Luzin, and then started to read, without discrimination, all mathematical books from the library of my early-deceased father (I am a mathematician in the fourth generation). My favorite one was "Introduction to the Analysis of the Infinite" by L. Euler (partitions, generating functions) and "Course of Analysis" by Ch. Hermite (complex analysis, elliptic integrals).

A. A. Lyapunov organized at his home the "Children Learned Society". The curriculum included mathematics and physics, along with chemistry and biology,

<sup>&</sup>lt;sup>1</sup>The original Russian text is available on the web site of Kvant magazine http://kvant.mccme.ru/1990/07/intervyu\_s\_viarnoldom.htm.

 $<sup>^2</sup>$  "The enjoyment of math", in English translation. The title of the Russian translation was close to the German original.

including genetics that was just recently banned<sup>3</sup> (a son of one of our best geneticists was my classmate; in a questionnaire, he wrote: "my mother is a stay-at-home mom, my father is a stay-at-home dad").

At that time (in the mid 1950s), mathematical circles for 7<sup>th</sup>-10<sup>th</sup> graders flourished at Moscow State University. On Sundays, professors gave lectures to high school students (many are published in the series "Popular Lectures on Mathematics"). By the time we graduated from high school, we had a rather clear picture of the merits (and demerits) of the majority of the lecturers. High school students were more sensitive to falsehood and cheating than undergraduate students since they were not yet used to pretending that they understood what cannot be understood at all (I am afraid that high school students of this day have lost this advantage, and the lectures for high school students do not exist anymore).

My math circle was run by A. P. Savin, N. D. Vvedenskaya, T. D. Ventzel, and I. A. Vinogradova. They are all accomplished mathematicians now; then, they were undergraduate students. The circles of that time provided much less mathematical knowledge than their counterparts today, but each session was a feast. The cult of truth, beauty and independence ("a student is not a sack to be filled but a torch to be lighted") restricted quantity of acquired knowledge but boosted its quality. It is in these debates over solutions to the problems that we learned complete and full understanding and mathematical rigor. Physics was pushed aside, the beauty of mathematics eclipsed it for a long time.

Mathematical olympiads, along with mathematical circles and lectures, were organized then by the Moscow Mathematical Society; they were attended by thousands. My results improved from honorable mention in the 7th grade to the second prize in the 9th and 10th. The emotional importance of the olympiads was very high, but now I remember the circles and lectures better. Up to this day I appreciate the excellent choice of the books received as the prizes at olympiads: "Geometry and Imagination" by Hilbert and Cohn-Vossen, "What is Mathematics?" by Courant and Robbins, "Linear Algebra" by G. Shilov, "Analytic Mechanics" and "Analysis" by Vallée Poussin which came with a wide stamp that read "to a winner of the Moscow Mathematical Olympiad".

**Q**: You have been actively working in mathematics for over 30 years. Has the attitude of the society towards mathematics and mathematicians changed?

A: The attitude of the society (not only in the USSR) to fundamental science in general, and to mathematics in particular, is well described by I. A. Krylov in the fable "The Hog Under the Oak".<sup>4</sup> In the 1930s and 1940s, mathematics suffered in this country less than other sciences. It is well known that Viète was a cryptographer in service of Henry IV of France. Since then, certain areas of mathematics are supported by all governments, and even Beria<sup>5</sup> cared about preservation of mathematical culture in this country.

In the last 30 years, the prestige of mathematics has declined in all countries. I think that mathematicians are partially to be blamed as well—foremost, Hilbert and

 $<sup>^{3}\</sup>mathrm{In}$  1948, genetics was officially declared "a bourgeois pseudoscience" in the former Soviet Union.

 $<sup>^{4}\</sup>mathrm{See}$  a (slightly modernized) translation of this early 19th century Russian fable at the end of this interview.

<sup>&</sup>lt;sup>5</sup>The monstrous chief of Stalin's secret police.

Bourbaki—the ones who proclaimed that the goal of their science was investigation of all corollaries of arbitrary systems of axioms.

#### **Q**: Does the concept of fashion apply to mathematics?

A: Development of mathematics resembles a fast revolution of a wheel: sprinkles of water are flying in all directions. Fashion - it is the stream that leaves the main trajectory in the tangential direction. These streams of epigone works attract most attention, and they constitute the main mass, but they inevitably disappear after a while because they parted with the wheel. To remain on the wheel, one must apply the effort in the direction perpendicular to the main stream.

**Q**: Do the criteria of mathematical rigor change with time? Do computer experiments have to do with "real" mathematics (for example, in the theory of fractals)? Does a mathematical researcher need a computer? Do you use computers in your research? Recently we read much about the new discipline, "the catastrophe theory". Is this a new science or another fad?

**A**: As far as I know, the criteria for rigor have not changed from the time of Euclid.

Computers provide a huge opportunity for experimentation, and I use one, along with a slide rule and a multiplication table. I think that without experimentation of some kind, most mathematical results would never be discovered. Computer experiments added somewhat to the brilliant works of Julia, Fatou and others about iterations of polynomials. Fractal set is just a term.

A mathematician finds it hard to agree that the introduction of a new term, not supported by new theorems, constitutes a substantial progress. However, the success of "cybernetics", "fractals", "synergetics", "catastrophe theory", and "strange attractors" illustrates the fruitfulness of word creation as a scientific method.

Poincaré said: "It is incredible how much a well-chosen word can economize thought. Often one only needs to invent a new word, and this word becomes a creator on its own right".<sup>6</sup> By leaving scientific terms ("files", "interfaces") untranslated we lose the power of this method.

As to "catastrophe theory", this term was invented to attract the public attention to really important mathematical achievements: to singularity theory of smooth functions and to bifurcation theory of dynamical systems. The simplest conclusions of catastrophe theory (for example, that a continuous motion from a bad stable regime to a better one leads to worsening of the state, that the speed of this worsening increases as one approaches the better regime, that the resistance of the system, originally small, also increases, and that if one overcomes this resistance, the system momentarily changes to the better state, and otherwise returns equally catastrophically fast to the bad state) are undoubtedly correct, but alas, they do not prevent catastrophes.<sup>7</sup>

**Q**: Mathematics is a very old and important part of human culture. What is your opinion about the place of mathematics in cultural heritage?

<sup>&</sup>lt;sup>6</sup>Perhaps Arnold was quoting by memory. Here is a similar quotation: "We have just seen, through an example, the importance of words in mathematics, but I could cite many more cases. It is scarcely credible, as Mach said, how much a well-chosen word can economize thought". H. Poincaré, "The future of mathematics".

<sup>&</sup>lt;sup>7</sup>The reader should bear in mind that this remark was made in the heat of perestroyka.

A: The word "Mathematics" means science about truth. It seems to me that modern science (i.e., theoretical physics along with mathematics) is a new religion, a cult of truth, founded by Newton 300 years ago.

**Q**: When you prove a theorem, do you "create" or "discover" it?

**A**: I certainly have a feeling that I am discovering something that existed before me. To quote from A. K. Tolstoy:

Vainly, the author, you picture yourself as your artworks' creator, Over the Earth they have hovered forever remaining unnoticed. Space harbors many invisible shapes and inaudible motives, As it holds many delightful ensembles of colors and letters.<sup>8</sup>

**Q**: Please tell us about your mathematical interests. Are there results that can be explained to "Kvant"'s readers?

**A**: a). D. Hilbert used to say that a really good mathematical result can be explained to a man on the street. One of my results implies the impossibility of a long-term forecast, no matter how many powerful computers are used. A passerby would probably understand this, but it would take too much time to explain the relevant mathematics.

b). Imagine a charged particle that moves with speed v in the horizontal plane subject to a vertical magnetic field H(x, y). If the field is constant then the orbit of the particle is a circle of radius Cv/H. Assume now that the magnetic field depends on the point. Then each coil of the orbit will resemble a small circle, provided the initial speed is small. From the mathematical point of view, this orbit is just a plane curve whose radius of curvature at each point has a prescribed value Cv/H.

If this radius varies, the coils are not closed anymore, and the center of the coil starts to "drift". Although this drift is small over one coil, it may accumulate over time, and the particle may go far away.

Where will it drift? This question is the simplest model of the problems that appear in various situations: in the study of the motion of charged particles in accelerators and magnetic traps for plasma, in the analysis of the small perturbations exerted by planets on each other in celestial mechanics, in the study of stability of fast spinning bodies in the theory of gyroscopes. Applied to our particle in the plane, the result is as follows:

**Theorem.** The orbit forever stays in a narrow annulus between two close level curves of the function H, assuming that these level curves are closed and that the radius Cv/H is sufficiently small.

The readers that have access to computers can check this experimentally. It is easier to experiment with a discrete version of this theory. Consider the map T = AB of the plane where A is the rotation through  $2\pi/q$  and  $B(x, y) = (x, y + a \sin x)$ where q is an integer, say 5, and a is a small parameter, say 0.05.

**Theorem.** For most initial points P, the points  $P, T^q(P), T^{2q}(P), \ldots$  lie on a smooth closed curve, provided that the parameter a is sufficiently small.

For special values of P, the orbit gradually fills a beautiful unbounded figure that G. Zaslavsky and R. Sagdeev called *a stochastic web.*<sup>9</sup>

<sup>&</sup>lt;sup>8</sup>Translation of A. Givental and E. Wilson-Egolf.

 $<sup>^9\</sup>mathrm{See}$  http://www.scholarpedia.org/article/Zaslavsky\_web\_map.

The same mathematical theory proves the stability of the inverted pendulum whose pivot rapidly oscillates in the vertical direction (I am talking about non-linear oscillations without friction).<sup>10</sup> One can experiment with an electric shaver, or a sewing machine (P. L. Kapitsa), or a linear accelerator with hard focusing (A. M. Budker).

c). The reader is well familiar with the quadratic formula. Equations of degrees 3 and 4 also can be solved in radicals. Equations of degree 5 already are not solvable in radicals, but they still can be reduced to one special equation  $f^5 + xf + 1 = 0$ . Thus the answer can be expressed via arithmetic operations, roots, and one special function f(x). Equations of degree 6 similarly reduce to special functions of two variables.

Using functions of two variables, one can make functions of several variables by substitution (for example, f(g(x, y), h(z, y)) is a function of three variables). D. Hilbert formulated his 13th problem as follows: Which continuous functions of three variables can be expressed as superpositions of continuous functions of two variables? Hilbert thought that already the function f(x, y, z), defined by the equation  $f^7 + xf^3 + yf^2 + xf + 1 = 0$ , cannot be expressed this way.

**Theorem**. Every continuous function of any number of variables is a superposition of continuous functions of two variables.

That functions of several variables were superpositions of functions of three variables was discovered by A. N. Kolmogorov, so it remained for me to improve the result from three to two.

The problem of representing a function by a superposition of algebraic functions remains open, and it is very interesting. It is expected that the "topological complexity" of the ramification of a multi-valued function is an obstruction to such a representation.

d). "Kvant"'s readers know what the curves given by equation of degree two look like: ellipses, hyperbolas, and parabolas. But what the curves of high degree look like is unknown. The greatest number of components (ovals) that a curve of degree n may have is

$$N = \frac{(n-1)(n-2)}{2} + 1,$$

(where the points at infinity count as well so, for example, the hyperbola consists of *one oval only*).

The question what the mutual position of these N ovals could be is part of Hilbert's 16th problem. If n = 4 then all N = 4 ovals are disjoint (do you see why?) Let us call an oval even if it lies inside an even number of other ovals, and odd otherwise (so that all four ovals of a curve of the 4th degree are even).

**Theorem.** The difference between the number of even and odd ovals of a curve of degree n = 2k with the maximal possible number of ovals is equal to  $k^2$  modulo 8.

**Example**. Out of 11 ovals of degree 6, the number of those that may lie inside the others is either 5 or 9 (why?). In particular, all 11 ovals cannot be disjoint.

The history of this theorem is as follows. A mathematician from Gorky, D. A. Gudkov, studied all mutual positions of the ovals of a curve of degree 6. He noticed that the comparison in the theorem held in all his examples and he conjectured that it was always the case. I. G. Petrovsky asked me to check the work of Gudkov that

<sup>&</sup>lt;sup>10</sup>See, e.g., http://www.youtube.com/watch?v=cHTibqThCTU.

was technically very complicated. I was stunned by Gudkov's conjecture since there was no visible connection between the position of the ovals and arithmetics. But I recalled that there were theorems in 4-dimensional topology whose formulations involved the residue mod 8. And indeed, it turned out that the position of the ovals of the plane curve f(x, y) = 0 is governed by the 4-dimensional manifold given by the complex solutions to the equation  $f(x, y) = z^2$ .

The discovery of this connection between the ovals and 4-dimensional topology (and hence, with the arithmetics of integral quadratic forms) made it possible to prove Gudkov's conjecture mod 4. After that, V. A. Rokhlin, a leading expert in 4-dimensional topology, managed to prove Gudkov's conjecture in full generality. Since then, a number of remarkable results have been proved in real algebraic geometry (and some of them were even discussed in "Kvant"). Still, one doesn't know what are all possible arrangements of the 22 ovals of a curve of degree 8.

Concrete topological problems of this kind (for polynomials of a fixed degree) reduce, in principle, to finite algebraic computations. But these computations apparently exceed the capacity of modern computers; at least until now, not a single new result in this area has been obtained by the aid of computer.

# **Q**: You spend much time popularizing mathematics. What is your opinion about popularization? Please name merits and demerits of this hard genre.

A: One of the very first popularizers, M. Faraday, arrived at the conclusion that "Lectures which really teach will never be popular; lectures which are popular will never teach." This Faraday effect is easy to explain: according to N. Bohr, clearness and truth are in the quantum complementarity relation.

"Kvant"'s attempts to overcome this complementarity are laudable, but unfortunately, the magazine is somewhat eclectic: its mathematical and physical parts are not quite aligned. Compared with the brilliant article of M. P. Bronstein about X-rays, many contemporary popularizers, alas, look helpless. The most boring sections are the ones concerning tutoring for exams and chess. Full color print often does disservice to the figures, and they would benefit if they were closer to the encyclopedic traditions of the 18th century. The problems are usually good, especially the ones for elementary schoolers.

**Q**: Many readers of Kvant aspire to become mathematicians. Are there "indications" and "contraindications" to becoming a mathematician, or anyone interested in the subject can become one? Is it necessary for a mathematician-to-be to successfully participate in mathematical olympiads?

A: When 90-years-old Hadamard was telling A. N. Kolmogorov about his participation in Concours Général (roughly corresponding to our olympiads) he was still very excited: Hadamard won only the second prize, while the student who had won the first prize also became a mathematician, but a much weaker one!

Some olympiad winners later achieve nothing, and many outstanding mathematicians had no success in olympiads at all.

Mathematicians differ dramatically by their time scale: some are very good tackling 15-minute problems, some are good with the problems that require an hour, a day, a week, the problems that take a month, a year, decades of thinking... A. N. Kolmogorov considered his "ceiling" to be two weeks of concentrated thinking.

A success in an olympiad largely depends on one's sprinter qualities, whereas a success in serious mathematical research requires long distance endurance (B. N. Delaunay used to say: "a good theorem takes not 5 hours, as in an olympiad, but 5,000 hours").

There are contraindications to becoming a research mathematician. The main one is lack of love of mathematics. Also, to quote from a poet:

> Each walk of life attracts a random Tom. Of talent by misfortune destitute, He with the coldness of a foster mom Is treated by his favorite pursuit.<sup>11</sup>.

But mathematical talents can be very diverse: geometrical and intuitive, algebraic and computational, logical and deductive, natural scientific and inductive. And all kinds are useful. It seems to me that one's difficulties with the multiplication table or a formal definition of half-plane should not obstruct one's way to mathematics. An extremely important condition for serious mathematical research is good health.

**Q**: Tell us about the role of sport in your life.

A: When a problem resists a solution, I jump on my cross country skis. Forty kilometers later a solution (or at least an idea for a solution) always comes. Under scrutiny, an error is often found. But this is a new difficulty that is overcome in the same way.

### The Hog Under the Oak I. A. Krylov, 1769–1844

A Hog under a mighty Oak Had glutted tons of tasty acorns, then, supine, Napped in its shade; but when awoke, He, with persistence and the snoot of real swine, The giant's roots began to undermine. "The tree is hurt when they're exposed." A Raven on a branch arose. "It may dry up and perish — don't you care?" "Not in the least" The Hog raised up its head. "Why would the prospect make me scared?" The tree is useless; be it dead Two hundred fifty years, I won't regret a second. "Nutritious acorns — only that's what's reckoned!"— "Ungrateful pig!" the tree exclaimed with scorn. "Had you been fit to turn your mug around You'd have a chance to figure out Where your beloved fruit is born." Likewise, an ignoramus in defiance Is scolding scientists and science, And all preprints at lanl\_dot\_gov, Oblivious of his partaking fruit thereof.<sup>12</sup>

<sup>&</sup>lt;sup>11</sup>E. Evtushenko, translation by A. Givental and E. Wilson-Egolf.
<sup>12</sup>Translation of A. Givental and E. Wilson-Egolf.

### CHAPTER 2

# From Hilbert's Superposition Problem to Dynamical Systems

### V. I. Arnold

ABSTRACT. This is the first of the series of three lectures given by Vladimir Arnold in June 1997 at the meeting in the Fields Institute dedicated to his 60th birthday.

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.

Today, I shall try to explain the diversity of subjects I was working on. In fact, I was following 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.



Figure 1: The Mathematical Mushroom

In mathematics, the upper part of the mushroom corresponds to theorems that you see, but you don't see the things which are below, that is: *problems, conjectures, mistakes, ideas, and so on.* 

You might have several unrelated mushrooms being unable to see what their relation 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, the logic, while for the other part the

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left brain has no role at all, since this part is highly illogical. It is hence difficult to communicate it to others. But in this series of lectures I shall try to do it.

Today, I shall mostly discuss the history, in the second lecture — the hidden part of the mushroom, the irresponsible ideas, providing the main motivation for the research. And then some theorems will appear in the last lecture.

The first serious mathematical problem with which I started was formulated by Hilbert. It is a problem on superpositions emerging from one of the main mathematical problems: 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 is not solvable — that there is no such formula.<sup>1</sup> However, there is a classical result that if you know how to solve one very special equation

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

i. e. 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 — square roots and cubic roots (which can be considered a simple special function), for quartic equations also the root of degree 4, but in this case (deg = 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 this was supposed to be done? You kill the terms of the equation one by one using some substitutions and to find those 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. 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.

<sup>&</sup>lt;sup>1</sup>I have lectured on this topological version of Abel's theorem to Moscow high-school children. This course, supplied with exercises, was later published by one of the listeners, V. Alekseev, in the form of a nice book "Abel's theorem in problems". Unfortunately, it was never translated into English. [This book is now available in English: V. Alekseev, *Abel's theorem in problems and solutions. Based on the lectures of Professor V. I. Arnold. With a preface and an appendix by Arnold and an appendix by A. Khovanskii.* Kluwer Academic Publishers, Dordrecht, 2004. *Editors' Note*]. My student A. G. Khovansky in his thesis has extended these topological ideas to differential algebra. 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.

For degree 7 the same procedure leaves you 3 coefficients

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

which defines an algebraic function of 3 variables z(a, b, c).<sup>2</sup> Hilbert asked whether functions of three variables really do exist.

If you have a function in two variables z(a, b) and you put inside, say instead of a, a function in two variables a(u, v) and continue in this way — 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 by combinations of *continuous* functions you can get any continuous function 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 has done it. In 1956 I was an undergraduate student and 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 he gave this problem to me.

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, depending on one coordinate each. 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 in a minute for dynamical systems. In the simplest example of Fig. 2:



Figure 2: The Simplest Tree

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 of the tree, you take the value of the function at this point and you decompose it arbitrarily. Then at point B lying on the same

<sup>&</sup>lt;sup>2</sup>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.

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, you will have to work more, and in fact to make the infinite processes converge you need 3 variables, not 2, but here 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.<sup>3</sup> I would say that this was the *genuine* Hilbert problem. He did not formulate it in this way — unfortunately, 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 Vitushkin in the beginning of the 50's. Vitushkin 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 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.

His 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 in 2 variables, say of degree n, 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 working on this problem: Hilbert himself obtained some results, Harnack found the number of ovals for the curves. For high dimensional varieties the problem was studied by Petrovsky and his student Oleinik. They found the bounds for the Betti numbers of algebraic varieties defined by (systems of) polynomial equations in terms of the degrees and the 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 Petrovsky of 40's and 50's was later rediscovered in the West by Thom and by Milnor. Although they did quote Petrovsky

<sup>&</sup>lt;sup>3</sup>The given function being an entire algebraic function (without poles), 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.

and Oleinik, the results are mostly attributed to Milnor and Thom, who introduced the modern terminology related to Smith theory and to the interaction between the homology of real and complex manifolds. But stronger results were already present in those papers by Petrovsky and Oleinik and 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  $z(a_1, ..., a_n)$  defined by the equation  $z^n + a_1 z^{n-1} + ... + a_n = 0$ . This function has a complicated discriminant hypersurface in the base space of coefficients  $\mathbb{C}^n = \{(a_1, ..., a_n)\}$ . The discriminant hypersurface is the set of all points where the function z is not nice, in particular, not smooth. In the 60's, around 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 have 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 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 been even formalized (by A. Szücs and R. Rimányi, 1996).

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

Thinking on this, I decided to find the cohomology ring of the braid group. I have computed the first dozen of those 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, this 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 to be 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 Smale in his theory of complexity of the computations. In the topological complexity theory by Smale he discovered (using a theory which was essentially developed by Albert Schwarz years before) that the structure of the cohomology ring of the complementary space of the discriminant is an obstacle to compute numerically the roots of a complex polynomial with few branchings in the algorithm. For polynomials of degree n Smale proved (using essentially my computations of the cohomology) that the complexity is at least  $n^{2/3}$ (you really need this number of branchings). One can obtain stronger results using the information about braids found in the marvellous paper by Fuchs "Cohomology of the braid group modulo 2" published in "Functional Analysis". By the way, the English translation of this paper was titled "Cohomology of the group kosmod 2", because 'braid group' is 'gruppa kos' in Russian.<sup>4</sup> Everybody thought that the paper was related rather to space research and probably because of that it was not appreciated at that time as it should be. But later it was understood and now Vassiliev using the results of Fuchs has increased the number of topologically necessary branchings to  $n - \log(n)$ , 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 problem 13! Later, inspired by the analogy between between algebraic functions and fiber bundles, I constructed a theory of characteristic classes of entire algebraic functions and found a class, invariant under the substitutions, so I was able to prove some impossibility theorems 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 the Chebotarev's ideas dating back to the 40's 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,  $z(a_1, \dots, a_n)$ , and only this function. 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, parasite roots, because you have some signs in the formula, some multivaluedness. The difficulty is how to understand such functions as, say,  $\sqrt{z} - \sqrt{2z}$ . 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. There's perhaps some kind of a mixed Hodge structure whose weight filtration provides the information on the dimension of the smooth algebraic manifold 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 and my conjecture is that our special function z(a, b, c) cannot be represented as a combination of algebraic functions in two variables for some essentially topological reasons. I think, the representation remains impossible even if we replace all the algebraic functions in the superposition by non-holomorphic complex functions which are *topologically* equivalent to algebraic ones.

Now I'm coming to mechanics. As I have mentioned, my supervisor was Kolmogorov. He formulated the problem at the seminar and went to Paris for a semester. When he returned, I explained him 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

<sup>&</sup>lt;sup>4</sup>See A. Givental's and E. Wilson-Egolf's (slightly modernized) translation of this early 19th century Russian fable at the end of the interview.

be glad to discuss with you any kind of mathematics, but do not ask me the next problem. Choose it yourself, this will be much better for you."

Perhaps I should explain one more thing here. He took my first article, for the Doklady (the Russian *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 the 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 Gelfand, who was also his student. This is one of the few papers signed by Gelfand alone, with no collaborators. Gelfand, 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 papers. I dare to guess that these papers were physically written in most cases by the collaborators.

I have never collaborated with Gelfand. Recently, at the Zürich Congress he had asked me what is 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 Gelfand there is a paper which is not by Gelfand and where he is not even a coauthor. It's my paper signed *Arnold*. I shall now explain what is it about and how is it related to my story.

When Kolmogorov suggested 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. (Gelfand 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 had chosen.

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? <sup>5</sup> So I started to study the curves with cycles.

<sup>&</sup>lt;sup>5</sup>This problem has been recently (1996) reexamined in a nice paper by Skopenkov 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 70's in the works of Dufour and Voronin studying the representations of functions on the germs of curves with cusps by the sums of smooth and of holomorphic functions of coordinates.



We choose a point 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 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, and so we continue. We thus get a dynamical system on this curve. We have two involutions of the curve: the vertical involution A and the horizontal involution B and hence we have a mapping T = AB, a diffeomorphism of the circle preserving the orientation.

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

In the resonance case, when there is a periodic orbit, the periodic points are obstacles to the solution of the initial problem, because the alternating sum of 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 elliptical case, then you can formally continue, but you will have the 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 works on small denominators and on Hamiltonian systems and on what is now called KAM theory. And so my attempt to invent something independent was completely unsuccessful!

I came to Kolmogorov with my theorems. "Well, he said. Here is my paper in Doklady '54. I think it will be good if you continue with this problem, try to think on applications to celestial mechanics and rigid body rotation. I am very glad that you have chosen a good problem." But I was completely upset by this, because it was just the opposite of my plan of a complete independence. However, it was an interesting nice problem. A few days later I learned from the Vakhania thesis which was defended at mechmath that this problem was in fact considered before me by Sobolev in a classified work of 1942 on missiles' rotation and oscillations, related to hydrodynamics. Resonances are dangerous there, since they can destroy the tank. This theory is also related to Sobolev's equation which is called so because it was first written by Poincaré in  $1910.^{67}$ 

In this way I have started to learn some mathematics. I have read some other people's works and finally I discovered some papers by Siegel who was a personal friend of Kolmogorov when they stayed in Göttingen in the 1930's. 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 had discovered this Siegel theorem related to the normal forms for the circle rotations due to the system of education in Moscow University which was different from that in America. I think, it followed the German tradition that when you have a result and you wish to publish it, you have first to check the literature and 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. There was no Internet, of course, at that time, but still we were able to find the references, and this is how I discovered that Siegel existed.

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 the shells. This stability problem is very important, because a shell must be very thin, if you want to launch it far. But you cannot, by the architecture of the system, avoid non-convexity. In the convex part, you have good theorems by Cauchy that the metrics 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 the 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, whether you can deform it without deforming the metrics. Only in some particular cases, for example, for the rotationally symmetric torus lying between two parallel planes, the inflexibility has recently been proved, as I was told, 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. Problems of the interior geometry of surfaces in the Euclidean space are in fact closely related to the theory we are now discussing. Many years ago I have 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), but until now this conjecture is neither proved nor disproved.<sup>8</sup>

Returning to the shell stability, people who were constructing those 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

<sup>&</sup>lt;sup>6</sup>Applications of the modern KAM theory to the corresponding hydrodynamical problems have been discovered recently by A. Babin, A. Mahalov and B. Nicolaenko.

<sup>&</sup>lt;sup>7</sup>This system also appears in the study of pseudo-Euclidean billiards, see: B. Khesin, S. Tabachnikov, *Pseudo-Riemannian geodesics and billiards*. Adv. Math. **221** (2009), 1364–1396. (Editor's footnote.)

 $<sup>^{8}</sup>$ The problem was solved positively by the author in January 1998 ("On the problem of realization of a given Gaussian curvature function". Topol. Methods Nonlinear Anal. **11** (1998), 199–206.
define a dynamical system similar to that which lead us to the diffeomorphisms of the circle. This dynamical system is in fact related to the characteristics of the string equation  $\partial^2 f / \partial x \partial y = 0$ . To represent a function as a sum of a function of x and a function of y means to solve the 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 one-sheet hyperboloid you have the Dirichlet problem for a hyperbolic equation. Professor Goldenweiser discovered that the resonances of the dynamical system on the boundary circle, depending on the shape of the hyperboloid, are responsible for the flexibility. This is not a theorem, as far as I know — there are some formal obstacles to inflexibility, but no mathematical proof of flexibility. People who studied these problems were doing real work, they were really constructing the 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 resonances, then they are flexible in your hands — but I have not seen any mathematical proof of this.

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 of the "resonances materialization", providing the topological reasons of the series divergence in perturbation theory.

Consider an analytic diffeomorphism of a circle onto itself (defined by a holomorphic mapping of the neighbouring annulus onto another neighbouring 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 neighbourhoods of the boundary of the maximal annulus. One is even tempted to conjecture that the points of such orbits exist in any neighbourhood 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 have been initially formulated in 1958, or for the generic mappings. In 1958, I have also formulated similar conjectures for the boundary of the Siegel disk (centered at a fixed point).

In the case where there exists no analytical 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 neighbourhood of the invariant circle (of the fixed point). 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 with some details by Halmos, Rokhlin and others). So the "physical genericity" notion should be different from what the mathematicians suggested.

The topological definitions (using the "Baire second category sets" etc) have the following defect. A phenomenon happenning with positive probability (in the sense of the measure of the set of the corresponding parameters values) might be neglectable from the point of view of the 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 neglectable.

But the ergodicity of the diffeomorfism happens with probability 99%, if b is small, while the "generic" behaviour is highly unprobable!

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

To overcome these difficulties I have then proposed to call generic those events, which happen when the parameter of the topologically generic finite-dimensional family of systems belongs to a positive measure set in the finite-dimensional parameter space.

For years I was thinking that this "physical genericity" definition was introduced by Kolmogorov in his Amsterdam talk. However recently Yu. S. Ilyashenko has explained me that Kolmogorov used a rather dual definition and that I was perhaps the first one to introduce (in 1959) the physical genericity notion described above (and now called "prevalence").

I was trying to apply this philosophy to many problems, for instance to the study of the chaotical dynamics of the area-preserving mapping in the neighbourhood of a hyperbolic fixed point whose separatrices have a homoclinical transversal intersection. My guess was that the positiveness of the measure of the "Smale's horseshoe" type Cantor set on which the dynamics is chaotical should be a physically generic event. As far as I know, this conjecture is still neither proved nor disproved. Its topological version (not referring to measure) was proved by V.M.Alexeev in a very general situation.

I was still an undergraduate student. Once Gelfand invited me to talk on the circle rotations and when I explained him my theorems, he said that they could be applied to what he was working on. He was working with M. L. Zeitlin on the mathematical model of the heart beat. In the heart, you have the 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 Gelfand and Zeitlin 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 the applications of the theory of resonances and structural stability of the mappings of a circle into itself and on small denominators, to the heart beat problem. The paper was sent to Vinogradov's journal "Izvestiva of the Russian Academy of Sciences" for publication, but was rejected. Kolmogorov told me: "you should delete the Gelfand theory part".<sup>9</sup> 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 better concentrate on the three body problem", he told me. This was the only mathematical advice I ever got from Kolmogorov.<sup>10</sup> When I deleted the part on the Gelfand 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 have deleted a paragraph about the influence of a small noise on the circle diffeomorphism invariant measure. Today these problems are included into the general Morse-Witten theory (but the discrete time case I was studying seems to remain still unsettled in the modern theory). Kolmogorov did not approve my naïve approach to the theory of asymptotics of solutions of the (discrete time) Focker-Planck equation in the small diffusion limit — which was of course his kingdom.

The deleted heart beat part of the paper lied 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 Gelfand 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!

This was the story of how my works in what is now called KAM theory have started. Later I worked on the many body problem, following Kolmogorov's suggestion. Reading the "Méthodes nouvelles de la Mécanique Céleste" of Poincaré and having discussions with V. M. Alekseev during our weekly common "windows" (breaks between two classes) at Moscow University, I realized that 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  $2\pi$ . If the linearized mapping is a non-resonant rotation, Birkhoff was able to

<sup>&</sup>lt;sup>9</sup>Vinogradov was a pathological antisemite, and that was the reason that the paper, that mentioned the Gelfand–Zeitlin theory, was rejected. Arnold does not say this explicitly, but this conclusion is clear to those who are familiar with the context. (Editor's footnote.)

<sup>&</sup>lt;sup>10</sup>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 (thinking on the relations to the superposition problem). As a result, I started to study differential topology from Novikov, Fuchs and Rokhlin — and even served as an opponent for the candidate thesis of S. P. Novikov, the supergenial topologist and the glory of Russian mathematics, on the differential structures on the products of spheres.

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  $2\pi$  — 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 the 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 — *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 the analytical mappings) to the case of weak resonances, while I had formulated the result only in the non-resonant 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 the predecessors.

The second main difficulty of the planetary motion problem was the so called "proper degeneration" (the terminology was introduced, I guess, by M. Born). The point is that some of the frequencies of the quasiperiodic motion of the perturbed system might be small together with the perturbation parameter. The simplest case is the adiabatic invariants theory. Consider, for instance, the motion of a charged particle along a surface under the influence of a strong magnetic field, orthogonal to the surface. Mathematically it is 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 adiabatic invariants theory) 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 Larmor circle center 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 1960) gave a positive answer to this question, also providing many other physically important results on the infinite time behaviour 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 that I send the resulting paper to ZETP, the main Russian physical journal.

A few weeks later M. A. Leontovich (who was, as far as I remember, the vicechairman of the editorial board) invited me to his home, near the Atomic Energy (now Kurchatov) Institute to discuss the paper. Leontovich, heading 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 usually, with buckwheat porridge and, calling me, as usually, "Dimka" (he used this nickname until his death some 20 years later), Leontovich explained me the reasons why the paper cannot be published in ZETP:

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 non-physical 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.<sup>11</sup>

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 the 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.

I took the paper back from ZETP 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 the conference on theoretical astronomy held in Moscow on 20–25 November 1961. The conference's main topic was the artificial satellite motion. I was delighted to meet there and make friends with M. L. Lidov whose students A. I. Neishtadt and M. L. Zieglin later made profound contributions to perturbation theory, averaging, adiabatic invariants, Hamiltonian chaos and materialization of resonances. The resulting theories are well known, and I shall only mention one small relevant detail.

Before I turn to this small detail, let me remark that now you have almost the whole picture of all my mathematical subjects. They all are starting from this problem of superpositions and you now see how they are connected. There is one more topic, hydrodynamics and hydrodynamic stability, but this is also related to the same origin.

 $<sup>^{11}{\</sup>rm Rumours}$  later reached me that the paper had been reviewed by Landau, but I do not know if this was indeed the fact.

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 the continuous systems, in particular, to hydrodynamics. Kolmogorov, of course, was also a classic of hydrodynamics and he had a seminar at that time (1958-1959) called "Seminar on dynamical systems and hydrodynamics", where the celebrated work by Sinai and Meshalkin was done on Kolmogorov flows instability and continuous fractions, where the Kolmogorov–Sinai entropy was invented and so on.

In 1961 Smale came to Moscow. He was the first foreigner I met in my life. We were discussing 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 were discussing structural stability and he formulated the conjecture 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. Describing this proof at my Dynamical Systems seminar, 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. Next week Anosov reported his proof of it, but my proofs were wrong, since I was using too many derivatives of the invariant foliation.

And this is why I have never tried again to prove collective theorems. This happened in 1961–63, and since that time I was trying to find applications of this philosophy of structural stability. My first idea was to think on the hard balls model of statistical mechanics. I speculated that such systems might be considered as the limit case of geodesic flows on negatively curved manifolds (the curvature being concentrated on the collisions hypersurface). I had never proved anything in this direction, but I explained this idea to the greatest expert in dynamical systems and ergodic theory 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 V. 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 project already in 1961–1962 with V. Yudovich and O. Ladyzhenskaya.

The idea was that because of the high sensitiveness of the flows on surfaces of negative curvature, the positive Lyapunov exponents are stable. The Euler and Navier-Stokes equations contain many "parameters": the domains and the exterior forces. One might hope — had I conjectured — to 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 I have tried, and in 1964 I made some numerical experiments (with the help of N. Vvedenskaya) on a model with 6 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 I have observed was a 3dimensional torus in 6-dimensional space — a scenario predicted by Landau. But I was certain that with more work you might find the positive Lyapunov exponents, perhaps even the geodesic flow on a surface of negative curvature. This was the reason of my paper of 1966 on the differential geometry of infinite dimensional Lie groups, on the diffeomorphism groups which is the configuration space in hydrodynamics. I have calculated the curvature of this group<sup>12</sup> and I even used it to show that the weather prediction is impossible for periods longer than 2 weeks. In a month you lose 3 digits in the prediction, just because of the curvature. 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 were the byproduct of the works on the Hilbert problem.

Trying to study the slow mixing in Hamilton dynamics, I have introduced the "interval exchange" model (as the simplest discrete time description of the events in the pseudo-periodic system whose Hamilton equations are defined by a closed but not exact 1-form on a surface of genus 2).

This intervals exchange model is so natural that I was always amazed not to find it in the works on ergodic theory prior to my 1963 paper where it was introduced. I have returned to pseudo-periodic topology many times since 1963. Pseudo-periodic functions are sums of linear functions and periodic functions, like  $f = ax + by + \sin(x + y)$ . Pseudo-periodic manifolds are those defined by pseudoperiodic 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-the-art in pseudo-periodic topology might be understood from the forthcoming book by A. Zorich, S. Pajitnov and D. Panov (prepared in 1997 for the Advances in Soviet Mathematics AMS series). Studying the intervals exchange, Zorich discovered (by computer experimentation) astonishing new laws of correlation decay in such systems. In a recent joint work with Kontsevich they 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 intervals exchange model has thus returned to the non-exact Hamiltonians pseudo-periodic topology on higher genus surfaces, which was the initial motivation for the introduction of this model in 1963.

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

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

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

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

— A. Kushnirenko (slow mixing, structural stability of analytical semisimple groups actions, later — Newton polyhedra and fewnomials conjecture),

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

 $<sup>^{12}</sup>$ A few years earlier I had translated into Russian Milnor's wonderful "Morse theory". 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 of the hydrodynamical paper.

I turn now to the KAM theory. This theory is called KAM, or Kolmogorov– Arnold–Moser, and people say that there is even a KAM theorem. I was never able to understand what theorem is it. 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 analytical. What I have contributed was the study of some degenerate cases — when one of the frequencies is 0 in the non-perturbed system or when you have the vicinity 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 the systems with many degrees of freedom, close to integrable, — later called "Arnold diffusion" by the physicists.

In 1962 Moser has extended Kolmogorov's theorem to the case of smooth functions.<sup>13</sup> In the first papers of Moser the number of derivatives was enormous. 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. For Kolmogorov, this was like a complete change of philosophy, he told me, because he was expecting and even claiming in his Amsterdam talk that the result should be wrong even in  $C^{\infty}$  and one needs analyticity or something close to it, like the Gervais condition.

Moser criticized 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 the years still remaining 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.

Thanks for your attention.

**Question** (*J. Milnor*). You often told us about important mathematical work in Russia we did not know about and you gave another example today. I wonder if you can explain us how do you 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 new for me, I usually start with the German *Encyclopædia of Mathematical Sciences* edited by 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 had been organized — it is full of information. Then, I usually consult the collected works of Felix Klein and Poincaré. In Klein's *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert* there's a lot of information on whatever has happened in 19th century and before.

The other books by Klein are also extremely informative. For instance, in Klein you can find the article by Emil Artin on continuous fractions and on braid

 $<sup>^{13}</sup>$ It is interesting to note that, when Moser's papers appeared, some American mathematicians began to publish their papers that "extended the Moser theorem to the case of analytical functions". J. Moser himself has never supported these attempts to attribute Kolmogorov's results to him.

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 to Radon, the Levi-Civita connection can be defined by the adiabatic invariants theory. 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 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 find this in Klein.

Then from the 40's starts the Mathematical Reviews and the Zentralblatt and then later the Russian *Referativnyi Zhurnal Matematika* and then it's more or less OK. 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 mechanics. There were the same people doing mathematics, mechanics and physics. For example, in Kolmogorov's collected works there is a paper by Kolmogorov and Leontovich, who was a famous physicist, on the neighbourhood of a Brownian trajectory. This is a paper of 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 neighbours. Many times I have used the opportunity to pose silly questions to Fuchs, Novikov, Sinai, Anosov, V. M. Alekseev, 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 me a graduate student of yours still interested in number theory, to explain me what is known?" "How naïve you are, he replied, you 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 of mathematical information, Russian mathematicians have tried to cover most of the present day mathematics in the more than one hundred volumes of the *Encyclopædia of mathematical sciences*, several dozens of which have already been translated into English. The idea of this collection was to represent the living mathematics as an experimental science, as a part of physics rather than the systematic study of corollaries of the arbitrary sets of axioms, as Hilbert and Bourbaki 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 the catastrophe theory in vol. 5). Unfortunately, in the Library of Congress, and hence in all the USA libraries, the volumes of the Encyclopædia of Mathematical Sciences are scattered according to the author/subject alphabetical order, which makes its use as an encyclopædia extremely difficult. I have seen, however, all the collection arranged on one shelf in some European universities, for example in France.

Of course, in spite of all these precautions, you may discover too late that your result was known many years ago. It happened to me to rediscover the results of many mathematicians. And I am especially grateful to Prof. Milnor who explained me the relation to the works of G. Tyurina of my ADE paper of 1972 dedicated to her memory.

### CHAPTER 3

## Recollections

## Jürgen Moser

I would like to use this opportunity to describe to the readers the early development of the KAM theory as I experienced it. Some 40 years ago, when I was at MIT, the Mathematical Reviews asked me to review the famous lecture of Kolmogorov, held at the International Congress 1954 in Amsterdam. This is how I first learned about this work and I was very excited about it. At that time there were few mathematicians interested in Hamiltonian mechanics, and it was encouraging to me to find others working in this field. The significance of this fundamental work was indeed apparent to me, since I had been working on the stability problem of elliptic fixed points of area-preserving mappings, a problem C.L. Siegel had urged me to pursue. Naturally, I was disappointed that neither Kolmogorov's address nor his Doklady announcement contained a proof. Therefore I wrote to Kolmogorov asking for the argument. I never received a reply, and I had to write my review not knowing whether this theorem was actually true. I never believed in proof "by authority"! I also had no doubt that Kolmogorov knew how to prove his claims, but that did not help me!

From this point on I concentrated my efforts on trying to find a proof for this theorem, at least for the special case of area-preserving twist maps. This took me several years, during which I moved from MIT to the Courant Institute where I finally succeeded in proving the invariant curve theorem in 1961. I was, of course, very happy to know that the theorem was indeed correct, and I presented my result in the Analysis seminar at the Courant Institute.

Incidentally, I saw it as a shortcoming that I could not handle the real analytic case! This was due to the fact that I considered a wider class of annulus mappings satisfying a certain curve-intersection property (i.e. not necessarily area-preserving mappings), and for this class of mappings the proof of the analyticity of the invariant curve eluded me at that time. So I restricted my argument to the smooth case. I needed some ridiculous number of derivatives (16 or 17) for the mapping—an assumption which obviously had nothing to do with the problem. Therefore I decided to forget about optimizing this number and to assume that the mapping had the even more ridiculous number of 333 derivatives, whereby I could shorten the exposition a bit. This silly choice was sometimes taken seriously! Anyhow my proof was presented in the smooth category, and it came as a surprise to me that the very thing I had considered a shortcoming was subsequently recognized as its main merit: Ever since, my proof was described as the extension of Kolmogorov's from the analytic to the smooth category! Logically this is correct. But since the

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#### JÜRGEN MOSER

argument was not available to me how could I extend it! I was, of course, very happy to know that, at least in the special case, the theorem of Kolmogorov was true, and to understand what he must have had in mind. The generalization to higher dimensions required some more work, but the principle was rather clear.

My paper appeared in 1962 in the—nowadays—little known journal of the Academy of Göttingen, and in the same year I presented it at the International Congress in Stockholm. Kolmogorov was in the audience. The proof of Kolmogorov's theorem was published a year later by Arnold; it had a nice touch: Arnold presented it to Kolmogorov on his 60th birthday. (By the way, when Kolmogorov wrote his paper he was 51 years old, not 60 as Arnold implied in his lecture.)

In September 1962 I decided to visit Moscow. I had the great fortune to meet all the people involved in the above story; I brought my manuscript along, which Arnold and I discussed at length. It was a most stimulating visit in a most harmonious and friendly atmosphere. I learned a great deal about other developments, such as Sinai's then ongoing work on the Gibbs model, Anosov's work on what is now called Anosov systems, and much more. Most memorable for me was the walk with Kolmogorov where he showed me icons at various museums and churches in Moscow; I had expressed an interest in this topic, having seen many icons in Novgorod a week before. Our mathematical discussions were not about his theorem, but his ideas about turbulence and complexity theory, e.g. how many binary operations are required in the multiplication of two numbers.

I want to add some remarks about the unlikely events that sometimes have to come together for the completion of a proof. For me there were at least 3 ingredients that played a crucial role in my proof: The first was, of course, Kolmogorov's address. Though I was not privy to his proof—or maybe just because of this reason—this address, and his claim that the assertion was true, gave me a tremendous impetus to look at it on my own. The second one was the insight I got from the work on the regularity theory of elliptic partial differential equations, initiated by DeGiorgi, on which I worked in 1960. In this work, one is faced with a set of a-priori estimates involving small denominators. Here one uses an appropriate iteration technique to get rid of the disturbing effect of these denominators. The expert will recognize the analogy to the Newton iteration as applied in Hamiltonian mechanics to beat the small divisors. The third contributing factor for me was the work of Nash on "isometric embedding". In his approach Nash was faced with a "loss of derivatives" which led him to his novel technique including his smoothing method. I was familiar with these ideas and could use them for my purposes. Incidentally, I prefer to refer to the KAM "technique", instead of the KAM "theory"; but what can one do about traditions! We know now that in the isometric embedding theorem the loss of derivatives can be avoided entirely, as was shown by Matthias Günther (see his lecture at the ICM 1990 in Kyoto). This allowed Günther to give a simpler proof, avoiding the Nash technique, and to obtain sharper results. It is tempting to contemplate which way things would have developed had Nash recognized this possibility! His innovative method would not have been necessary, he may not have invented it and we may ask whether we would still think that the invariant tori can be constructed only in the real analytic category? Or would somebody else have hit upon Nash's technique?

### RECOLLECTIONS

My point is that the advances in mathematics require the interaction of many mathematicans, from different schools and various countries, as well tools from different fields. The last point is amply clear from Arnold's lecture.

### CHAPTER 4

## Polymathematics: Is Mathematics a Single Science or a Set of Arts?

## V. I. Arnold

The shaft becomes too deep ... and if new veins would not be discovered, places for geometry in the Academy will become what already are the Arabic chairs at the Universities.

Lagrange to D'Alembert, 1781

All mathematics is divided into three parts: cryptography (paid for by CIA, KGB and the like), hydrodynamics (supported by manufacturers of atomic submarines) and celestial mechanics (financed by military and other institutions dealing with missiles, such as NASA.).

Cryptography has generated number theory, algebraic geometry over finite fields, algebra<sup>1</sup>, combinatorics and computers.

Hydrodynamics procreated complex analysis, partial differential equations, Lie groups and algebra theory, cohomology theory and scientific computing.

Celestial mechanics is the origin of dynamical systems, linear algebra, topology, variational calculus and symplectic geometry.

The existence of mysterious relations between all these different domains is the most striking and delightful feature of mathematics (having no rational explanation).

The experience of past centuries shows that the development of mathematics was due not to technical progress (consuming most of the efforts of mathematicians at any given moment), but rather to discoveries of unexpected interrelations between different domains (which were made possible by these efforts). The flood of precious technical reports on the present state of different mathematical domains reminds us of trench warfare. Descriptions of the front line with its serpentine shapes and daily fluctuations are of course highly important for the battle participants. But the pernicious character of diverging modes of thought (to which the growing specialisation of mathematicians and the fragmentation of mathematics into small domains leads) becomes evident when one tries to understand the development of mathematics in the past with all its meanderings.

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 $<sup>^1\</sup>mathrm{The}$  creator of modern algebra, Viète, was the cryptographer of King Henry IV of France.

#### V. I. ARNOLD

Sylvester(1876) described as an astonishing intellectual phenomenon the fact that general statements are simpler than their particular cases. The anti-Bourbakist conclusion that he drew from this observation is even more striking. According to Sylvester, a mathematical idea should not be petrified in a formalised axiomatic setting, but should be considered instead as flowing as a river. One should always be ready to change the axioms, preserving the informal idea.

Consider for instance the idea of a number. It is impossible to discover quaternions by trying to generalise real, rational, complex or algebraic number fields.

The possibility of such *informal generalisation* of all mathematics, for which we have no ready axioms, seems to me a most appealing dream.

The informal complexification, quaternionization, symplectization, contactization etc., described below, act not on such small things as points, functions, varieties, categories or functors, but on the whole of mathematics.

I have successfully used these ideas many times as a method to guess new results. I hope therefore that in the future this method of the "multiplication" of mathematics will become as standard as is now the transition from finite dimensional linear algebra to the theory of integral equations and to functional analysis.

Perhaps the simplest example of this multiplication paradigm is provided by Killing – Coxeter theory of the reflection groups. Linear algebra is essentially the theory of the special root systems  $A_k$ . The basic facts of linear algebra (like eigenvalues and Jordan blocks theory) can be reformulated in terms of roots, making the statements meaningful for other systems of roots. These new statements miraculously happen to be correct (when suitably modified). The theories of the root systems  $B_k$ ,  $C_k$ ,  $D_k$  (corresponding to Euclidean and symplectic spaces geometry) are from this point of view the sisters rather than the daughters of the usual vector space geometry (even though they are, of course, the geometries of the usual vector spaces endowed with additional structures).

By the way, many of the results of these theories are in fact also true not only for the exceptional crystallographic groups  $(E_6, E_7, E_8, F_4, G_2)$  but also for the noncrystallographic Euclidean reflection groups  $(I_2(p), H_3, H_4)$ . For example, starting from the ideas of O. Scherbak (1985), A.B. Givental (1988) discovered, in the theory of Lagrange and Legendre projections in symplectic and contact geometry, a geometrical problem whose solutions are in a natural one-to-one correspondence with the Euclidean reflection groups (and not just with the crystallographic ones as in my preceding theory). This is the problem of classification of the simple projections (having no continuous invariants) of the (virtually singular) Lagrange and Legendre subvarieties.

The main applications of the idea of mathematical multiplication are not to be found in finite dimensional algebra but rather in infinite dimensional calculus, where the Killing classification of simple Lie algebras is replaced by the Cartan classification of simple Lie pseudo-groups.

For example, the symplectization idea suggests that all notions and results of differential geometry and topology should have symplectic versions – symplectic geometry and symplectic topology.

In some cases these versions are quite obvious. The ordinary vector fields correspond to Hamiltonian ones, their Poisson brackets algebra to the Poisson brackets of the Hamilton functions, the strings should move from the configuration space to the phase space and so on. In other cases this generalisation is less evident. For instance, the submanifolds of ordinary geometry correspond to the Lagrangian submanifolds of symplectic manifolds (A. Weinstein's Principle).

Trying to symplectize the Euler – Poincaré theorem on the sum of the indices of the singular points of vector fields, I was led in 1965 to the conclusion that symplectization should be an astonishing extension of Morse theory. I formulated "Arnold's conjectures" on the fixed points of symplectomorphisms, on the intersections of Lagrangian submanifolds and on the linkings of Legendrian ones. The simplest of these conjectures states that the number of fixed points of an area preserving diffeomorphism of a two-torus to itself, "preserving the mass centre", has at least 4 fixed points (taking multiplicities into account).

In the more general case of a compact symplectic manifold, the diffeomorphism should be the time-one map of a flow defined by a time-dependent Hamilton function, and the number of fixed points is minorated by the minimal number of critical points of a function on the manifold (the fixed and critical points being counted either both with their multiplicities, or in both cases geometrically).

These conjectures, generalising the "last theorem" of Poincaré on the circular annulus mapping, were later studied in a series of brilliant works of many authors (Ya. Eliashberg, P. Rabinowitz, C. Conley and E. Zehnder, M. Chaperon, J.-C. Sikorav, F. Laudenbach, Yu. Chekanov, A. Floer, H. Hofer, C. Viterbo, A. Weinstein, D. Salomon, A. Givental, M. Gromov, and others). *Quantum and Floer cohomologies* and Gromov's *pseudoholomorphic curves* theories are well-known byproducts of this development. *Lagrange intersection theory* is a far-reaching extension of Morse theory, replacing functions by such genuinely multivalued functions as  $\sqrt{x}$ . Another extension replaces functions by closed 1-forms and Morse theory by Novikov's complex. The corresponding version of Lagrange submanifolds intersection theory is due to J.-C. Sikorav.

I have heard that my initial conjectures, which triggered all these theories, are now proved (by Fukaya, Ono, Salomon, Ruan and others). Unfortunately I was unable to understand the technical details of these proofs. Kontsevich was unable to explain them to me, though all the proofs are based on his theory of stable mappings of curves. As far as I understand, all these proofs minorate the number of fixed points by the sums of the Betti numbers, while I conjectured that it is minorated by the Morse number (or by the minimal number of geometrically different critical points if the fixed points are counted geometrically).

In my dreams of the 60s, the symplectomorphisms fixed points number minoration was followed by a similar study of the symplectic correspondences, which are not graphs of symplectomorphisms. My idea of symplectic and contact topology was from the very beginning different from that of Gromov and Eliashberg, who were the first to explore, in the 70s, the new domains discovered by symplectization and contactization methods. In their opinion, the symplectic (or contact) topology objects should have the "symplectic (or contact) homeomorphism invariance" property: they should persist under  $C^0$ -small symplectic (or contact) diffeomorphisms.

To me the word "topology" with any adjective is the study of the discrete invariants of the continuous objects of the corresponding branch of geometry, be they homeomorphism invariant or not. Thus, I include into projective topology the Möbius theorem on the three inflection points of a projective line deformation (claiming that a noncontractible circle, embedded generically into the real projective plane, has at least three inflection points, the dual curve having at least three cusps). A recent conjecture in projective topology (due to F. Aicardi and D. Panov) extends the Möbius theorem to the generic surfaces in  $\mathbb{R}P^3$ , smoothly close to a plane. The conjecture claims that the parabolic line of such a surface is tangent to the asymptotic direction of the surface at least at 6 points (generating 6 swallowtails of the dual surface) and that there are at least 4 parabolic lines if there are only 6 tangency points.

Recent spectacular achievements of *contact topology* include the creation by Yu. Chekanov (and also by Ya. Eliashberg and H. Hofer) of the *contact homology* theory. One may hope that the further development of these ideas would provide a proof of the conjecture on the necessity of four cusps of any wave front eversion in the plane. This conjecture extends to the case of genuine multivalued functions (of  $\sqrt{x}$  type) the *Sturm theorem on periodic functions*, which itself extends the Morse theory of critical points to higher derivatives. This theorem claims that the number of zeros of a function on a circle is minorated by the number of zeros of its first nonvanishing Fourier harmonic (providing a confirmation of the general topological economy principle of algebraic objects).

The relation of all this domain to cyclic homology and characteristic classes, discovered by M. Kazarian, provides some hope that Morse theory can be extended to higher derivatives (or to the more general case of nonholonomic constraints). This extension should contain the (genuine) multivalued functions Morse theory – the theory of Lagrange intersections and Legendre linking (describing causality in relativistic physics, according to R. Low and R. Penrose) as the first derivative case. Symplectic and contact topologies, created as the symplectization and contactization of differential topology, are today well settled domains of mathematics.

Another attempt to multiply mathematical results is the *complexification and* quaternionization dream. The first spectacular success of this idea was the proof of Gudkov's conjecture in real algebraic geometry. One of the most fundamental problems of mathematics is the problem of the topological structures of the real curves <sup>2</sup> defined by algebraic equations of degree n. This problem was solved for n = 2 by the Ancients (ellipses, hyperbolas, parabolas,...). The cases n = 3 or 4 were settled by Descartes and Newton. Hilbert included the case n = 6 in his 16th problem. For n = 8, the answer is still unknown.

These *real problems* are too difficult for modern algebraic geometers and for present day computers.

According to Harnack, the number of connected components of a curve of degree n in the real projective plane does not exceed g + 1, where g = (n - 1)(n - 2)/2 is the genus of the curve. Curves with g + 1 components do exist; they were called M-curves (M for "maximal") by I.G. Petrovsky. Hilbert announced that he had proved that only two arrangements of the 11 ovals of an M-curve of degree 6 were possible: only one oval contains other ovals inside its disc, the number of the interior ovals being either one or nine.

In 1970, a Nizhni Novgorod mathematician, D. Gudkov, submitted his thesis proving that Hilbert was wrong. He proved that there exists one (and only one)

<sup>&</sup>lt;sup>2</sup>The Russian way to formulate problems is to mention the first nontrivial case (in a way that no one would be able to simplify it). The French way is to formulate it in the most general form making impossible any further generalisation.

more possible arrangement, for which the number of interior ovals is five. I.G. Petrovsky asked me to check this paper, which indeed contradicts both Hilbert's statement and a wrong proof of that statement by Gudkov himself in a previous paper.

Mistakes are an important and instructive part of mathematics, perhaps as important a part as proofs. *Proofs are to mathematics what spelling (or even calligraphy) is to poetry*. Mathematical works consist of proofs, just as poems consist of characters.

Leibniz started his calculus studies from the formula d(uv) = dudv. Cauchy, the  $\epsilon - \delta$  inventor, proved in his calculus course the continuity of the limit of a (nonuniformly) convergent series of continued functions. Lagrange's mistake in linear ODEs theory hampered the development of linear algebra and of Jordan's form theory. The story of the Poincaré conjecture started from his confusion of homotopy with homology. His New methods of celestial mechanics are the by-product of attempts to prove a wrong statement (the by-product – the creation of dynamical systems theory – being by far more important than this wrong statement "solving" a prize problem). Burnside's celebrated theorem on the groups of order  $p^a q^b$ was first formulated by him wrongly. Leray told me that his works on the hyperbolic PDEs were motivated by the remarkable paper of Petrovsky, who wrongly used the triviality of the cotangent bundle of every sphere (the paper was later modernised by Atiyah, Bott and Gårding). Kolmogorov's initial definition of the entropy of a dynamical system was wrong, as well as Pontriagin's and Rokhlin's calculations of homotopy groups of spheres. I would be able to provide dozens of more recent examples of mistakes in celebrated papers if I did not fear for my life. I shall only mention a wrong symplectic reduction theorem in the first edition of the Mathematical Methods of Classical Mechanics (appendix 5,B).

Hilbert's mistake, discovered by Gudkov, has led to the foundation of modern real algebraic geometry. Trying to understand Gudkov's very complicated paper, I observed that in all his examples of M-curves of degree 2k, bounding orientable surfaces B, the following "Gudkov congruence" for the Euler characteristics holds,

$$\chi(B) = k^2 \mod 8.$$

Knowing the importance of congruences mod 8 in the topology of 4-manifolds, I decided to replace the real surface B by its complexification of real dimension four. The problem arose: how to complexify a manifold with boundary? This is a standard difficulty: complexification is an informal operation for which there are no axioms; we should try to guess.

After several attempts I came to the following conclusion. Consider the real manifold with boundary, defined by the inequality  $f(x) \ge 0$ , where f is a real function. To complexify it, replace the inequality by the equality  $f(x) = y^2$ . We guess that the complexification of the real surface B with boundary f = 0 is the two-fold covering of the complement in  $\mathbb{C}P^2$  of the Riemannian surface f = 0 (which complexifies the boundary curve), ramified along the Riemannian surface.

Applying the 4-dimensional topology result to this 4-manifold, I was able to prove the Gudkov congruence mod 4 (Rokhlin later extended it to the mod 8 case).

From this moment on, real algebraic geometry entered modern mathematics. In a series of brilliant works by Kharlamov, Nikulin, Viro, Shustin, Polotovski, Khovanski, Orevkov and others, substantial progress was achieved. The interrelations of this domain to the "geometry of formulae" (that is to the toric varieties

(1)	$\mathbb{R}$	$\mathbb{C}$	IHI
(2)	Morse theory handles attachment	Picard-Lefschetz theory Dehn twist	?
(3)	$\pi_0(\mathbb{R}\setminus 0) = \mathbb{Z}_2$	$\pi_1(\mathbb{C}\setminus 0) = \mathbb{Z}$	$\pi_3(\mathbb{H}\setminus 0) = \mathbb{Z} ?$
(4)	$\mathbb{R}P^n$	$\mathbb{C}P^n$	$\mathbb{H}P^n$
(5)	$\mathbb{R}P^1 = S^1$	$\mathbb{C}P^1 = S^2$	$\mathbb{H}P^1 = S^4$
(6)	$\mathbb{R}P^1/\mathrm{Aut}\mathbb{R}=S^1$	$\mathbb{C}P^2/\mathrm{Aut}\mathbb{C}=S^4$	$\frac{\mathbb{H}P^4/\mathrm{Aut}\mathbb{H}}{\mathrm{Conj}} = S^{13}$
(7)	Quadratic forms	Hermitian forms	Hyperhermitian forms
(8)	Von Neumann – Wigner eigenvalues repulsion	Quantum Hall effect and Berry phase	?
(9)	$ \begin{array}{c} \text{M\"obius } S^0 \text{ bundle} \\ S^1 \to S^1 \end{array} $	$\begin{array}{c} \operatorname{Hopf} S^1 \text{ bundle} \\ S^3 \to S^2 \end{array}$	$\begin{array}{c} \operatorname{Hopf} S^3 \text{ bundle} \\ S^7 \to S^4 \end{array}$
(10)	Monodromy of a covering	Curvature of a connection	Hypercurvature of a hyperconnection?
(11)	w	с	p
(12)	O, SO	U, SU	Sp, ?
(13)	Tetrahedron	Octahedron	Icosahedron
(14)	(4, 4, 6)	(6, 8, 12)	(12, 20, 30)
(15)	$x^2 + y^3 + z^4$	$x^2 + y^3 + yz^3$	$x^2 + y^3 + z^5$
(16)	$x^3 + y^3 + z^3$	$x^2 + y^4 + z^4$	$x^2 + y^3 + z^6$
(17)	$(\pi/3,\pi/3,\pi/3)$	$(\pi/2,\pi/4,\pi/4)$	$(\pi/2,\pi/3,\pi/6)$
(18)	$A_3 $ $\frown $	$B_3$ $\longrightarrow$	$H_3 $ $\sim 5 $ $\sim \sim \circ$
(19)	2(1+3+3+5) = 24	2(1+5+7+11) = 48	2(1 + 11 + 19 + 29) = 120
(20)	(2, 4, 4, 6)	(2, 6, 8, 12)	(2, 12, 20, 30)
(21)	$D_4$	$F_4 $ $\frown$	$H_4 \circ 5 \circ - \circ \circ \circ$
(22)		$E_7 $ $\sim \sim $	
(23)	$\mathbb{C}[t]^{oldsymbol{\circ}}$	$\mathbb{C}[t, t^{-1}]$	$\mathbb{C}[t, t^{-1}, (1-t)^{-1}]$
(24)	Numbers	Trigonometric numbers	Elliptic numbers
(25)	Н	K	Ell

and Newton polyhedra theory), to the "fewnomials" of Kushnirenko – Sevastianov – Khovanski and even to mathematical logic have been discovered.

Applying these ideas, I once suggested to I. M. Gelfand that they might simplify the complicated formulae of representation theory and hypergeometric functions theory: one should express their coefficients in terms of convex polyhedra geometry. Gelfand (with his collaborators) immediately used this suggestion, finding new brilliant applications. He has always stressed (quite correctly) that *mathematicians never appreciate new ideas*, only the last step to the summit counts in this mountainclimbing.<sup>3</sup>

I shall now list some complexifications and quaternionizations of different mathematical objects. The *trinities* (real version, complex version, quaternionic versions) are listed below as the lines of a big informal commutative diagram, whose

 $<sup>^{3}</sup>$ M. M. Postnikov formulated an even more radical statement: *science never accepts new ideas, it fights against them.* Most scientists at any given moment are working on horseshoes and naturally do react negatively against limousines. See the curious attempt of S.-T. Yau to fight against Givental's theory in the present volume.

verticals are (mostly nontrivial) operations, transforming one trinity into another. Some of these operations are described in the comments below.

 $(2) \rightarrow (3)$ : Morse theory describes the modification of a real level hypersurface of a smooth function, due to the jump of the noncritical value from one of the two connected components of the noncritical values set to the other. Picard – Lefschetz theory describes the Dehn twist of a complex level hypersurface of a holomorphic function, due to the motion of the noncritical value around critical one.

 $(5) \rightarrow (6)$ : I used the diffeomorphism  $\mathbb{C}P^2/\operatorname{Conj} \approx S^4$  in my 1971 paper on real algebraic geometry as a well-known fact. But Rokhlin told me that the proof of this fact, known to Pontriagin in the 30s, had never been published. I therefore asked experts (including Kuiper, who was visiting Moscow – which I was unable to leave – for IMU business) whether they were aware of this fact, and later Kuiper and Massey published their proofs.

My original (1971) proof, based on the *theory of hyperbolic PDEs*, provides also the *real and the quaternionic versions of Pontriagin's theorem*: the proofs of the three versions are identical.

 $(6) \rightarrow (7)$ : Hermitian (hyperhermitian) quadratic forms in complex (quaternionic) vector spaces are *real quadratic forms*, invariant under the action of multiplication of vectors by complex numbers (quaternions) of norm 1. This definition is missing in algebra textbooks.

The proof of (6) is based on the hyperbolicity of the cone of degenerate real quadratic forms. The cones of degenerate Hermitian (hyperhermitian) forms are also hyperbolic, providing such generalisations of (6), as

$$S^{11}/U(2) = \mathbb{C}P^5/SU(2) = \mathbb{H}P^2/S^1 = S^7, \quad S^{23}/Sp(2) = S^{13}.$$

The three hyperbolic cones (corresponding to the real quadratic, Hermitian and hyperhermitian forms) are in fact *universal* varieties, providing the "Schur index" of representation theory.

 $(7) \rightarrow (8)$ : The Wigner – von Neumann theorem of the eigenvalues repulsion is based on the fact that the codimension of the variety of quadratic forms with multiple eigenvalues (in the space of the quadratic forms in an Euclidean space) is equal to 2.

In my 1972 paper "Modes and quasimodes" I have studied the monodromy of the *eigenvectors fibration* over the complement of this codimension-two variety as well as the Hermitian case (where the codimension of the variety of the forms having a multiple eigenvalue is equal to 3).

As S.P. Novikow later explained to me, the complexification of this study implies the topological theory of the (integer) quantum Hall effect. I had also missed in 1972 the Berry phase theory, describing the natural adiabatic connection of the eigenvectors fibration in the Hermitian case.

The present situation of the hyperhermitian case is in a sense similar to that of the Hermitian case in 1972. The mathematical theory is ready, but its physical applications are still to be named. These applications should be interesting: the complexification of the monodromy being the connection form, its quaternionization (which is also the complexification of the connection ) should provide a 4-form, measuring the dependence of the complex connection on the complex structure.

 $(8) \rightarrow (9)$ : Restriction of the eigenvectors bundle to the link  $S^1$  of the variety of quadratic forms having a multiple eigenvalue (at its regular point) is the *Möbius* bundle (sending the boundary of the Möbius band to its central circle). In the

Hermitian (hyperhermitian) case, the transversal to the variety of multiple eigenvalues forms is  $\mathbb{R}^3$  (respectively  $\mathbb{R}^5$ ), therefore the link is  $S^2$  (respectively  $S^4$ ). The eigenvectors bundle's restriction to this link is the *Hopf bundle*.

 $(9) \rightarrow (10)$ : "Hypercurvature" should be a 4-form, providing the first Pontriagin class. Its geometrical description as the *complexification of curvature* might hopefully provide more topological information than its probable relation to the square of the curvature form and to Chern – Simons theory.

The algebra of forms, generated by the Chern forms of eigenvector bundles, has been studied recently by B. Shapiro and his collaborators; they have also proved degeneration of the spectral sequence, corresponding to stratification of the Hermitian forms space according to the eigenvalues' multiplicities.

The real and hyperhermitian cases seem to be not yet settled in these theories.

 $(1) \rightarrow (13)$ : It is difficult to believe that the octahedron is the complex version of the tetrahedron, and icosahedron – of the octahedron. I first deduced it from the lecture of D. Kazhdan at Gelfand's 80th birthday celebration at Rutgers (1993). The strange numerology below provides some confirmation of the mysterious parallelisms of all mathematical trinities, such as the parallelisms  $(1) \rightarrow (13)$  or  $(1) \rightarrow (22)$ .

 $(13) \rightarrow (14)$ : The numbers of edges have the following property :

$$6 = 2.3, 12 = 3.4, 30 = 5.6.$$

We recognise in the trinity (2,3,5) the codimensions of the varieties of the multiple eigenvalues' quadratic, Hermitian and hyperhermitian forms. Of course, we have also

$$2 = 1 + \dim \mathbb{R}, \quad 3 = 1 + \dim \mathbb{C}, \quad 5 = 1 + \dim \mathbb{H}$$

and the numbers

$$3 = 2 + \dim \mathbb{R}, \quad 4 = 2 + \dim \mathbb{C}, \quad 6 = 2 + \dim \mathbb{H}$$

are the codimensions of the varieties of degenerate forms.

(13)  $\rightarrow$  (15): The rotation group  $\Gamma \subset SO(3)$  of a regular polyhedron (13) is covered twice by the "binary group"  $\widetilde{\Gamma} \subset \text{Spin}(3) = SU(2)$ , acting on  $\mathbb{C}^2$ . Its *orbit* space is the surface of zeros of the polynomial (15).

 $(15) \rightarrow (16)$ : The simple singularities (15) are "fenced" from the sea of nonsimple ones by the "parabolic" (or "affine  $E_6, E_7, E_8$ ") singularities, of which the polynomials (16) are the simplest representatives.

 $(16) \rightarrow (17)$ : Consider a triangle which one can buy in a stationery shop, (17). The affine reflection group defined by the triangle acts on the corresponding *elliptic* curve. The fencing singularity (16) can be obtained from this action on the elliptic curve essentially by the same invariant theory construction that builds the simple singularity (15) from the action of the binary group of a regular polyhedron (13) on the rational curve  $\mathbb{C}P^1$ .

 $(13) \rightarrow (18)$ : The symmetry groups (18) of regular polyhedra (13) are generated by reflections.

 $(18) \rightarrow (19)$ : The mirrors of a reflection group (18) of order |W| subdivide the 3-space into |W| simplicial cones, called Weyl chambers.

The three boundary mirrors of a chamber subdivide the 3-space into 8 larger pyramids – the *Springer cones*. The summands in (19) represent the numbers of Weyl chambers in each Springer cone. Note the prime summands.

 $(19) \rightarrow (20) \rightarrow (21) \rightarrow (13)$ : Adding one to the numbers of line (19) one gets the *degrees* (20) of the invariants of the reflection groups (21) and also the numbers of vertices, faces and edges of the polyhedra (13).

 $(15) \rightarrow (22)$ : The monodromy groups of the simple singularities (15) are the Euclidean reflection groups (22).

 $(13) \rightarrow (22)$ : The J. MacKay correspondence describes the Dynkin diagrams of the (extended) root systems (22) directly in terms of representation theory of the binary group of the corresponding regular polyhedron (13).

 $(11) \rightarrow (23)$ : Gabrielov observed that, in the polylogarithmic formulae for characteristic classes studied in his works with Gelfand, Losik and McPherson, the two poles case corresponded to the Chern class and the three poles case to the Pontriagin class.

 $(23) \rightarrow (24)$ : The Turaev–Frenkel theory of the *elliptic numbers relation to mod*ular hypergeometric functions is described in their paper in the "Arnold–Gelfand Mathematical Seminars", Birkhäuser, 1997.

 $(23) \rightarrow (25)$ : The trinity, consisting of *cohomology*, *K*-theory and elliptic theory, has been suggested by A. B. Givental.

It is interesting that the complexification of a manifold is in no way unique, depending on the structures we are interested in. Thus, the complexification of  $S^1 = \mathbb{R}P^1$  being  $\mathbb{C}P^1 = S^2$ , that of  $S^1 = SO(2)$  is  $SU(2) = S^3$ . This is nice, making it possible to complexify the Möbius bundle

$$S^0 \to (S^1 = SO(2)) \to (S^1 = \mathbb{R}P^1)$$

to obtain the Hopf bundle

$$S^1 \to (S^3 = SU(2)) \to (S^2 = \mathbb{C}P^1).$$

Trinity (6) suggests that the complexification of  $S^1$  is sometimes also  $S^4$ .

Trinity (3) suggests that the complexification of  $\mathbb{Z}_2$  is  $\mathbb{Z}$ . This is confirmed also by the following construction (originating from one of the attempts to *complexify* braid groups and to quaternionize permutation groups).

Consider the set E of homotopy classes of mappings from a Lie group G to itself preserving the identity. Define the (virtually noncommutative) "addition" in E by (f + g)(x) = f(x)g(x) and the "multiplication" by (fg)(x) = f(g(x)). The resulting algebraic structure on E seems to have no name (the distributivity holding only from one side), so I shall call this "ring" E the *ellipse of* G.

The ellipse of  $S^0$  is the ring  $\mathbb{Z}_2$  of two elements, that of  $S^1 = SO(2)$  is the ring  $\mathbb{Z}$ . The ellipse of  $S^3 = SU(2)$  is also  $\mathbb{Z}$  (I would prefer to obtain the Gauss numbers ring as the complexification of  $\mathbb{Z}$ ).

To complexify  $\mathbb{Z}$  one can also consider it as the *colored braid group*  $\mathbb{B}r(2)$  where

 $\mathbb{B}r(n) = \pi_1(\mathbb{C}^n \setminus \mathbb{C}\text{-mirrors of } A_n),$ 

or as the ordinary braid group Br(2), where

$$Br(n) = \pi_1(\operatorname{Config}_n \mathbb{C}).$$

The colored braid group  $\mathbb{B}r(n)$  is the complexification of the symmetric group:

 $\pi_0(\mathbb{R}^n \setminus \mathbb{R}\text{-mirrors of } A_n) \approx S(n).$ 

To complexify the fundamental group  $\pi_1(X)$ , one is tempted to write  $\pi_1(X) = \pi_0(\Omega X)$  and to complexify it to  $\pi_1({}^{\mathbb{C}}\Omega {}^{\mathbb{C}}X)$ . The complexification of the loop space is perhaps related to holomorphic loops. Applying these ideas to  $X = \mathbb{C} \setminus 0$ , we

should consider as complex loops the polynomial mappings of  $\mathbb{C}$  into  $\mathbb{C}^2 \setminus 0$  (the boundary conditions at infinity being provided by the highest degree terms).

Zariski's theorem reduces calculation of the fundamental group of the space of these pairs of polynomials to the calculation of the fundamental group of the complement of a single rational curve in  $\mathbb{C}^2$  (which is the image of a generic polynomial mapping of  $\mathbb{C}$  of a given degree). I guess that the final answer is either  $\mathbb{Z}$  or  $\mathbb{Z}^2$ , depending on whether the boundary conditions at infinity correspond to the complexification of the colored braid group  $\mathbb{Z} \approx \mathbb{B}r(2)$  or to the ordinary braid group  $Br(2) \approx \mathbb{Z}$ .

The quaternionic versions of the Coxeter and Shephard – Todd groups are still to be defined and classified. These versions might be Lie groups whose orbit spaces are smooth, or groups generated by the "quaternionic reflections". One might speculate that existence of the sporadic simple groups is related to what remains from the real and complex reflection groups in the quaternionic version of the reflection groups trinity.

The complexification of orientation is of course a nowhere-vanishing holomorphic highest degree form. B. Khesin and I. Frenkel have suggested that the complexification of cohomology theory should be Leray's theory of meromorphic differential forms. Using this theory, Khesin and Rosly were able to define the "complex linking number" of two complex curves in some complex 3-manifolds (generalising the Gauss integral formula for the linking number of real curves in real 3-space).

In mathematics we always encounter mysterious analogies, and our trinities represent only a small part of these miracles. I might mention, as an example, the "strange duality" of Lobachevsky triangles, which I discovered in 1974 and which is now explained by V. Batyrev as the first manifestation of the general *mirror symmetry* of physicists. As an example of a still puzzling mystery, I shall mention R. Faure's duality, relating the particle moving with energy E in the field with potential  $U(z) = |dw/dz|^2$  with the particle moving with energy -1/E in the field with potential energy  $V(w) = -|dz/dw|^2$ , whatever be the holomorphic function w(z). The function  $w = z^2$  provides the classical duality between Hooke and gravitation or Coulomb forces, but the above duality is quite general and holds both in the classical and in the quantum versions.

A commented list of several hundred problems, originating from such "experimental facts", has been prepared by my Moscow seminar participants and will hopefully be published soon.<sup>4</sup>

Attempts to complexify and to quaternionize mathematical theories are making clear the fundamental unity of all parts of mathematics. Growing specialisation and bureaucratic subdivision of mathematics into small domains becomes an obstacle to its development. The organisers of the International Congress of Mathematicians in Berlin in 1998 considered their bureaucratic sections as scientifically independent entities. As a result, parallel talks, formally belonging to different sections, were in fact devoted to the same subject, be it called symplectic geometry, mathematical physics, differential topology, partial differential equations, global analysis, quantum mathematics or infinite dimensional Lie algebra theory.

<sup>&</sup>lt;sup>4</sup>This book is now published: V. Arnold, *Arnold's problems*. Translated and revised edition of the 2000 Russian original. With a preface by V. Philippov, A. Yakivchik and M. Peters. Springer-Verlag, Berlin; PHASIS, Moscow, 2004.

Several invited speakers of the Berlin Congress told me that they would rather have listened to each other's talk, but were unable to do it as they were speaking simultaneously. This lack of understanding of the interrelations between different domains of mathematics originates from the disastrous divorce of mathematics from physics in the middle of the 20th century, and from the resulting degeometrisation of mathematical education.

Criticising my statement that mathematics is a part of physics, one of the former best Bourbaki leaders <sup>5</sup> wrote to me in June 1998: "Mathematics is completely different from physics... Mathematicians should not write on such philosophical questions, since even the best of them can write pure nonsense." It is interesting to compare these boomerang statements with the words of Hilbert, who wrote in 1930: "Geometry is nothing more than a branch of physics; the geometrical truths are not essentially different from physical ones in any aspect and are established in the same way" (Naturerkennen und Logik, Naturwissenschaften, 1930, 959-963).

Hilbert tried to predict the future development of mathematics and to influence it by his Problems. The development of mathematics in the 20th century has followed a different path. The most important achievements – the flourishing of homotopy theory and of differential topology, the geometrisation of all branches of mathematics, its fusion with theoretical physics, the discovery of algorithmically undecidable problems and the appearance of computers – all this went in a different (if not opposite) direction.

The influence of H. Poincaré and of H. Weyl on the science of the 20th century was much deeper. To Poincaré, who created modern mathematics, topology and dynamical systems theory, the future of mathematics lay in the development of mathematical physics, oriented to the description of relativistic and quantum phenomena. Among other important things, Poincaré explained that only noninteresting problems might be formulated unambiguously and solved completely. According to Poincaré, one should rather try to understand what may be changed in the problem formulation. He had in mind first of all the variation of the coefficients of equations in bifurcation type problems and all kinds of general position arguments – the topics which are now called singularity theory, global analysis and functional analysis. Interestingly enough, what is now called the versal deformation theorem had already been proved in his Thesis (for the case of zero dimensional holomorphic complete intersections) as lemma 4, and was the basis for his bifurcation theory.

The mathematics of the 20th century mostly followed the road shown by Poincaré (the main difficulty being — as A. Weil once told me — the fact that too many good mathematicians have appeared, whereas all valuable mathematicians personally knew each other at Poincaré's time). According to Kolmogorov, Hilbert was seriously worried by what would happen to the Mathematische Annalen cover in 500 years: he thought that the names of the former Editors would fill up all the space. Kolmogorov in return expressed to Hilbert his own worries that our culture would probably not survive for such a long period: the united bureaucrats of all countries would soon be able to stop all kind of creativity, making further mathematical discoveries impossible, as are geographical discoveries today. In our time, we can imagine that some of the most appealing domains of mathematics will be transformed into wilderness preserves, where rich people will be able to buy for

 $<sup>{}^{5}</sup>$ He declined the invitation to participate in the present book, explaining that, according to his experience, all collective works are failures.

an expensive price the pleasure to hunt one or two theorems, guided by scientific jaegermeisters.

It is difficult to decide which of these predictions is more likely to happen. It seems however clear that the centre of humanity will soon move from wealthy Europe and North America to hungry Asia, where the culture to which Kolmogorov and Hilbert referred may indeed have little chance of surviving.

I would like to hope that this prediction is as wrong as the others. Discussing these perspectives, the optimist H. Whitney insisted that America is still producing excellent mathematicians in spite of the sad fact that its general cultural and educational level is already almost as low as that of the future global attractor. One may also hope that the coming nuclear civil wars and military confrontations will lead to a better appreciation of science by society and to a paradoxical flourishing of world mathematics (similar to the flourishing which occurred in Russia after the awful Bolshevik revolution).

## CHAPTER 5

# A Mathematical Trivium

## V. I. Arnold

This chapter, available in our print version <u>here</u>, is unavailable in the e-version of this volume; please see Arnol'd, V. I., "A mathematical trivium" originally published by the London Mathematical Society in *Russian Math. Surveys* 46:1 (1991), pp. 271-278.

### CHAPTER 6

# Comments on "A Mathematical Trivium"

Boris Khesin and Serge Tabachnikov

What follows are solutions, hints, and comments to some of the problems from Arnold's *Trivium*. This compendium is not uniform: some problems are standard exercises that every mathematician should do once in a lifetime (such as problem 90), some would be easy to those who have mastered a particular subject (for example, problem 99 is a test on the basics of game theory), and some are ingeniously constructed, akin to sophisticated chess problems (e.g., problem 2). The reader interested in a more-or-less complete set of solutions is referred to the blog [**21**] (in French).<sup>1</sup>

The problems in the Trivium cover a large part of mathematics but not all of it: one will not find number theoretical or combinatorial problems here. Of course, the selection of problems reflected mathematical interests and tastes of the author (at the time of writing). Overall, this collection represents well what Arnold expected from his students.

One of us (S.T.), when a graduate student at the Moscow State University, had to take a special topic examination from V. Arnold (every graduate student had to take three such exams). Arnold listened to the request and asked: "Can you draw a swallow tail?" (the discriminant surface of quartic polynomials  $x^4 + ax^2 + bx + c$ , see Figure 1). Only after this 'placement test' was more-or-less successfully passed, he agreed to discuss the matter.

When the other author of these comments (B.K.) asked Arnold to become his advisor (at the Moscow State this choice was - and still is - to be made during one's sophomore year), Arnold suggested first to solve "Test 1" given at the end of his book [2] as a take-home exam. As a matter of fact, almost all of Arnold's students (at least in the 80's) underwent a similar 'placement test'. We believe that that test served as a prototype for the whole "Mathematical Trivium" by Arnold. We present "Test 1" from [2] at the end of the comments.

One cannot help wondering what other mathematicians' (say Hilbert's or Poincaré's) Trivia would look like. By a wild flight of imagination, one could even think of those of Gauss, Euler, or Newton (the reader has noticed that Arnold's collection is somewhat tilted toward Newton and his time, as witnessed by the book [3]).

A brief comment on the name. The term *trivium* refers to the three subjects that were taught first at medieval universities: grammar, logic, and rhetoric. This was followed by the *quadrivium*, consisting of geometry, arithmetic, astronomy, and

 $<sup>^1\</sup>mathrm{Warning}$ : Some of the problems were translated to French with distortion, and this affected some solutions.

music. The trivium and quadrivium resulted in the seven liberal arts of classical study.

Note that Arnold also wrote a sequel, the paper "Mathematical Trivium II", which, unlike the list of 100 problems under discussion, consists of about a dozen typical final exams in courses (in analysis, differential equations, group theory, etc.) given at mathematics departments around the world, see [5].

A final remark: Mathematical Trivium overlaps with another collection of problems written by V. Arnold, this one for children from 5 to 15 years old [8]. Namely, problems 6, 13, 86 from the Trivium (all commented upon below) are included into [8], and problem 65 appears in both collections. Curiously, in the latter problem the students (i.e., the readers of Trivium) have to consider the 2-dimensional case, whereas the 'kids' (solving [8]) are to deal with the 3-dimensional case!

**2**. The answer is 1, see [**3**]. To quote from that book:

Here is an example of a problem that people like Barrow, Newton and Huygens would have solved in a few minutes and which present-day mathematicians are not, in my opinion, capable of solving quickly...

**6**. This is a curve with two cusps and one self-intersection. It appears (upside down) as a section of the swallowtail in  $\mathbb{R}^3$ , see Figure 1.



FIGURE 1. Swallowtail

7. The generic answer is 2 or 4, depending on whether the point is inside or outside of the evolute. At a smooth point of the evolute, the answer is 3, and at its cusp, the answer is 2. See Figure 2.

A version of this problem, to describe the set of points outside of an ellipse from which one can drop the greatest number of perpendiculars to the ellipse, was offered at the First All-Union Mathematical Olympiad for college students in 1974, see [4]. Out of 89 carefully selected participants, only one completely solved the problem, and 39 did not even attempt to solve it.



FIGURE 2. The evolute (the envelope of the normals) of an ellipse

**9.** No; example:  $x^2 + (xy-1)^2$ . This problem was offered at the 'Mathematical Wrangle' (Matematicheskiy Boy) at the Soviet All-Union Mathematics Olympiad in 1973.

10. For a curve defined by a given polynomial in x and y one can find each branch of the curve with undetermined coefficients in the form of the Puiseux series  $y(x) = \sum_{k=k_0}^{\infty} a_k x^{k/n}$  in fractional powers of x, where n and  $k_0$  are defined by the leading term and slope of the segments facing the origin of the corresponding Newton polygon.

In this problem, after substituting  $u = y^2$ , in order to find expansion of the curve given by the equation  $f(x, u) = x^5 + x^2u - u^3 = 0$  we first construct its Newton polygon; i.e., mark on the two-dimensional plane  $\mathbb{Z}^2$  the integer points A = (5, 0), B = (2, 1), and C = (0, 3), corresponding to the powers of monomials  $x^5, x^2u$ , and  $u^3$ , respectively, see Figure 3.



FIGURE 3. The Newton polygon of the polynomial  $x^5 + x^2u - u^3$ 

Then the branches of the curve at the origin in the (x, u)-plane are described by the parts of the original equation corresponding to those sides of the Newton polygon that face the origin:  $x^5 + x^2u = x^2(x^3 + u)$  and  $x^2u - u^3 = u(x^2 - u^2)$  corresponding to the segments [A, B] and [B, C], respectively. Then the first approximations of the branches of the curve defined by  $x^5 + x^2u - u^3 = 0$  are the components of these curves  $x^3 + u = 0$  and  $x^2 - u^2 = 0$ . Namely, the curve  $x^3 + u = 0$  is a cubic parabola  $u = -x^3$ , so in terms of the initial variable y we obtain  $y^2 = -x^3$ ; therefore,  $y = ix^{3/2}$ . This defines the asymptotics of the branch corresponding to [A, B]. Similarly, the equation  $x^2 - u^2 = (x - u)(x + u) = 0$  defines two lines, u = x and u = -x; i.e., two different branches,  $y = x^{1/2}$  and  $y = ix^{1/2}$  corresponding to the side [B, C].

To find a more detailed asymptotic given by a full-fledged expansion one proceeds as follows. For the branch corresponding to the side [A, B] one plugs the expression  $u = -x^3(1 + \sum_{k=1}^{\infty} a_k x^k)$  with undetermined coefficients into the original equation. The other two branches, which correspond to [B, C], are defined by plugging the expansions  $u = \pm x(1 + \sum_{k=1}^{\infty} b_k^{\pm} x^k)$ . To obtain the expansion in terms of y one can either take the square root of the above expansions for u or, alternatively, find the corresponding expansions for the branches directly from the original equation by using Puiseux series  $y = ix^{3/2} + \ldots, y = x^{1/2} + \ldots$ , and  $y = ix^{1/2} + \ldots^2$ 

12. Note that this vector field is  $\nabla(-1/r)$ , the gradient of the potential function -1/r. By the divergence theorem the given integral over the sphere equals the integral of the divergence of this field,  $\operatorname{div}(\nabla(-1/r)) = \Delta(-1/r)$ , over the ball bounded by the sphere. In turn, the function 1/r is a multiple of the Green function in  $\mathbb{R}^3$ , so its integral over a domain depends on whether the source point is inside the sphere or not.

**13**. The answer is surprisingly neat:  $3 \cdot 10^9$ .

Here is a sketch of a solution, adapted from [21]. Consider the function

$$f(x) = \frac{x^x}{\ln x + 1}.$$

Then,

$$x^{x} = f'(x) + \frac{x^{x-1}}{(\ln x + 1)^{2}};$$

hence,

$$\int_{1}^{10} x^{x} dx = f(10) - f(1) + \int_{1}^{10} \frac{x^{x-1}}{(\ln x + 1)^{2}} dx \approx f(10),$$

since  $x^x$  is sufficiently larger than  $x^{x-1}/(\ln x + 1)^2$  and than f(1) = 1.

This problem was also offered at the First All-Union Mathematical Olympiad for college students in 1974. Only two students solved the problem, and 50 did not even attempt to solve it. See [4].

A version of this problem, to evaluate  $\int_{1}^{100} x^{x} dx$ , with 5% accuracy, is found in [17] (problem 174). The problem is attributed to M. Klamkin. The solution makes use of the inequalities

$$\frac{b^b - a^a}{1 + \ln b} \le \int_a^b x^x \, dx \le \frac{b^b - a^a}{1 + \ln a}.$$

As we mentioned above, this very problem appears in the list for kids from 5 to 15 [8]!

14–15. In both of these problems after some tricks, (see the corresponding solutions in [21]), one is taking the first two terms of an appropriate series expansion. One cannot help but quote from [3]:

<sup>&</sup>lt;sup>2</sup>We are grateful to O.Viro for an improvement of this comment.

As for the convergence, these series converge so rapidly that Newton, although he did not strictly prove convergence, had no doubt about it. He had the definition of convergence and explicitly calculated series for specific examples with an enormous number of digits (in the letter to Leibniz Newton wrote that he was ashamed to admit to how many digits he took these calculations).

16–17. The phenomenon of accumulation of volume near the boundary of the ball as the dimension goes to infinity is discussed; e.g., in [20].

18. The higher-dimensional Gauss integral

$$\int_{\mathbb{R}^n} \exp\left(-\frac{1}{2}\sum_{i,j=1}^n A_{ij}x_ix_j\right) d^n x = \sqrt{\frac{(2\pi)^n}{\det A}}$$

reduces the calculation to finding the determinant of the  $n \times n$  matrix A. In our case, A/2 has 1's on the diagonal and 1/2's otherwise. Its determinant can be thought of as the Gram determinant for n unit vectors forming a regular tetrahedron in  $\mathbb{R}^n$ , that is, the vectors connecting one vertex of a unit regular tetrahedron in  $\mathbb{R}^n$  with n other vertices. The Gram determinant is the square of the volume of the parallelotope formed by these vectors, which in turn, equals  $(n+1)/2^n$ .

**19**. The graph  $y = 1/\sin x$  is a trajectory.

**29**. By definition, the Larmor circle is the osculating circle of the trajectory, and the locus of its centers is the evolute of the trajectory, the envelope of its normals. Therefore the center of the Larmor circle drifts in the direction orthogonal to the trajectory.

**33**. These phase trajectories give the Hopf fibration of the 3-dimensional sphere, the level surface of the total energy. Different Hooke coefficients imply that the periods of oscillations in the  $(x, \dot{x})$ - and the  $(y, \dot{y})$ -planes differ. For the common period, one trajectory traverses the circle twice, and the other trice.

**34**. The curve  $y = x^3$  has a cusp at infinity: in the respective affine chart it is a semi-cubic parabola. The curve is projectively self-dual: the cusp at infinity corresponds to the inflection point at the origin. V. Arnold was interested in projectively self-dual curves and posed the problem of their classification in [7] (problem 1994-17).

**35**. Use Clairaut's theorem for geodesics on a surface of revolution: along such a geodesic, the quantity  $\rho \sin \alpha$  is constant, where  $\rho$  is the distance to the axis of rotation, and  $\alpha$  is the angle between the geodesic and meridians of the surface.

**36**. See Figure 4. Note that there is a 1-parameter family of involutes (also called evolvents): they form an equidistant family of curves.

Given a curve, move each point along the normal line the same distance t; the locus of these points is an equidistant curve. If the original curve is a source of light, the the equidistant curves consist of the points reached by light at a given time. If the initial curve is smooth then the equidistant curves are also smooth for small values of t but, typically, they eventually develop singularities.

The involutes of a given curve constitute an equidistant family. Equivalently, the evolutes of the equidistant curves coincide.



FIGURE 4. Involutes of a cubic parabola

**37**. This problem concerns the classical theory of elliptic coordinates, due to Jacobi. The dual problem reduces to the orthogonality of the eigen directions of a symmetric matrix. For proofs and discussions, see, e.g., [11].

**38**. This surface is a torus, its Euler characteristic is zero. By the Gauss-Bonnet theorem, the total curvature is also zero.

**39**. The Gauss integral is a multiple of the linking number of the corresponding curves.

40. This problem is solved using the Gauss-Bonnet theorem. See [1] where this question is discussed in the context of the Foucault pendulum (the one at the St. Isaac's Cathedral at St. Petersburg).

41. This curve is a horocycle; its curvature is 1.

42. Arnold was thinking about variations of this problem over the years. He gave a proof for the altitudes, deducing this fact from the Jacobi identity in the Lie algebra sl(2, R) of isometries of the hyperbolic plane. A similar, but simpler, proof can be given in the spherical geometry (the Lie algebra so(3)). See [9], and [14] for an exposition. Incidentally, the notion of center of mass is also well defined in the spherical and hyperbolic geometry; see [12].

53. One can show that the 1-form dx/y has no poles or zeros for finite x and y. The problem reduces to the study of the point(s) at infinity, compactifying the Riemann surface.

54. The potential energy is a polynomial of degree 4, and hence the corresponding (compactified) energy surface is an elliptic curve (cf. problem 53). The form dt = dx/y, where  $y = \dot{x}$ , is a holomorphic form on the curve, while oscillations in the two wells correspond to homologically equivalent cycles.

**61.** The equation describes velocity of a one-dimensional gas in a force field. Its equation of characteristics is the system  $\dot{x} = u, \dot{u} = \sin x$ , which is the physical pendulum equation:  $\ddot{x} = \sin x$ . The pendulum trajectories near stable equilibria  $x = \pi + 2\pi k$  behave like those of the mathematical pendulum equation  $\ddot{x} = -x$  after the shift of variable. The projections from the (x, u)-plane onto the x-axis

of the trajectories of the latter equation, that start at  $u|_{t=0} = 0$ , coincide after a quarter-period, that is, after  $t = \pi/2$ . Thus the solution u as a function of x should have different values at the same value of x starting at this time  $t = \pi/2$ : this corresponds to merging a shock wave for the gas motion. The same is true for the equation of physical pendulum, as x tends to any stable equilibrium. Hence the continuation of u as a function of x beyond  $\pi/2$  is impossible.

**62–63**. Rewrite the equation as a Lie derivative of u along a vector field. For example, the equation of problem 62 is  $L_v u = u^2$  where  $v = y\partial/\partial x - \sin x\partial/\partial y$ . Then make use of the topology of the field's orbits.

**64**. The problem reduces to investigating the smoothness of the fronts generated by the parabola (that is, its equidistant curves), inside and outside. Inside of the parabola, one encounters a focal point.

**70.** Use problem 69: such a solid angle is a harmonic function in  $\mathbb{R}^3$  outside the circle. Hence its mean value on the sphere is equal to its value at the sphere center.

**75**. Heat propagation is described by the heat equation

$$\frac{\partial T(x,t)}{\partial t} = \frac{\partial^2 T(x,t)}{\partial x^2}$$

where T(x,t) is the temperature at depth x at time t. This equation is invariant under the one-parameter group  $(x,t) \mapsto (cx, c^2t)$ . Therefore if the time is changed by a factor of 365 then the depth is changed by a factor of  $\sqrt{365} \approx 19$ . Hence the ground would freeze at depth of about 10 cm.

77. The eigenfunctions of the Laplacian on the unit sphere in  $\mathbb{R}^n$  are the spherical functions, that is, the restrictions on the sphere of the harmonic polynomials in  $\mathbb{R}^n$ . The harmonic polynomials of the same degree correspond to the same eigenvalue of the Laplacian. The dimension of the space of harmonic polynomials of degree k equals  $\binom{n+k-1}{n-1} - \binom{n+k-3}{n-1}$ . See, e.g., [6] for the theory of spherical functions.

83. This is the famous Korteweg–de Vries equation; the problem concerns its soliton solutions, see; e.g., [15].

84–85. The computations are simplified if one notices that the quadratic forms are symmetric and hence have n-1 equal eigenvalues.

86. The sum in question is the moment of inertia of the vertices about the line. Due to the symmetries of a regular polyhedron, its ellipsoid of inertia is a round sphere, and the sum of squares is the same for all lines.

This problem is discussed in [10] in the section "Symmetries (and the Curie Principle)". The problem is attributed to Landay and Lifshitz [18]. The Curie Dissymmetry Principle reads: a physical effect cannot have a dissymmetry absent from its efficient cause.

**94**. The irreducible components include two 2-dimensional and one 1-dimensional spaces, corresponding to two pairs of complex conjugated 5th roots of unity and 1. This decomposition is extensively discussed in [3] in the context of quasicrystals and the Penrose tilings.
**99.** (adapted from [21]) Let Player 1 conceal the 10-copeck coin with probability p (and respectively, he would conceal the 20-copeck coin with probability 1-p), while we let Player 2 call the 10-copeck coin with probability q. Then the expectation value of the gain for Player 2 is

$$E = 10pq + 20(1-p)(1-q) - 15p(1-q) - 15(1-p)q$$

copecks. Rewrite this as follows:  $E = 60(p - \frac{35}{60})(q - \frac{35}{60}) - \frac{35 \cdot 35}{60} + 20$ . Hence if p > 35/60 = 7/12, the second player has to choose q equal to 1, and if p < 7/12, the second player chooses q equal to 0 to maximize his gain. Similarly, if  $q \neq 7/12$  the first player can choose an appropriate p to his advantage and bigger loss of the second player. The optimal strategy for both players is choosing p = q = 7/12. The expected gain for the second player in this case is  $E = 20 - (35 \cdot 35)/60 = -25/60 = -5/12$  copecks. Thus the game is not fair: with the best strategy Player 2 loses at average 5/12 copecks in each round.

100. According to the Cauchy-Crofton formula, the area of a closed convex surface S in space is

$$\frac{1}{\pi} \int A(P_l(S)) \, dl$$

where  $P_l(S)$  is the projection onto the plane orthogonal to direction l and  $A(P_l(S))$  is the area of this projection. The integral is over the sphere of all directions with respect to the uniform measure on the sphere. Applied to the unit cube, the average area of the projection is 3/2. See, e.g., [13, 16] for details.

More generally, given a convex body  $K \subset \mathbb{R}^n$ , one may consider the mean volume of its projection on a k-dimensional subspace. Up to universal constants, these quantities are called intrinsic volumes. They appear as coefficients in Steiner's formula for the volume of the  $\varepsilon$ -neighborhood of K: this volume is a polynomial of degree n in  $\varepsilon$ . If the boundary of K is a smooth hypersurface then these quantities are equal to the normalized integrals of the elementary symmetric polynomials of its principal curvatures. Arnold also discusses this matter in [10].

By the way, the same Steiner's formula (for large  $\varepsilon$ !) solves the following Box in a Box problem (see [19]):

Let the cost of a rectangular box be given by the sum of its length, width, and height. Prove or disprove: It is impossible to fit a box into a cheaper box.

Test 1

$$\ddot{x} = -\sin x + \varepsilon \cos t.$$

(1)

As we discussed in our introduction to these comments, we finish with another examination, taken from [2]. The reader will see many a familiar theme; we hope that (s)he will find this test interesting and instructive.

In the four-hour written examination, 15 interrelated problems are given. Within square brackets, we indicate the point value of each problem. These values are revealed to the students beforehand.

(1) Linearize at the point  $x = \pi$ ,  $\dot{x} = 0$ . [1]

(2) Is this equilibrium position stable? [1]

(3) Find the Jacobian matrix of the mapping of the phase flow at the point  $x = \pi$ ,  $\dot{x} = 0$  at time  $t = 2\pi$ . [3]

(4) Find the derivative of the solution with initial condition  $x = \pi$ ,  $\dot{x} = 0$  with respect to the parameter  $\varepsilon$  at  $\varepsilon = 0$ . [5]

(5) Draw the graph of the solution and its derivative with respect to t under the initial condition x = 0,  $\dot{x} = 2$ . [3]

(6) Find this solution. [3]

II. Let Eq. (2) be the linearized equation along the solution indicated in problem **5**.

(7) Does Eq. (2) have unbounded solutions? [8]

(8) Does Eq. (2) have nonzero bounded solutions? [8]

(9) Find the Wronskian of a fundamental system of solutions of Eq. (2) given that W(0) = 1. [5]

(10) Write out Eq. (2) explicitly and solve it. [10]

(11) Find the eigenvalues and eigenvectors of the monodromy operator for the linearized equation along the solution with initial condition  $x = \pi/2$ ,  $\dot{x} = 0$ . [16]

(12) Prove that Eq. (1) has a  $2\pi$ -periodic solution depending smoothly on  $\varepsilon$  and vanishing at  $x = \pi$  for  $\varepsilon = 0$ . [6]

(13) Find the derivative of this solution with respect to  $\varepsilon$  at  $\varepsilon = 0$ . [6]

III. Consider the equation  $u_t + uu_x = -\sin x$ .

(14) Write out the equation of characteristics. [2]

(15) Find the largest value of t for which the solution of the

Cauchy problem with  $u|_{t=0} = 0$  can be extended to [0, t). [8]

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### CHAPTER 7

## About Vladimir Abramovich Rokhlin

## V. I. Arnold

I first met Vladimir Abramovich Rokhlin at the seminar on ergodic theory at Moscow State University, and he would weekly commute from Kolomna where he was able to get his resident registration.<sup>1</sup> Coming to Moscow, he would stay at his friends' place and sleep on a folding bed. In the morning though he insisted on calling the folding bed differently than at night. "It's a folding bed in the morning, but an unfolding bed at night".

But spending every year as neighbors at the cottages at Nikolina Gora<sup>2</sup> for over 10 years had a much more significant impact on me. We would talk for hours about all sorts of things, usually strolling along the Moscow river bank, often accompanied by other Zarechie inhabitants – the Efimovs, the Shilovs, the Shura-Buras, the Jacobsons, the Kushnirenkos, the Pomanskis.<sup>3</sup> Sinai used to come to fill his water canister, because there was no running water in the nearby Novo-Daryino at that time.

According to Courant's definition, a mathematician should be considered young for as long as he is inclined to discuss math at the most inappropriate times. Moscow river bank would become a special kind of a remote office of the Mekh-Mat, filled with young mathematicians of all ages.

Speaking with Vladimir Abramovich<sup>4</sup> I always felt as if I were communicating with a supreme mind, aware of the most final and true answers to all questions. I felt that air of irrefutable assurance about him that I probably have never felt about anybody else I have known. Perhaps, only Dieudonné possessed a similar air of confidence in his judgments and opinions, but it was so obvious when he was wrong and he would become excessively agitated in the course of an argument (probably, due to inferiority complex of some sort, and that was completely foreign to Rokhlin's nature).

Vladimir Abramovich's dignified demeanor and physical appearance reminded me of Korney Ivanovich Chukovsky,<sup>5</sup> who used to visit us at Arbat (he went to

Originally published in Russian in the book "V. A. Rokhlin: Selected Works", A. Vershik (ed.), Second Edition, MCCME, Moscow, 2009.

 $<sup>^1{\</sup>rm Kolomna}$  is 112 km away from Moscow. As a former prisoner of Gulag, Rokhlin was not allowed to live closer than 100 km to Moscow.

<sup>&</sup>lt;sup>2</sup>Nikolina Gora, Zarechie, Novo-Daryino are villages near Moscow where dachas (summer cottages) of many academics were located.

 $<sup>^3{\</sup>rm Moscow}$  mathematicians. Efimov served as the Dean of Mekh-Mat, the Faculty of Mechanics and Mathematics of the Moscow State University, in 1962–69.

 $<sup>^{4}</sup>$ Rokhlin

<sup>&</sup>lt;sup>5</sup>A famous children's poet, literary critic and essayist.

school with my grandmother's brother, B. S. Zhitkov,<sup>6</sup> and wrote very interesting things about him and my grandmother in his memoirs). Much later Vladimir Abramovich told me that this resemblance was no accident: Rokhlin and Chukovsky were closely related.

Although in a conversation, Rokhlin's air of irrefutable assurance could be irritating to others (not to me, I always listened to him with gratitude), this very aplomb added a unique charm to his lectures. And especially remarkable were V.A.'s lectures on topology, they entirely changed my views on what a good lecture is supposed to be.

One of the greatest science popularizers, Faraday, once said that the lectures that are really popular, are never instructive, and those, that are instructive, are never popular. Rokhlin's lectures managed to combine both virtues. Perhaps, the very reason why the book based on those lectures and written by D.B. Fuchs (a brilliant lecturer himself) is, in my opinion, no match for the lectures themselves is that while it is tempting to try and fit more material into a written text, this makes it difficult for a reader to follow the main idea.

Unlike Kolmogorov, who was hardly ever able to finish the phrase he started (let alone his proofs), Rokhlin was an articulate and effective speaker and never attempted to hide ideas behind calculations (which happened at times with his teacher, L. S. Pontryagin, whose brilliant lectures occasionally still lulled me to sleep). Rokhlin would just walk up to the blackboard, pick up a piece of chalk and start speaking. In about five minutes he would decide to use the board and write a letter on it (A, if he was talking about a ring, and M, if it was about a manifold, etc.). Then, a minute later he would erase what he had written, probably, so that the letter belonging to the part of the lecture that was over, would not divert the attention of the audience from the part that was to follow.

The audience (which was always too big to fit in the auditorium - undergraduates, graduate students, and professors, all attended his lectures) was listening in awe to the Great Master preaching. After all, the science Vladimir Abramovich was talking about had been all but banned on the Faculty (or at least had not been discussed in courses accessible to students) for over thirty years. The only topology allowed at Mekh-Mat was pathological (even now there still exists a chair of pathological topology here, perhaps the only one in the world).

To illustrate how detached we were from the rest of the world at the time, I should mention a curious incident. A remark found in the collection of translated papers "Fibre Bundles" that the idea of expressing a spectral sequence by rectangles consisting of groups, where the consecutive differentials act by generalized knight moves, is due to E. B. Dynkin (so that the corresponding figures in the notes to the translation are called Dynkin's diagrams). When I was in France in 1965, I asked Serre whether he knew of this improvement to the theory. Serre couldn't stop laughing. How else could one make calculations with spectral sequences? To be fair, in French publications there were no diagrams (indispensable to the reader) – probably to make the theory incomprehensible to the uninitiated (but more likely, due to typically careless French user-unfriendliness).

In his lectures, Rokhlin's used to reveal every such little secret one after another. Vladimir Abramovich was fully aware that no matter how much time one could save

<sup>&</sup>lt;sup>6</sup>An author, mainly of children's books, based on his rich experience as a sailor, ship captain, and explorer.

using the deductive methods ("from general to specific"), the value of a lecture for a student consists of merely a number of well-explained and thoroughly understood examples. Vladimir Abramovich's attitude towards examples was that of a respect, similar to the one held by physicists of inductive school of thought (starting with Newton), and contrary to the opinion of most of contemporary mathematicians (Sullivan once told me that he tended to avoid dealing with particular examples at all costs – they were way too complicated).

What made Rokhlin's lectures stand out was the way he dealt with both examples and theories, based on his perfectly pragmatic approach. "It is the cycles that are the most useful," he used to say, "but the cycles are like underwear you are not supposed to display in public; what is left in articles is only homological classes."

Rokhlin's opinions of the mathematicians around us and their mathematical theories was as insightful (as I see it now), as undeservedly harsh it seemed to me then. However, I heard P. S. Alexandrov say many times that "the highest degree in this country is doctorate; everything higher than that has little to do with achievements in science". But in some cases (for example, towards Kolmogorov and Pontryagin) Rokhlin was much more tolerant. His definitive opinions and the way of expressing them was what distinguished Rokhlin from almost all the mathematicians I have known, and in that respect I consider myself to be his follower. Rokhlin himself thought that he inherited these qualities from his teacher Pontryagin, who was known to follow in this in Benvenuto Cellini's steps.

When in 1961 Milnor came in Leningrad to attend the All-Union Mathematical Congress and talked about non-standard smooth structures on spheres, the impact of the progress made in non-pathological topology on mathematics in whole became evident even for older generation of the mathematicians, who had been ignoring everything that happened in scientific research after 1935 (as a consequence of an almost total 15-years-long isolation of Russian Mathematical school from the Western one). At that point Rokhlin became practically the only Russian mathematician who was actively involved in global efforts in conquering this new and unknown mathematical continent. "Mathematics today," he would say to me, "is like an exclusive aristocratic club. On top of a tremendous initiation fee, a hefty annual fee is required from its members".

Influenced by Milnor's presentation, my scientific advisor Kolmogorov recommended that I, a graduate student at the time, would include Milnor's spheres in my dissertation plan. (He intended to find out from me what was going on in topology from then on). At his advice I started learning differential topology from Fuchs, Novikov and Rokhlin and in a year I was appointed to be a referee of Novikov at his dissertation presentation. It was impossible to find a referee among the older generation, since already the words "exact sequence" represented an insurmountable obstacle for our professors at that time.

The level of understanding of "contemporary mathematics" that existed at the time at Mekh-Mat can be illustrated by the following episode. When I started explaining to Kolmogorov "what was going on in topology", he responded that his four articles published in 1935 in *Comptes Rendus* (where he introduced cohomology, simultaneously with Alexander, but based on physics, the ideas of hydrodynamics and electromagnetic theory) went unnoticed and unappreciated by topologists. "As a matter of fact," he said, "I not only identified the cohomology groups (which were

understood by everybody) but I also introduced the *ring*. If topologists paid attention to the ring (even now) they would be able to obtain interesting new results."

While in my unsuccessful attempts I was naively determined to make Andrey Nikolaevich change his views on the world of mathematics, Vladimir Abramovich displayed an uncharacteristic tolerance in this case. "The assessment of cohomological multiplication given by Kolmogorov," he told me, "is twice as remarkable because it is an evidence of his understanding of the significance of his own achievements and at the same time it foresees the future role of cohomology operations in general!"

Vladimir Abramovich somehow managed to combine his dangerously uncompromising views with an unusually apparent self-confidence, which inadvertently earned him respect even among his enemies. Perhaps, the reason for it was that unlike the majority of the colleagues his age and older, Vladimir Abramovich managed to avoid the humiliating compromises with the authorities, which poisoned the lives of generations of our countrymen. It is clear, of course, that he was just lucky to survive and that he occupied an undeservedly low position in the official hierarchy of the Soviet mathematicians. (He was undoubtedly our best mathematician of his generation, who also greatly influenced the future development of mathematical science in Russia). But unlike Kolmogorov and Pontryagin for example, by and large he had no reason to reproach himself. (This was something that he had in common with I. G. Petrovsky, who also earned an involuntary respect even among his enemies).

Unlike Petrovsky, Rokhlin was at some point influenced by the brown plague of Nietzschean philosophy. Rokhlin's friends recall that before the war he had been quite impressed with Hitler, his determination and his success.

After having spent lengthy periods of time both in France and Germany I am less surprised now with the viewpoint of young Rokhlin. France all but followed in Nazi Germany's footsteps in 1933. The majority of French now believe that Hitler beat Russia in WWII, but that he was later, as I recently found out from my French colleagues, defeated by France under de Gaulle. In German schools even now children are taught the following: "The defeat in WWI left Germany in a very bad shape economically. Hitler saved Germany, but he committed some mistakes in his foreign policy, which in turn lead to the defeat of Germany in WWII." When I asked random people on the streets of Bonn, I found out that "Adolf wouldn't tolerate such a lack of order we have now" and that the years of his rule were the best years ever in Germany. I asked an old little lady in Dusseldorf, "What is the name of this street – Adolf Hitler or Count Adolf?"<sup>7</sup> She replied, "Adolf deserves it" (i.e., Adolf earned the title of a count).

There is nothing surprising in the popularity of pro-Hitler sentiments in contemporary Russia, including among mathematically educated folks! Rokhlin, as it seems, was cured of his pro-Hitler illusions in a Nazi camp (while a Stalin camp completed his education).

I can easily picture young Vladimir Abramovich (as per his fellow students' recollections) leaving Steklov Institute library building at night, stretching, and saying, "Today is not my day, proved only seven theorems."

 $<sup>^7\</sup>mathrm{Count}$  Adolf Street would be the equivalent of Yuri Dolgorukiy Street in Moscow (V. Arnold's footnote).

Our conversations with V.A. usually were more like his monologues. I gained a fair amount of valuable information from his stories and not only about theorems, ideas and trends in mathematics (e.g., "the main idea of contemporary topology is to exploit the simplifications that result in considering infinite-dimensional spaces" – while I always tried to replace this actual infinite dimensionality by a high, but finite, dimension of the manifold approximating the functional space).

It was he who told me about wicked characters that prevailed among mathematicians I would have to deal with. I am amazed when I think about it now, how serious he was about what is mildly put as "a breach in research ethics" (and what I have come to call shameless thievery, especially when it is taken from naive Russian authors who tend to talk about their ideas without publishing proofs or only publishing them in Russian journals).

At that time I was under the impression that Kolmogorov was one who dealt with these situations (which he constantly experienced) with regal indifference. But Rokhlin explained to me that it was just his good upbringing and self-control, but any such case insults every mathematician, and the more he tries not to show it, the more it shortens his life.

It was Rokhlin who taught me the subtle difference between the technical achievements in the Amerigo Vespucci style and Christopher Columbus-esque fundamentally new paradigms, which he valued more than any "sporting" achievements (like proving the Four Color or Fermat's Last Theorems).

"Some very gifted mathematicians", V.A. used to say, "are always on standby and as soon as a new idea appears they are able to appreciate it and manage to gain more dividends on it than the author". Only later did I find out how Vladimir Abramovich was right. There are countless results (especially, the ones belonging to Russian authors) that are stolen by the international gangs of epigoni and arrogated to their sidekicks. I was especially impressed by V.A.'s clear-sightedness when I caught in the act three of the standby specialists he specifically mentioned (I was not the victim). Clearly, the Americanized ethical system in the mathematical science nowadays does not provide for penalties for such crimes (in Russia one would not shake hands with these people).

I can see now that there is a certain expediency in this immoral system. The society benefits from the actions of go-getters who develop new ideas fast. Few people are aware that the prosperity of Bell Phone Company was built on the stolen invention. (The priority of Antonio Meucci, whose application "had been lost" by Western Telegraph and whom Bell promised to pay 20% of its dividends, was established by the Supreme Court in 1886, that is only after Meucci's patent application expired).

I could give more examples like this one from the world of mathematics, where there are no patents and where Meucci's part was played by Andronov, Petrovsky, Pontryagin, Kolmogorov, and the part of Bell – by very influential mathematicians of the West. I can just refer to a recent scandalous publication by B.Arratin, A.Barbour, S.Tavaré "Random Combinatorial Structures and Prime Factorization" in the Notices AMS (1997), 44:8, pp 903-910, which shamelessly expropriated an important theory created by Rokhlin's student A.M. Vershik.

I made it a point not to hush up similar crimes (the way other Russian mathematicians do in the interest of self-preservation and because they are financially dependent on mathematical community of the West) and not only because I am

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always inclined to do so, but also to continue Rokhlin's legacy who thought that this situation was created only because the criminals know their deeds will go unpunished.

Indeed, nobody steals my own ideas (probably, out of fear), whereas almost all of my teachers, students, colleagues are being robbed and under-appreciated by the international mathematical community. Although, I heard that even the Nobel Prize Committee has become no more objective than the Fields one (it was brought to my attention by S. Smale even before the Nobel Prize for physics was awarded in 1997 for a discovery that was published in Russia according to official data about ten years before it was done by the laureate).

Rokhlin used to coach me that there are only two ways to avoid this kind of trouble. Either never tell anyone about your discovery until it is published (with all the corollaries and variations so that nobody can generalize anything at all), or tell everybody about it on every street corner so that they learn the results from the author himself.

To realize the second plan one had to attend international conferences and congresses a lot, which was impossible for us at the time. But I still chose the second way outlined by Rokhlin and would tell about my results at both Moscow and Leningrad mathematical societies meetings without waiting for anything to be published.

"From here it might seem," V.A. used to say, "that over there in the West they have more justice, and a scientist is judged according to his scientific achievements and not according to the party membership and ethnicity, as it is done here. But on closer examination the state of things proves to be even better here because there are two distinct groups that never mix together – ambitious social climbers and true mathematicians. We know who belongs to which group, and since there could not be any real scientific and fair manner than in the West where scientific motives cannot be distinguished from ambition-driven ones."

I believe that these idyllic concepts (circa mid 1960s) need to be adjusted: our mathematical community is becoming more like international.

V.A. treated writing (of his own articles and the articles of his students) with ferocity and used to spend a disproportionately large amount of time polishing multitudes of them. He explained to me once that "all students belong to one of two categories: intelligent and not (and such a division has nothing to do with either their mathematical abilities or social background)".

According to Rokhlin, you only need to show an intelligent student once how to turn his structureless and incoherent babble into a nicely edited and well-structured text. At the beginning of such a text Rokhlin would usually place his own "Premium quality seal": "The terminology used in this article is that of differential topology".

In plain language these magic words meant, "The author of this article is V. A. Rokhlin's student".

The subsequent texts written by the same student will never need editing again. Simply put, he will not be able to write differently. Other equally gifted, but "not intelligent" mathematicians will write the second and the third article (that may contain remarkable results) in the same helpless way. All of them will have to be rewritten by the teacher, and if he stops doing it, the results will be lost for mathematical community (until such time when someone may be willing to reformulate them properly).

When I complained to Vladimir Abramovich about the unreadable proofs of Ya .M. Eliashberg (I was a referee of his doctoral dissertation), V. A. responded: "I would never allow my student to publish such texts, but Eliashberg is not my student, but Gromov's. And you spoiled Misha yourself, when you wrote a positive referee report on his unreadable doctoral dissertation. I got tired of fighting with him and when I invited you to be a referee, I was hoping that you would smash his style. But you cut him some slack and Misha decided from then on that it was ok the way it was. And now you will have to put up with Eliashberg because of this!"

I have to mention that I could never come up with any counterarguments to any of Gromov's statements, while I was always able to find counterexamples to Eliashberg's ones (which were always very interesting). However, to fight back he would always modify his initial definitions for his theories, and that made them entirely incomprehensible for me as a result. After four or five of such modifications I still have no idea whether Eliashberg's proof of "Arnold's conjecture" (which in 1965 became a cornerstone of symplectic topology), described in his 1978 Syktyvkar work, is correct or not. Recently, Yasha promised to publish English translation of this work. Now, I hope, we will be able to find out if his proof was correct, the one that was 5 years ahead of the famous work of Conley and Zehnder (carried on by Floer, Chekanov, Gromov, Chaperon, Laudenbach, Sikorav, Hofer, Givental and many others).

The impact of Vladimir Abramovich on the writing style of works written in Russian in many different fields of mathematics (especially in differential and algebraic geometry, the singularity theory) was absolutely exceptional.

There is yet another Rokhlin's pedagogical theory, the true insight of which I have come to appreciate more and more over the years. According to Rokhlin, the teacher gives his student a gift he is not yet able to appreciate.

As a matter of fact, when the teacher sets the goal before his student, he performs a highly qualified job – pinpointing the main idea, communicating everything he knows about it, the significance of its meaning, and the results achieved in the field or even lack thereof, which is equally important. This work can be compared to that of a huntsman, leading the hunter to the right spot, or to the work of a guide in Himalayas.

A good teacher allows his student to find the solution oneself and creates an illusion that the student would achieve the same kind of success in the future, since he was able to overcome significant difficulties. Naturally, the student underestimates the significance of that advance preparation, which takes a completely different set of abilities and qualifications, and whose results he received as a gift from his teacher (in the form of the formulation of the problem and correct methodology).

Even if the student is an extremely gifted mathematician who is able to overcome significant difficulties, the outcome can often be tragic. The student is not able to produce any new results compared to his first outstanding achievement despite all his talent. He is not willing to produce less than brilliant work, and as a result he does nothing. ("Why do our young and talented mathematicians stop growing after having achieved their first success?" Kolmogorov would wonder, while he used to shower his students with his gifts.) If the student is smart enough (not as a mathematician, but as a human being), he starts studying seriously and without chasing an instant success. First he gradually masters his field in general and after that he masters the art of formulation of problems. He either works out his own philosophy or finally realizes the significance of the gift he had received at the beginning and demands from his teacher (or starts looking somewhere else) for another gift of comparable value.

This is how Rokhlin explained the emergence of a multitude of schools of epigoni developing the ideas which were once new and fresh. (He named a whole list of Western and Russian mathematicians. Naturally, I am not going to repeat the names for fear of unintentionally hurting their feelings by forgetfully leaving out some of the names.)

Being an advocate of the purity of the Russian language, V.A. was very sensitive when it came to overusing of bureaucratic jargon. He would quote his teacher, A.I. Plesner (a German mathematician, who escaped Hitler's regime and moved to Russia) and who used to very diligently edit articles for the "Russian Mathematical Surveys" while saying "Your Russian Language grates *in* my ears." With all his brutal honesty Vladimir Abramovich was impeccably polite, especially with younger people and even more so with his students. His self-confidence and dignity prevented him from using any of many commonly used degrading methods of putting a person down. In spite of his passion for polemics, Rokhlin's noble and respectful attitude towards his opponent made him stand out, similar to the one that was admirable in Kolmogorov and Petrovsky.

I remember only one incident when V.A. who was accustomed to always winning, was forced to yield to a superior adversary. It all happened in Tsakhhadzor in 1969.

In that small town 2000 meters above sea level, high in the mountains of Armenia, used to be a training camp for the Olympic team, but at that time a Mathematics winter school was organized, where many fallen out of favor mathematicians gathered (mostly those who signed "The Letter of 99" in 1968 in support of Esenin-Volpin, a mathematician-dissident, who had been confined to a psychiatric hospital by the authorities).

In the morning, as always ignoring all the bans, I set off alone towards the slope for some skiing. As I was reaching the peak (about 3000 meters—now they have installed ski-lifts there, but at the time it was just a scenic spot in the wilderness with an amazing view of Lake Sevan), I got to the edge of the mountain and saw in about 20 meters a reddish boulder sticking out from the snow. It suddenly shook a bit and went rolling and not down the hill toward me, but up the slope. When I looked closely, I realized that it was a bear bolting from me.

It was almost lunch time. After a brisk descent through the virgin snow, I made to the cafeteria just in time – there waiting for me on the table was a steaming hot bowl of kharcho soup. I was about to tell Vladimir Abramovich about my adventure, when I nearly choked. I saw my bear through the glass wall of a pavilion, peacefully wobbling through the town. At that time they used to fatten up bears like piglets in Tsakhkadsor so that they could eat them later (they might still be doing it now).

It turned out that the bear was passionate about splashing out in the jet of water spurting out of the street water well pump. But he only knew how to do one of two things: he could either pull the lever with his paw and let others quench their thirst or he could bathe and drink himself but in that case he needed somebody else to pull the pump lever for him.

After lunch Vladimir Abramovich and I were headed to a lecture and on our way there we met the bear at the water pump. The bear decided to entrust Rokhlin with the duty of pulling the lever. But V.A. was not aware of his habits and didn't seem to want to understand him. He tried to get rid of the bear using certain phrases that would (without insulting a person) convince him to give up any attempts of collaborating with V.A. Still the bear proved to be more persistent than Rokhlin. Accepting of his lack of insight, the bear grabbed Rokhlin's handsome white coat by its lapel with his teeth and dragged majestically elegant, in spite of what was happening, Vladimir Abramovich toward the water pump. (We had trouble explaining to Vladimir Abramovich what was it that the bear wanted from him). In any case, V.A. handled this incident with the bear with his usual both mathematical and non-mathematical undeterred aplomb. After all, he had gone through a lot more than that. (I also think that Rokhlin treated "The case of 99", which shook the whole mathematical hierarchy in Russia, much less tragically and more philosophically than the rest of us who remained hopeful until Khrushchev's "Thaw" collapsed in 1968.

I recall another conversation with Vladimir Abramovich, the subject of which he would return to again and again – his vision of the future of humanity. According to him, humanity is moving towards bureaucratization where an all-powerful bureaucratic apparatus will be suppressing everything alive and creative that still exists. According to him, this phenomenon is not exclusive to Russia, it is global, although this is an uneven process. Rokhlin thought that this process would be soon completed (in view of the fact that two-dimensional sphere is compact), and the Global Government will be created, which will realize the worst predictions of Zamyatin's "We" and Huxley's "Brave New World" on the global scale. Degenerating humanity lead by their worst representatives will democratically establish ochlocratic dictatorship, which will be suppressing everything out of the ordinary and will be mainly preoccupied with stopping progress, and, as a result, destruction of education and science (by means of dumbing down children from a very young age by watching TV, playing video and computer games).

Our times, the golden age of mathematics and science in general will then be considered an unprecedented highest point, the way we now think of Italian Renaissance Art, and Klein's "Lectures on Development of Mathematics in the Nineteenth Century" will read as Vasari's "The Lives of the Artists".

"I am glad I will not live to see that", concluded Rokhlin.

It is difficult to debate such predictions, however I would like to cite a similar prediction by Leo Tolstoy that has not quite become a reality. "The strength of the government lies in the people's ignorance, and government knows this, and will, therefore, always oppose true enlightenment".

A century has already passed, and the education has not been completely wiped out and it gives us a reason to look to the future with a touch of hope.

# Photographs of V. I. Arnold

## 1940s - 1970s



I. E. Tamm tells young Vladimir and Katya Arnold about "makhnovtsy", late 1940s; see Chapter 5.



In Riga, 1949.



An official photo, 1957.



In Palanga, 1953. Left to right: Dmitry Arnold (brother of V. Arnold), Lev Pereslegin (classmate of V. Arnold), Vladimir Arnold, Tatiyana Vainshtein (nee Mandelstam, second cousin of V. Arnold, granddaughter of L. I. Mandelstam, a prominent Soviet physicist).



Members of the Children's Learned Society (DNO), around 1948. Left to right: Andrei Novikov (brother of Sergei Novikov), Vladimir Arnold, Mikhail Zalkind, Dmitry Arnold (brother of V. Arnold), Sergei Novikov (well-known Russian mathematician), Oskar Krauze.

Outdoors, mid-1950s









At Nikolina Gora, with V. Rokhlin; see Chapter 7 for Arnold's memories about Rokhlin.



At Nikolina Gora, near Moscow.



On a hike, the 1960s.



In Kozha, the 1960s.



Painting, 1968.



With A. Kolmogorov (left), mid-1960s.



In Otepya, Estonia, the 1960s.



Ya. G. Sinai and V. Arnold in front of the main building of Moscow State University, 1963.



V. A. Rokhlin gives a lecture, 1960s; see Chapter 7 for Arnold's memories about Rokhlin.



President of the Moscow State University, I. G. Petrovsky (middle) with A. Kirillov and V. Arnold, around 1960.



Arnold with students at Kolmogorov's mathematical boarding school, the 1960s.



Lecturing in Syktyvkar, 1976; see Chapter 14 for Eliashberg's memories of Arnold.

## 1980s - 1990s



Course lecture on ordinary differential equations, Moscow State University, 1983; see Chapter 13 for Ilyashenko's memories of Arnold.



At Moscow State University, circa 1985.



In Yosemite, California, 1989.



On the Golden Gate Bridge, 1989.



In the woods near New Haven, CT, 1993.



Swimming in November, New Haven, CT, 1993.



Giving Bowen Lectures, Berkeley, 1997.



With Jürgen Moser at the Euler Institute, St. Petersburg, Russia, 1991.



At The Fields Institute, Toronto, 1997.

## Speakers at Arnoldfest, Fields Institute, Toronto, 1997



**Back:** Y. Yomdin, V. Arnold, J. Marsden; **Front:** B. Khesin, R. de la Llave, A. Varchenko.



**Back:** E. Bierstone, T. Ratiu, Ya. Eliashberg, A. Givental, A. Neistadt; **Front:** V. Vassiliev, Yu. Ilyashenko, D. Fuchs.



With his wife Elya.



With Yu. Chekanov, V. Zakalyukin, A. Khovanskii.



With Ya. Eliashberg.



With B. Khesin.



With his son Igor and Igor's wife Yulya.



On Niagara Falls with A. Gabrielov, V. Zakalyukin, and A. Khovanskii.



With A. Khovanskii and I. Scherbak.



Receiving the Doctor Honoris Causa Degree from the University of Toronto, June 1997.



With his wife Elya at the ceremony, University of Toronto, June 1997.



With Miles Reed at the ceremony, University of Toronto, June 1997.

# The 2000s


In Paris.



At the Globus seminar, Independent University of Moscow.



At the Mathematical Olympiad "Arnold's problems for all ages", with V. Kleptsyn, S. Gusein-Zade, S. Lando.



The award ceremony of a Moscow Mathematical Olympiad.

# Lecturing







International Summer School "Modern Mathematics" in Dubna, near Moscow with B. Khesin.



International Summer School "Modern Mathematics" in Dubna, near Moscow with a participant of the school.



A cavern in Italy; see Chapter 12 for Fuchs' memories of Arnold.

# Portraits











V. I. Arnold's tombstone at the Novodevichy Cemetery in  $$\operatorname{Moscow}$$ 

The inscription on the tombstone is from Arnold's paper about Pushkin. It reads: "As a mathematician, I constantly have to base my work not on formal proofs but on feelings, guesses, and conjectures, going from one fact to another by means of a special kind of illumination which allows me to see common traits in phenomena that to an outsider appear totally unrelated."

# Part 2

# About Arnold

## To Whom It May Concern

ALEXANDER GIVENTAL

Но есть и Божий суд ... М. Ю. Лермонтов, "Смерть Поэта"<sup>1</sup>

Posthumous memoirs seem to have the unintended effect of reducing the person's life to a collection of stories. For most of us it would probably be a just and welcome outcome, but for Vladimir Arnold, I think, it would not. He tried and managed to tell us many different things about mathematics, education, and beyond, and in many cases we've been rather slow listening or thinking, so I believe we will be returning again and again not only to our memories of him but to his own words as well. What is found below is not a memoir, but a recommendation letter, albeit a weak one, for he did *not* get the prize, and yet hopefully useful as an interim attempt to overview his mathematical heritage.

January 25, 2005

Dear Members of [the name of the committee],

You have requested my commentaries on the work of Vladimir Arnold. Writing them is an honorable and pleasurable task for me.

In the essence the task is easy:

Yes, Vladimir Arnold fully merits [the name of the prize] since his achievements are of extraordinary depth and influence.

His work indeed resolves fundamental problems, *and* introduces unifying principles, *and* opens up major new areas, *and* (at least in some of these areas) it introduces powerful new techniques too.

On the other hand, writing this letter is not easy, mainly because the ways Arnold's work contributes to our knowledge are numerous and go far beyond my personal comprehension. As Arnold's student, I am quite familiar with those aspects of his work which inspired my own research. Outside these areas, hopefully, I will be able to convey the conventional wisdom about Arnold's most famous achievements. Yet this leaves out the ocean of numerous, possibly less famous but extremely influential, contributions, of which I have only partial knowledge and understanding. So, I will have to be selective here and will mention just a handful of examples which I am better familiar with and which for this reason may look chosen randomly.

Originally published in the Notices of the American Mathematical Society, 59 (2012), no. 3. Alexander Givental is professor of mathematics at the University of California, Berkeley.

<sup>&</sup>lt;sup>1</sup> Yet, there is God's Court, too..., M. Yu. Lermontov, "Death of Poet".

Perhaps the most legendary, so to speak, of Arnold's contributions is his work on **small denominators**,<sup>2</sup> followed by the discovery of *Arnold's diffusion*,<sup>3</sup> and known now as part of the Kolmogorov–Arnold–Moser theory. Among other things, this work contains an explanation (or, depending on the attitude, a proof, and a highly technical one) of stability of the solar planetary system. Even more importantly, the KAM theory provides a very deep insight into the real-world dynamics (perhaps one of the few such insights so far, one more being stability of Anosov's systems) and is widely regarded as one of the major discoveries of twentieth-century mathematical physics.

**Symplectic geometry** has established itself as a universal geometric language of Hamiltonian mechanics, calculus of variations, quantization, representation theory and microlocal analysis of differential equations. One of the first mathematicians who understood the unifying nature of symplectic geometry was Vladimir Arnold, and his work played a key role in establishing this status of symplectic geometry. In particular, his monograph *Mathematical Methods of Classical Mechanics*<sup>4</sup> has become a standard textbook, but thirty years ago it indicated a paradigm shift in a favorite subject of physicists and engineers. The traditional "analytical" or "theoretical" mechanics got suddenly transformed into an active region of modern mathematics populated with Riemannian metrics, Lie algebras, differential forms, fundamental groups, and symplectic manifolds.

Just as much as symplectic geometry is merely a language, **symplectic topol**ogy is a profound problem. Many of the best results of such powerful mathematicians as Conley, Zehnder, Gromov, Floer, Hofer, Eliashberg, Polterovich, McDuff, Salamon, Fukaya, Seidel, and a number of others belong to this area. It would not be too much of an overstatement to say that symplectic topology has developed from attempts to solve a single problem: to prove the Arnold conjecture about Hamiltonian fixed points and Lagrangian intersections.<sup>5</sup> While the conjecture has been essentially proved<sup>6</sup> and many new problems and ramifications discovered, the theory in a sense continues to explore various facets of that same topological rigidity property of phase spaces of Hamiltonian mechanics that goes back to Poincaré and Birkhoff and whose symplectic nature was first recognized by Arnold in his 1965 notes in *Comptes Rendus*.

<sup>&</sup>lt;sup>2</sup>Small denominators III. Problems of stability of motion in classical and celestial mechanics, Uspekhi Mat. Nauk **18** (1963), no. 6, 91–192, following Small denominators I. Mappings of a circle onto itself, Izvestia AN SSSR, Ser. Mat. **25** (1961), 21–86, Small denominators II. Proof of a theorem of A. N. Kolmogorov on the preservation of conditionally periodic motions under a small perturbation of the Hamiltonian, Uspekhi Mat. Nauk **18** (1963), no. 5, 13–40, and a series of announcements in DAN SSSR.

 $<sup>^{3}</sup>Instability$  of dynamical systems with many degrees of freedom, DAN SSSR **156** (1964), 9–12.

<sup>&</sup>lt;sup>4</sup>Nauka, Moscow, 1974.

<sup>&</sup>lt;sup>5</sup>First stated in Sur une propriété topologique des applications globalement canoniques de la mécanique classique, C. R. Acad. Sci. Paris **261** (1965), 3719–3722, and reiterated in a few places, including an appendix to Math Methods....

<sup>&</sup>lt;sup>6</sup>By Hofer (1986) for Lagrangian intersections and by Fukaya–Ono (1996) for Hamiltonian diffeomorphisms, while "essentially" refers to the fact that the conjectures the way Arnold phrased them in terms of critical point of functions rather than (co)homology, and especially in the case of *possibly degenerate* fixed or intersection points, still remain open (and correct just as likely as not, but with no counterexamples in view).

Arnold's work in Riemannian geometry of infinite-dimensional Lie groups had almost as much of a revolutionizing effect on hydrodynamics as his work in small denominators produced in classical mechanics. In particular, Arnold's seminal paper in *Annales de L'Institut Fourier*<sup>7</sup> draws on his observation that flows of incompressible fluids can be interpreted as geodesics of right-invariant metrics on the groups of volume-preserving diffeomorphisms. Technically speaking, the aim of the paper is to show that most of the sectional curvatures of the area-preserving diffeomorphism group of the standard 2-torus are negative and thus the geodesics on the group typically diverge exponentially. From time to time this result makes the news as a "mathematical proof of impossibility of long-term weather forecasts". More importantly, the work had set Euler's equations on coadjoint orbits as a blueprint and redirected the attention in many models of continuum mechanics toward symmetries, conservation laws, relative equilibria, symplectic reduction, topological methods (in works of Marsden, Ratiu, Weinstein, Moffat, and Freedman among many others).<sup>8</sup>

Due to the ideas of Thom and Pham and fundamental results of Mather and Malgrange, **singularity theory** became one of the most active fields of the seventies and eighties, apparently with two leading centers: Brieskorn's seminar in Bonn and Arnold's seminar in Moscow. The theory of critical points of functions and its applications to classification of singularities of caustics, wave fronts and short-wave asymptotics in geometrical optics as well as their relationship with the ADE-classification are perhaps the most famous (among uncountably many other) results of Arnold in singularity theory.<sup>9</sup> Arnold's role in this area went, however, far beyond his own papers.

Imagine a seminar of about thirty participants: undergraduates writing their first research papers, graduate students working on their dissertation problems, postgraduates employed elsewhere as software engineers but unwilling to give up their dream of pursuing mathematics even if only as a hobby, several experts-Fuchs, Dolgachev, Gabrielov, Gusein-Zade, Khovansky, Kushnirenko, Tyurin, Varchenko, Vassiliev-and the leader, Arnold-beginning each semester by formulating a bunch of new problems, giving talks or listening to talks, generating and generously sharing new ideas and conjectures, editing his students' papers, and ultimately remaining the only person in his seminar who would keep in mind everyone else's works-in-progress and understand their relationships. Obviously, a lion's share of his students' achievements (and among the quite famous ones are the theory of Newton polyhedra by Khovansky and Kushnirenko or Varchenko's results on asymptotical mixed Hodge structures and semicontinuity of Steenbrink spectra) is due to his help, typically in the form of working conjectures, but every so often through his direct participation (for, with the exception of surveys and obituaries, Arnold would refuse to publish joint papers—we will learn later why).

<sup>&</sup>lt;sup>7</sup>Sur la géométrie différentielle des groupes de Lie de dimension infinie et ses applications à l'hydrodynamique des fluides parfaits, Ann. Inst. Fourier **16**:1 (1966), 319–361, based on a series of earlier announcements in C. R. Acad. Sci. Paris.

<sup>&</sup>lt;sup>8</sup>As summarized in the monograph *Topological Methods in Hydrodynamics*, Springer-Verlag, 1998, by Arnold and Khesin.

<sup>&</sup>lt;sup>9</sup>Normal forms of functions near degenerate critical points, the Weyl groups  $A_k, D_k, E_k$  and Lagrangian singularities, Funct. Anal. Appl. **6**, no. 4 (1972), 3–25; see also Arnold's inspiring paper in Proceedings of the ICM-74 Vancouver and the textbooks Singularities of Differential Maps, Vols. I and II, by Arnold, Gusein-Zade and Varchenko.

#### ALEXANDER GIVENTAL

Moreover, under Arnold's influence, the elite branch of topology and algebraic geometry studying singular real and complex hypersurfaces was transformed into a powerful tool of applied mathematics dealing with degenerations of all kinds of mathematical objects (metamorphoses of wave fronts and caustics, evolutes, evolvents and envelopes of plane curves, phase diagrams in thermodynamics and convex hulls, accessibility regions in control theory, differential forms and Pfaff equations, symplectic and contact structures, solutions of Hamilton-Jacobi equations, the Hamilton-Jacobi equations themselves, the boundaries between various domains in functional spaces of all such equations, etc.) and merging with the **the**ory of bifurcations (of equilibria, limit cycles, or more complicated attractors in ODEs and dynamical systems). Arnold had developed a unique intuition and expertise in the subject, so that when physicists and engineers would come to him asking what kind of **catastrophes** they should expect in their favorite problems, he would be able to guess the answers in small dimensions right on the spot. In this regard, the situation would resemble experimental physics or chemistry, where personal expertise is often more important than formally registered knowledge.

Having described several (frankly, quite obvious) broad areas of mathematics reshaped by Arnold's seminal contributions, I would like to turn now to some more specific classical problems which attracted his attention over a long time span.

The affirmative solution of the **13th Hilbert problem** (understood as a question about superpositions of *continuous* functions) given by Arnold in his early (essentially undergraduate) work<sup>10</sup> was the beginning of his interest in the "genuine" (and still open) Hilbert's problem: Can the root of the general degree 7 polynomial considered as an algebraic function of its coefficients be written as a superposition of *algebraic* functions of 2 variables? The negative<sup>11</sup> solution to the more general question about polynomials of degree n was given by Arnold in 1970 for  $n = 2^{r}$ .<sup>12</sup> The result was generalized by V. Lin. Furthermore, Arnold's approach, based on his previous study of cohomology of braid groups, later gave rise to Smale's concept of topological complexity of algorithms and Vassiliev's results on this subject. Even more importantly, Arnold's study of braid groups via topology of configuration spaces<sup>13</sup> was generalized by Brieskorn to E. Artin's braid groups associated with reflection groups. The latter inspired Orlik-Solomon's theory of hyperplane arrangements, K. Saito–Terao's study of free divisors, Gelfand's approach to hypergeometric functions, Aomoto's work on Yang-Baxter equations, and Varchenko-Schekhtman's hypergeometric "Bethe ansatz" for solutions of Knizhnik-Zamolodchikov equations in conformal field theory.

Arnold's result<sup>14</sup> on the **16th Hilbert problem, Part I**, about disposition of ovals of real plane algebraic curves, was immediately improved by Rokhlin (who applied Arnold's method but used more powerful tools from the topology of 4manifolds). This led Rokhlin to his proof of a famous conjecture of Gudkov (who corrected Hilbert's expectations in the problem), inspired many new developments

 $<sup>^{10}</sup>$  On the representations of functions of several variables as a superposition of functions of a smaller number of variables, Mat. Prosveshchenie (1958), 41–46.

<sup>&</sup>lt;sup>11</sup>That is, positive in Hilbert's sense.

<sup>&</sup>lt;sup>12</sup> Topological invariants of algebraic functions. II, Funct. Anal. Appl. 4 (1970), no. 2, 1–9.

<sup>&</sup>lt;sup>13</sup> The cohomology ring of the group of dyed braids, Mat. Zametki 5 (1969), 227–231.

 $<sup>^{14}</sup>$  The situation of ovals of real algebraic plane curves, the involutions of four-dimensional smooth manifolds, and the arithmetic of integral quadratic forms, Funct. Anal. Appl. 5 (1971), no. 3, 1–9.

(due to Viro and Kharlamov among others), and is considered a crucial breakthrough in the history of real algebraic geometry.

Among other things, the paper of Arnold outlines an explicit diffeomorphism between  $S^4$  and the quotient of  $\mathbb{C}P^2$  by complex conjugation.<sup>15</sup> The fact was rediscovered by Kuiper in 1974 and is known as textitKuiper's theorem [5]. Arnold's argument, based on hyperbolicity of the discriminant in the space of Hermitian forms, was recently revived in a far-reaching paper by M. Atiyah and J. Berndt [2].

Another work of Arnold in the same field<sup>16</sup> unified the Petrovsky-Oleinik inequalities concerning topology of real hypersurfaces (or their complements) and brought mixed Hodge structures (just introduced by Steenbrink into complex singularity theory) into real algebraic geometry.

Arnold's interest in the 16th Hilbert problem, Part II, on the number of limit cycles of polynomial ODE systems on the plane has been an open-ended search for simplifying formulations. One such formulation<sup>17</sup> (about the maximal number of limit cycles born under a nonconservative perturbation of a Hamiltonian system and equivalent to the problem about the number of zeroes of Abelian integrals over a family of real algebraic ovals) generated extensive research. The results here include the general deep finiteness theorems of Khovansky and Varchenko, Arnold's conjecture about nonoscillatory behavior of the Abelian integrals, his geometrization of higher-dimensional Sturm theory of (non)oscillations in linear Hamiltonian systems,<sup>18</sup> various attempts to prove this conjecture (including a series of papers by Petrov-Tan'kin on Abelian integrals over elliptic curves, my own application of Sturm's theory to nonoscillation of hyperelliptic integrals, and more recent estimates of Grigoriev, Novikov-Yakovenko), and further work by Horozov, Khovansky, Ilyashenko and others. Yet another modification of the problem (a discrete one-dimensional analogue) suggested by Arnold led to a beautiful and nontrivial theorem of Yakobson in the theory of dynamical systems [9].

The classical problem in the theory of Diophantine approximations of inventing the **higher-dimensional analogue of continued fractions** has been approached by many authors, with a paradoxical outcome: there are many relatively straightforward and relatively successful generalizations, but none as unique and satisfactory as the elementary continued fraction theory. Arnold's approach to this problem<sup>19</sup> is based on his discovery of a relationship between graded algebras and Klein's *sails* (i.e., convex hulls of integer points inside simplicial convex cones in Euclidean spaces). Arnold's problems and conjectures on the subject have led to the results of E. Korkina and G. Lauchaud generalizing Lagrange's theorem (which identifies quadratic irrationalities with eventually periodic continued fractions) and to the work of Kontsevich–Sukhov generalizing Gauss's dynamical system and its ergodic

<sup>&</sup>lt;sup>15</sup>Details were published much later in *The branched covering*  $CP^2 \rightarrow S^4$ , hyperbolicity and projective topology, Sibirsk. Mat. Zh. **29** (1988), no. 5, 36–47.

<sup>&</sup>lt;sup>16</sup> The index of a singular point of a vector field, the Petrovsky-Oleinik inequalities, and mixed Hodge structures, Funct. Anal. Appl. **12** (1978), no. 1, 1–14.

<sup>&</sup>lt;sup>17</sup>V. I. Arnold, O. A. Oleinik, *Topology of real algebraic varieties*, Vestnik Moskov. Univ. Ser. I Mat. Mekh. (1979), no. 6, 7–17.

<sup>&</sup>lt;sup>18</sup>Sturm theorems and symplectic geometry, Funct. Anal. Appl. **19** (1985), no. 4, 1–10.

<sup>&</sup>lt;sup>19</sup>A-graded algebras and continued fractions, Commun. Pure Appl. Math. **49** (1989), 993– 1000; *Higher-dimensional continued fractions*, Regul. Chaotic Dyn. 3 (1998), 10–17; and going back to *Statistics of integral convex polyhedra*, Funct. Anal. Appl. **14** (1980), no. 1, 1–3; and to the theory of Newton polyhedra.

properties. Thus the Klein-Arnold generalization, while not straightforward, appears to be just as unique and satisfactory as its classical prototype.

The above examples show how Arnold's interest in specific problems helped to transform them into central areas of modern research. There are other classical results which, according to Arnold's intuition, are scheduled to generate such new areas, but to my understanding have not yet achieved the status of important mathematical theories in spite of interesting work done by Arnold himself and some others. But who knows? To mention one: the Four-Vertex Theorem, according to Arnold, is the seed of a new (yet unknown) branch of topology (in the same sense as the Last Poincaré Theorem was the seed of symplectic topology). Another example: a field-theoretic analogue of Sturm theory, broadly understood as a study of topology of zero levels (and their complements) of eigenfunctions of selfadjoint linear partial differential operators.

Perhaps with the notable exceptions of KAM-theory and singularity theory, where Arnold's contributions are marked not only by fresh ideas but also by technical breakthroughs (e.g., a heavy-duty tool in singularity theory—his spectral sequence),<sup>20</sup> a more typical path for Arnold would be to invent a bold new problem, attack its first nontrivial cases with his bare hands, and then leave developing an advanced machinery to his followers. I have already mentioned how the theory of hyperplane arrangements emerged in this fashion. Here are some other examples of this sort where Arnold's work starts a new area.

In 1980 Arnold invented the concepts of **Lagrangian and Legendrian cobordisms** and studied them for curves using his theory of bifurcations of wave fronts and caustics.<sup>21</sup> The general homotopy theory formulation was then given by Ya. Eliashberg, and the corresponding "Thom rings" computed in an award-winning treatise by M. Audin [**3**].

A geometric realization of Lagrange and Legendre characteristic numbers as the enumerative theory of singularities of global caustics and wave fronts was given by V. Vassiliev [8].

The method developed for this task, namely associating a spectral sequence to a stratification of functional spaces of maps according to types of singularities, was later applied by Vassiliev several more times, of which his work on *Vassiliev invariants of knots* is the most famous one.

Arnold's definition<sup>22</sup> of the **asymptotic Hopf invariant** as the average selflinking number of trajectories of a volume-preserving flow on a simply connected 3-fold and his "ergodic" theorem about coincidence of the invariant with Moffatt's *helicity* gave the start to many improvements, generalizations, and applications of topological methods in hydro- and magneto-dynamics due to M. H. Freedman et al., É. Ghys, B. Khesin, K. Moffatt, and many others.<sup>23</sup>

 $<sup>^{20}</sup>A$  spectral sequence for the reduction of functions to normal forms, Funct. Anal. Appl. 9 (1975), 81–82.

 $<sup>^{21}</sup>Lagrange$  and Legendre cobordisms. I, II, Funct. Anal. Appl. 14 (1980), no. 3, 1–13; no. 4, 8–17.

 $<sup>^{22}</sup>$ See The asymptotic Hopf invariant and its applications, Selected translations. Selecta Math. Soviet. **5** (1986), no. 4, 327–345, which is the translation of a 1973 paper and one of Arnold's most frequently quoted works.

<sup>&</sup>lt;sup>23</sup>See a review in Chapter III of V. I. Arnold, B. A. Khesin, *Topological methods in hydrody*namics, Applied Math. Sciences, vol. 125, Springer-Verlag, NY, 1998.

As one can find out, say, on MathSciNet, Arnold is one of the most prolific mathematicians of our time. His high productivity is partly due to his fearless curiosity and enormous appetite for new problems.<sup>24</sup> Paired with his taste and intuition, these qualities often bring unexpected fruit, sometimes in the areas quite remote from the domain of his direct expertise. Here are some examples.

Arnold's observation<sup>25</sup> on the pairs of triples of numbers computed by I. Dolgachev and A. Gabrielov and characterizing respectively uniformization and monodromy of 14 exceptional unimodal singularities of surfaces (in Arnold's classification) is known now under the name **Arnold's Strange Duality**. In 1977, due to Pinkham and Dolgachev–Nikulin, the phenomenon received a beautiful explanation in terms of geometry of K3-surfaces. As became clear in the early nineties, Arnold's Strange Duality was the first, and highly nontrivial, manifestation of **Mirror Symmetry**: a profound conjecture discovered by string theorists and claiming a sort of equivalence between symplectic topology and complex geometry (or singularity theory).

Arnold's work in pseudo-periodic geometry<sup>26</sup> encouraged A. Zorich to begin a systematic study of dynamics on Riemann surfaces defined by levels of closed 1-forms, which led to a number of remarkable results of Kontsevich–Zorich [7] and others related to ergodic theory on Teichmüller spaces and conformal field theory, and of Eskin–Okounkov [4] in the Hurwitz problem of counting ramified covers over elliptic curves.

Arnold seems to be the first to suggest<sup>27</sup> that monodromy (say of Milnor fibers or of flag varieties) can be realized by symplectomorphisms. The idea, picked up by M. Kontsevich and S. Donaldson, was upgraded to the monodromy action on the Fukaya category (consisting of all Lagrangian submanifolds in the fibers and of their Floer complexes). This construction is now an important ingredient of the Mirror Symmetry philosophy and gave rise to the remarkable results of M. Khovanov and P. Seidel about faithfulness of such Hamiltonian representations of braid groups [6].

The celebrated Witten's conjecture proved by M. Kontsevich in 1991 characterizes intersection theory on Deligne–Mumford moduli spaces of Riemann surfaces in terms of KdV-hierarchy of integrable systems. A refreshingly new proof of this result was recently given by Okounkov–Pandharipande. A key ingredient in their argument is an elementary construction of Arnold from his work on enumerative geometry of trigonometric polynomials.<sup>28</sup>

Among many concepts owing Arnold their existence, let me mention two of general mathematical stature which do not carry his name.

 <sup>&</sup>lt;sup>24</sup>See the unusual book Arnold's Problems, Springer-Verlag, Berlin; PHASIS, Moscow, 2004.
<sup>25</sup>See Critical points of smooth functions, Proceedings of the International Congress of Mathematicians (Vancouver, B. C., 1974), Vol. 1, pp. 19–39.

<sup>&</sup>lt;sup>26</sup> Topological and ergodic properties of closed 1-forms with incommensurable periods, Funct. Anal. Appl. **25** (1991), no. 2, 1–12.

<sup>&</sup>lt;sup>27</sup>See Some remarks on symplectic monodromy of Milnor fibrations, The Floer memorial volume, 99–103, Progr. Math., 133, Birkhäuser, Basel, 1995.

 $<sup>^{28}</sup>$  Topological classification of complex trigonometric polynomials and the combinatorics of graphs with an identical number of vertices and edges, Funct. Anal. Appl. **30** (1996), no. 1, 1–17.

One is the **Maslov index**, which proved to be important in geometry, calculus of variations, numbers theory, representation theory, quantization, index theory of differential operators, and whose topological origin was explained by Arnold.<sup>29</sup>

The other one is the geometric notion of **integrability in Hamiltonian sys**tems. There is a lot of controversy over which of the known integrability mechanisms is most fundamental, but there is a consensus that integrability means a complete set of Poisson-commuting first integrals.

This definition and "Liouville's Theorem" on geometric consequences of the integrability property (namely, foliation of the phase space by Lagrangian tori) are in fact Arnold's original inventions.

Similar to the case with integrable systems, there are other examples of important developments which have become so common knowledge that Arnold's seminal role eventually became invisible. Let me round up these comments with a peculiar example of this sort.

The joint 1962 paper of Arnold and Sinai<sup>30</sup> proves structural stability of hyperbolic linear diffeomorphisms of the 2-torus. Their idea, picked up by Anosov, was extended to his famous general stability theory of *Anosov systems* [1]. Yet, according to Arnold, the paper is rarely quoted, for the proof contained a mistake (although each author's contribution was correct, so that neither one could alone be held responsible). By the way, Arnold cites this episode as the reason why he refrained from writing joint research papers.

To reiterate what I said at the beginning, Vladimir Arnold has made outstanding contributions to many areas of pure mathematics and its applications, including those I described above and those I missed: classical and celestial mechanics, cosmology and hydrodynamics, dynamical systems and bifurcation theory, ordinary and partial differential equations, algebraic and geometric topology, number theory and combinatorics, real and complex algebraic geometry, symplectic and contact geometry and topology, and perhaps some others. I can think of few mathematicians whose work and personality would influence the scientific community at a comparable scale. And beyond this community, Arnold is a highly visible (and possibly controversial) figure, the subject of several interviews, of a recent documentary movie, and even of the night sky show, where one can watch an asteroid, *Vladarnolda*, named after him.

I am sure there are other mathematicians who also deserve [the name of the prize], but awarding it to Vladimir Arnold will hardly be perceived by anyone as a mistake.

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## **Remembering Vladimir Arnold: Early Years**

## Yakov Sinai

De mortuis veritas<sup>1</sup>

My grandparents and Arnold's grandparents were very close friends since the beginning of the twentieth century. Both families lived in Odessa, which was a big city in the southern part of Russia and now is a part of Ukraine. At that time, Odessa was a center of Jewish intellectual life, which produced many scientists, musicians, writers, and other significant figures.

My maternal grandfather, V. F. Kagan, was a well-known geometer who worked on the foundations of geometry. During World War I, he gave the very first lecture course in Russia on the special relativity theory. At various times his lectures were attended by future famous physicists L. I. Mandelshtam, I. E. Tamm, and N. D. Papaleksi. In the 1920s all these people moved to Moscow.

L. I. Mandelshtam was a brother of Arnold's maternal grandmother. He was the founder and the leader of a major school of theoretical physics that included A. A. Andronov, G. S. Landsberg, and M. A. Leontovich, among others. A. A. Andronov is known to the mathematical community for his famous paper "Robust systems", coauthored with L. S. Pontryagin, which laid the foundations of the theory of structural stability of dynamical systems. A. A. Andronov was the leader of a group of physicists and mathematicians working in Nizhny Novgorod, formerly Gorky, on nonlinear oscillations. M. A. Leontovich was one of the leading physicists in the Soviet Union. In the 1930s he coauthored with A. N. Kolmogorov the well-known paper on the Wiener sausage. I. E. Tamm was a Nobel Prize winner in physics in the fifties. N. D. Papaleksi was a great expert on nonlinear optics.

V. I. Arnold was born in Odessa, where his mother had come for a brief visit with her family. She returned to Moscow soon after her son's birth. When Arnold was growing up, the news that his family had a young prodigy soon became widely known. In those days, when we were both in high school, we did not really know each other. On one occasion, Arnold visited my grandfather to borrow a mathematics book, but I was not there at the time. We met for the first time when we were both students at the mathematics department of the Moscow State University; he was walking by with Professor A. G. Vitushkin, who ran a freshman seminar on real analysis, and Arnold was one of the most active participants. When Arnold was a third-year undergraduate student, he was inspired by A. N. Kolmogorov to work on superposition of functions of several variables and the related Hilbert's

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<sup>&</sup>lt;sup>1</sup>About those who have died, only the truth.

thirteenth problem. Eventually this work became Arnold's Ph.D. thesis. When I visited the University of Cambridge recently, I was very pleased to learn that one of the main lecture courses there was dedicated to Arnold's and Kolmogorov's work on Hilbert's thirteenth problem.

Arnold had two younger siblings: a brother, Dmitry, and a sister, Katya, who was the youngest. The family lived in a small apartment in the center of Moscow. During one of my visits, I was shown a tent in the backyard of the building where Arnold used to spend his nights, even in cold weather. It seems likely that Arnold's excellent knowledge of history and geography of Moscow, which many of his friends remember with admiration, originated at that time.

Like me, Arnold loved nature and the outdoors. We did hiking and mountain climbing together. Since I knew Arnold so closely, I often observed that his ideas both in science and in life came to him as revelations. I remember one particular occasion, when we were climbing in the Caucasus Mountains and spent a night with some shepherds in their tent. In the morning we discovered that the shepherds were gone and had left us alone with their dogs. Caucasian dogs are very big, strong, and dangerous, for they are bred and trained to fight wolves. We were surrounded by fiercely barking dogs, and we did not know what to do. Then, all of a sudden, Arnold had an idea. He started shouting very loudly at the dogs, using all the obscenities he could think of. I never heard him use such language either before or after this incident, nor did anybody else. It was a brilliant idea, for it worked! The dogs did not touch Arnold and barely touched me. The shepherds returned shortly afterwards, and we were rescued.

On another occasion, roughly at the same time, as Anosov, Arnold, and I walked from the main Moscow University building to a subway station, which usually took about fifteen minutes, Arnold told us that he recently came up with the Galois theory entirely on his own and explained his approach to us. The next day, Arnold told us that he found a similar approach in the book by Felix Klein on the mathematics of the nineteenth century. Arnold was always very fond of this book, and he often recommended it to his students.

Other examples of Arnold's revelations include his discovery of the Arnold-Maslov cocycle in the theory of semi-classical approximations and Arnold inequalities for the number of ovals in real algebraic curves. Many other people who knew Arnold personally could provide more examples of this kind.

Arnold became a graduate student at the Moscow State University in 1959. Naturally it was A. N. Kolmogorov who became his advisor. In 1957 Kolmogorov gave his famous lecture course on dynamical systems, which played a pivotal role in the subsequent development of the theory. The course was given three years after Kolmogorov's famous talk at the Amsterdam Congress of Mathematics.

Kolmogorov began his lectures with the exposition of the von Neumann theory of dynamical systems with pure point spectrum. Everything was done in a pure probabilistic way. Later Kolmogorov found a similar approach in the book by Fortet and Blank–Lapierre on random processes, intended for engineers.

This part of Kolmogorov's lectures had a profound effect on researchers working on the measure-theoretic isomorphism in dynamical systems, a long-standing problem that goes back to von Neumann. It was shown that when the spectrum is a pure point one, it is the only isomorphism invariant of a dynamical system and that two systems with the same pure point spectrum are isomorphic. The excitement around these results was so profound that people began to believe that the isomorphism theory of systems with continuous spectrum would be just a straightforward generalization of the theory of systems with pure point spectrum. However, this was refuted by Kolmogorov himself. He proposed the notion of entropy as a new isomorphism invariant for systems with continuous spectrum. Since the entropy is zero for systems with pure point spectrum, it does not distinguish between such systems, but systems with continuous spectrum might have positive entropy that must be preserved by isomorphisms. This was a path-breaking discovery, which had a tremendous impact on the subsequent development of the theory.

The second part of Kolmogorov's lectures was centered around his papers on the preservation of invariant tori in small perturbations of integrable Hamiltonian systems, which were published in the *Doklady* of the Soviet Academy of Sciences. Unfortunately there were no written notes of these lectures. V. M. Tikhomirov, one of Kolmogorov's students, hoped for many years to locate such notes, but he did not succeed. Arnold used to claim in his correspondence with many people that good mathematics students of Moscow University could reconstruct Kolmogorov's proof from the text of his papers in the *Doklady*. However, this was an exaggeration. Recently two Italian mathematicians, A. Giorgilli and L. Chierchia, produced a proof of Kolmogorov's theorem, which was complete and close to Kolmogorov's original proof, as they claimed.

Apparently Kolmogorov himself never wrote a detailed proof of his result. There might be several explanations. At some point, he had plans to work on applications of his technique to the famous three-body problem. He gave a talk on this topic at a meeting of the Moscow Mathematical Society. However, he did not prepare a written version of his talk. Another reason could be that Kolmogorov started to work on a different topic and did not want to be distracted. There might be a third reason, although some people would disagree with it. It is possible that Kolmogorov underestimated the significance of his papers. For example, some graduate exams on classical mechanics included the proof of Kolmogorov's theorem, so it was easy to assume that the proof was already known. The theory of entropy, introduced by Kolmogorov roughly at the same time, seemed a hotter and more exciting area. He might have felt compelled to turn his mind to this new topic.

Arnold immediately started to work on all the problems raised in Kolmogorov's lectures. In 1963 the Moscow Mathematical Society celebrated Kolmogorov's sixtieth birthday. The main meeting took place in the Ceremony Hall of the Moscow State University, with about one thousand people attending. The opening lecture was given by Arnold on what was later called KAM theory, where KAM stands for Kolmogorov, Arnold, Moser. For that occasion, Arnold prepared the first complete exposition of the Kolmogorov theorem. I asked Arnold why he did that, since Kolmogorov presented his proof in his lectures. Arnold replied that the proof of the fact that invariant tori constitute a set of positive measure was not complete. When Arnold asked Kolmogorov about some details of his proof, Kolmogorov replied that he was too busy at that time with other problems and that Arnold should provide the details by himself. This was exactly what Arnold did. I believe that when Kolmogorov prepared his papers for publication in the *Doklady*, he did have complete proofs, but later he might have forgotten some details. Perhaps it can be expressed better by saying that it required from him an effort that he was not prepared to make at that time.

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In the following years, Kolmogorov ran a seminar on dynamical systems, with the participation of many mathematicians and physicists. At some point, two leading physicists, L. A. Arzimovich and M. A. Leontovich, gave a talk at the seminar on the existence of magnetic surfaces. Subsequently this problem was completely solved by Arnold, who submitted his paper to the main physics journal in the Soviet Union, called JETP. After some time, the paper was rejected. According to Arnold, the referee report said that the referee did not understand anything in that paper and hence nobody else would understand it. M. A. Leontovich helped Arnold to rewrite his paper in the form accessible to physicists, and it was published eventually. According to Arnold, this turned out to be one of his most quoted papers.

Arnold's first paper related to the KAM theory was about smooth diffeomorphisms of the circle that were close to rotations. Using the methods of the KAM theory, Arnold proved that such diffeomorphisms can be reduced to rotations by applying smooth changes of variables. The problem in the general case was called the Arnold problem. It was completely solved by M. Herman and J.-C. Yoccoz.

A. N. Kolmogorov proved his theorem in the KAM theory for the so-called nondegenerate perturbations of integrable Hamiltonian systems. Arnold extended this theorem to degenerate perturbations, which arise in many applications of the KAM theory.

Arnold proposed an example of the Hamiltonian system which exhibits a new kind of instability and which was later called the Arnold diffusion. The Arnold diffusion appears in many physical problems. New mathematical results on the Arnold diffusion were recently proved by J. Mather. V. Kaloshin found many applications of the Arnold diffusion to problems of celestial mechanics.

In later years Arnold returned to the theory of dynamical systems only occasionally. One can mention his results in fluid mechanics (see his joint book with B. Khesin [1]) and a series of papers on singularities in the distribution of masses in the universe, motivated by Y. B. Zeldovich. But all this was done in later years.

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# Vladimir I. Arnold

## Steve Smale

My first meeting with V. I. Arnold took place in Moscow in September 1961 (certainly I had been very aware of him through Moser). After a conference in Kiev, where I had gotten to know Anosov, I visited Moscow, where Anosov introduced me to Arnold, Novikov, and Sinai. As I wrote later [2], I was extraordinarily impressed by such a powerful group of four young mathematicians and that there was nothing like that in the West. At my next visit to Moscow for the world mathematics congress in 1966 [3], I again saw much of Dima Arnold. At that meeting he introduced me to Kolmogorov.

Perhaps the last time I met Dima was in June 2003 at the one-hundred-year memorial conference for Kolmogorov, again in Moscow. In the intervening years we saw each other on a number of occasions in Moscow, in the West, and even in Asia.

Arnold was visiting Hong Kong at the invitation of Volodya Vladimirov for the duration of the fall semester of 1995, while we had just moved to Hong Kong. Dima and I often were together on the fantastic day hikes in the Hong Kong countryside parks. His physical stamina was quite impressive. At that time we two were also the focus of a well-attended panel on contemporary issues of mathematics at the Hong Kong University of Science and Technology. Dima expressed himself in his usual provocative way! I recall that we found ourselves on the same side in most of the controversies, and catastrophe theory in particular.

Dima Arnold was a great mathematician, and here I will just touch on his mathematical contributions which affected me the most.

While I never worked directly in the area of KAM, nevertheless those results had a great impact in my scientific work. For one thing they directed me away from trying to analyze the global orbit structures of Hamiltonian ordinary differential equations, in contrast to what I was doing for (unconstrained) equations. Thus KAM contributed to my motivation to study mechanics in 1970 from the point of view of topology, symmetry, and relative equilibria rather than its dynamical properties. The work of Arnold had already affected those subjects via his big paper on fluid mechanics and symmetry in 1966. See Jerry Marsden's account of how our two works are related [1]. I note that Jerry died even more recently than Dima.

KAM shattered the chain of hypotheses, ergodic, quasi-ergodic, and metric transitivity going from Boltzmann to Birkhoff. That suggested to me some kind of

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#### STEVE SMALE

non-Hamiltonian substitute in these hypotheses in order to obtain foundations for thermodynamics [4].

I read Arnold's paper on braids and the cohomology of swallowtails. It was helpful in my work on topology and algorithms, which Victor Vassiliev drastically sharpened.

Dima could express important ideas simply and in such a way that these ideas could transcend a single discipline. His work was instrumental in transforming Kolmogorov's early sketches into a revolutionary recasting of Hamiltonian dynamics with sets of invariant curves, tori of positive measure, and Arnold diffusion.

It was my good fortune to have been a part of Dima Arnold's life and his mathematics.

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## Memories of Vladimir Arnold

MICHAEL BERRY

My first interaction with Vladimir Arnold was receiving one of his notoriously caustic letters. In 1976 I had sent him my paper (about caustics, indeed) applying the classification of singularities of gradient maps to a variety of phenomena in optics and quantum mechanics. In my innocence, I had called the paper "Waves and Thom's theorem". His reply began bluntly:

Thank you for your paper. References:...

There followed a long list of his papers he thought I should have referred to. After declaring that in his view René Thom (whom he admired) never proved or even announced the theorems underlying his catastrophe theory, he continued:

I can't approve your system of referring to English translations where Russian papers exist. This has led to wrong attributions of results, the difference of 1 year being important—a translation delay is sometimes of 7 years...

and

... theorems and publications are very important in our science (... at present one considers as a publication rather 2-3 words at Bures or Fine Hall tea, than a paper with proofs in a Russian periodical)

and (in 1981)

I hope you'll not attribute these result [sic] to epigons.

He liked to quote Isaac Newton, often in scribbled marginal afterthoughts in his letters:

A man must either resolve to put out nothing new, or to become a slave to defend it

and (probably referring to Hooke)

Mathematicians that find out, settle and do all the business must content themselves with being nothing but dry calculators and drudges and another that does nothing but pretend and grasp at all things must carry away all the invention as well of those that were to follow him as of those that went before.

(I would not accuse Vladimir Arnold of comparing himself with Newton, but was flattered to be associated with Hooke, even by implication.)

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I was not his only target. To my colleague John Nye, who had politely written "I have much admired your work...," he responded:

I understand well your letter, your admiration have not led neither to read the [reference to a paper] nor to send reprints....

This abrasive tone obviously reflected a tough and uncompromising character, but I was never offended by it. From the beginning, I recognized an underlying warm and generous personality, and this was confirmed when I finally met him in the late 1980s. His robust correspondence arose from what he regarded as systematic neglect by Western scientists of Russian papers in which their results had been anticipated. In this he was sometimes right and sometimes not. And he was unconvinced by my response that scientific papers can legitimately be cited to direct readers to the most accessible and readable source of a result rather than to recognize priority with the hard-to-find original publication.

He never lost his ironic edge. In Bristol, when asked his opinion of perestroika, he declared: "Maybe the fourth derivative is positive." And at a meeting in Paris in 1992, when I found, in my conference mailbox, a reprint on which he had written: "to Michael Berry, admiringly," I swelled with pride—until I noticed, a moment later, that every other participant's mailbox contained the same reprint, with its analogous dedication!

In 1999, when I wrote to him after his accident, he replied (I preserve his inimitable style):

... from the POINCARÉ hospital... the French doctors insisted that I shall recover for the following arguments: 1) Russians are 2 times stronger and any French would already die. 2) This particular person has a special optimism and 3) his humour sense is specially a positive thing: even unable to recognize you, he is laughing.... I do not believe this story, because it would imply a slaughtering of her husband for Elia, while I am still alive.

(Elia is Arnold's widow.)

There are mathematicians whose work has greatly influenced physics but whose writings are hard to understand; for example, I find Hamilton's papers unreadable. Not so with Arnold's: through his pellucid expositions, several generations of physicists came to appreciate the significance of pure mathematical notions that we previously regarded as irrelevant. "Arnold's cat" made us aware of the importance of mappings as models for dynamical chaos. And the exceptional tori that do not persist under perturbation (as Kolmogorov, Arnold, and Moser showed that most do) made us aware of Diophantine approximation in number theory: "resonant torus" to a physicist = "rational number" to a mathematician.

Most importantly, Arnold's writings were one of the two routes by which, in the 1970s, the notion of genericity slipped quietly into physics (the other route was critical phenomena in statistical mechanics, where it was called universality). Genericity emphasizes phenomena that are typical rather than the special cases (often with high symmetry) corresponding to exact solutions of the governing equations in terms of special functions. (And I distinguish genericity from abstract generality, which can often degenerate into what Michael Atiyah has called "general nonsense".) This resulted in a shift in our thinking whose significance cannot be overemphasized. It suddenly occurs to me that in at least four respects Arnold was the mathematical counterpart of Richard Feynman. Like Feynman, Arnold made massive original contributions in his field, with enormous influence outside it; he was a master expositor, an inspiring teacher bringing new ideas to new and wide audiences; he was uncompromisingly direct and utterly honest; and he was a colorful character, bubbling with mischief, endlessly surprising.

# Dima Arnold in My Life

## DMITRY FUCHS

Unfortunately, I have never been Arnold's student, although as a mathematician, I owe him a lot. He was just two years older than I, and according to the University records, the time distance between us was still less: when I was admitted to the Moscow State university as a freshman, he was a sophomore. We knew each other but did not communicate much. Once, I invited him to participate in a ski hiking trip (we used to travel during the winter breaks in the almost unpopulated northern Russia), but he said that Kolmogorov wanted him to stay in Moscow during the break: they were going to work together. I decided that he was arrogant and never repeated the invitation.

Then he became very famous. Kolmogorov announced that his nineteen yearold student Dima Arnold had completed the solution of Hilbert's 13th problem: every continuous function of three or more variables is a superposition of continuous functions of two variables. Dima presented a two-hour talk at a weekly meeting of the Moscow Mathematical Society; it was very uncommon for the society to have such a young speaker. Everybody admired him, and he certainly deserved that. Still there was something that kept me at a distance from him.

I belonged to a tiny group of students, led by Sergei Novikov, which studied algebraic topology. Just a decade before, Pontryagin's seminar in Moscow was a true center of the world of topology, but then Cartan's seminar in Paris claimed the leadership, algebraic topology became more algebraic, and the rulers of Moscow mathematics pronounced topology dead. Our friends tried to convince us to drop all these exact sequences and commutative diagrams and do something reasonable, like functional analysis or PDE or probability. However, we were stubborn. We even tried to create something like a topological school, and, already being a graduate student, I delivered a course of lectures in algebraic topology. The lectures were attended by several undergraduates, and we were happy to play this game.

Then something incredible happened. One day I found the lecture room filled beyond capacity; I even had to look for a bigger room. My audience had become diverse: undergraduates, graduate students, professors. This change had a very clear reason: the Atiyah–Singer index theorem.

The problem of finding a topological formula for the index of an elliptic operator belonged to Gelfand. Our PDE people studied indexes a lot, and they had good results. It was not a disaster for them that the final formula was found by somebody else: their works were respectfully cited by Atiyah, Singer, and their followers. The

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trouble was that the formula stated, "the index is equal to" and then something which they could not understand. People rushed to study topology, and my modest course turned out to be the only place to do that.

And to my great surprise, I noticed Dima Arnold in the crowd.

I must say that Dima never belonged to any crowd. Certainly the reason for his presence did not lie in any particular formula. Simply, he had never dismissed topology as nonsense, but neither had he been aware of my lectures. When he learned of their existence, he appeared. That was all. He never missed a lecture.

One day we met in a long line at the student canteen. "Listen," he said, "can you explain to me what a spectral sequence is?" I began uttering the usual words: a complex, a filtration, differentials, adjoint groups, etc. He frowned and then said, "Thus, there is something invariant ['invariant' in his language meant 'deserving of consideration'] in all this stuff, and this is the spectral sequence, right?" I thought for a moment and said, yes. At this moment we got our meals, and our conversation changed its direction.

Evidently spectral sequences were not for Arnold. Nonetheless, there is such a thing as Arnold's spectral sequence [4], a humble object in the world of his discoveries, resembling the asteroid Vladarnolda in the solar system (the stability of which he proved approximately at the time of our conversation in the canteen), named after him. When I say that he could not appreciate spectral sequences, I mean that he in general had a strong dislike for unnecessary technicalities, and technicalities were often unnecessary to him because of his extremely deep understanding. By the way, this attitude toward impressive but unnecessary tricks extended beyond mathematics. Years later we spent a week or so with friends at a ski resort in Armenia. We showed each other different turns and slidings, but Dima obviously was not interested. He said that the slope was not too steep, and he simply went straight from the top to the bottom, where he somehow managed to stop. I was surprised: there was a stone hedge in the middle of the slope that you needed to go around. Dima said modestly, "You know, at this place my speed is so high that I simply pull my legs up and jump over the hedge." I could not believe it, so I waited at a safe distance from the hedge and watched him doing that. It was more impressive than all our maneuvers taken together. Whatever he did—mathematics, skiing, biking—he preferred not to learn how to do it but just to do it in the most natural way, and he did everything superlatively well.

I do not remember how it came about that I began attending his Tuesday seminar. Probably he asked me to explain some topological work there, then I had to participate in some discussion, and then I could not imagine my life without spending two hours every Tuesday evening in a small room on the fourteenth floor of the main building of the MSU. Works of Arnold, his numerous students, and other selected people were presented at the seminar, and Dima insisted that every word of every talk be clear to everybody in the audience. My role there was well established: I had to resolve any topology-related difficulty. Some of my friends said that at Arnold's seminar I was a "cold topologist". Certainly, a non-Russianspeaker cannot understand this, so let me explain. In many Russian cities there were "cold shoemakers" in the streets who could provide an urgent repair to your footwear. They sat in their booths, usually with no heating (this is why they were "cold"), and shouted, "Heels!...Soles!...." So I appeared as if sitting in a cold booth and yelling, "Cohomology rings!... Homotopy groups!... Characteristic classes!...."

In my capacity as cold topologist, I even had to publish two short articles. One was called "On the Maslov-Arnold characteristic classes," and the other one had an amusing history. One day Dima approached me before a talk at the Moscow Mathematical Society and asked whether I could compute the cohomology of the pure braid group ("colored braid group" in Russian); he needed it urgently. I requested a description of the classifying space, and the calculation was ready at the end of the talk. It turned out that the (integral) cohomology ring was isomorphic to a subring of the ring of differential forms on the classifying manifold. He suggested that I write a note, but I refused: for a topologist it was just an exercise; it could be interesting only in conjunction with an application to something else. (I knew that Dima was thinking of Hilbert's 13th problem in its algebraic form: the possibility of solving a general equation of degree 7 not in radicals, but in algebraic functions of two variables.) I suggested that he write an article and mention my modest contribution in an appropriate place. He did [1]. But, a couple of months later, he needed the cohomology of the classical Artin's braid group. This was more difficult and took me several days to complete the calculation. I did it only modulo 2, but I calculated a full ring structure and also the action of the Steenrod squares. (The integral cohomology was later calculated independently by F. Vainshtein, V. Goryunov, and F. Cohen and still later Graeme Segal proved that the classifying space of the infinite braid group was homologically equivalent to  $\Omega^2 S^3$ .) I phoned Dima and explained the results. First he requested that I give a talk at the seminar (Next Tuesday! That is tomorrow!), and then he decidedly refused to do what we had agreed upon for pure braids: to write an article and mention my participation where appropriate. After a brief argument, we arrived at a compromise: I publish an article about the cohomology of the braid group without any mentioning of Hilbert's problem, and he publishes an article where this cohomology is applied to superpositions of algebraic functions. When we met the next day, his article was fully written and mine had not even been started. But his article contained a reference to mine and hence the title of the latter. I could delay no longer, and the two articles were published in the same volume of Functional Analysis [3], [5]. Since the articles in *Functional Analysis* were arranged alphabetically, his article was the first, and mine was the last. But this was not the end of the story. A cover-to-cover translation of *Functional Analysis* was published by an American publisher. The braid group in Russian is called *группа* кос; the word кос is simply the genitive of koca, a braid, but the American translators thought that KOC was a Russian equivalent of COS, and the English translation of my article was attributed to a mysterious cosine group. I do not know how many English-speaking readers of the journal tried to guess what the cosine group was.

As a permanent participant of Arnold's seminar, I had an opportunity to give talks on my works not explicitly related to the main directions of the seminar. I gave a brief account of my work with Gelfand on the cohomology of infinitedimensional Lie algebras, of characteristic classes of foliations. These things did not interest Dima much, although he himself had a work on similar things [2]. He always considered algebra and topology as something auxiliary. Once I heard him saying respectfully, "Siegel's case, this is a true analysis," and this sounded
like "true mathematics". Whatever he did, his unbelievably deep understanding of analysis was always his main instrument.

One more story of a similar kind. In 1982 John Milnor, who briefly visited Moscow, delivered a talk at Arnold's seminar on a very recent (and not yet published) work of D. Bennequin on a new invariant in the theory of Legendrian knots in contact 3-manifolds. The main result of Bennequin stated that the "Bennequin number" (now justly called the "Thurston-Bennequin number") of a topologically unknotted Legendrian knot in the standard contact space must be strictly negative. For an illustration, Milnor showed an example of a Legendrian trefoil with the Bennequin number +1. Arnold said that at last he had seen a convincing proof that the trefoil is a topologically nontrivial knot. Certainly, this was a joke: Bennequin's proof at that time did not look convincing, and the nontriviality of the trefoil has a popular proof understandable to middle school students (via the tricolorability invariant). But for Dima only an analytic proof could be fully convincing.

When I joined the Arnold seminar, it had just acquired the name of "the seminar on singularities of smooth maps". In the mid-1960s, Arnold was fascinated by work of John Mather on singularities. People could not understand this. Allegedly, Pontryagin said: "We can always remove complicated singularities of a smooth map by a small perturbation; it is sufficient to study the generic case." But singularities appear in families of smooth maps; you cannot remove them, insisted Dima. Some people mocked his affection for singularity theory. There is a short story of Stanislav Lem (a Polish science fiction writer) in which robots that could experience human emotions were manufactured. One of these robots felt an immense joy when he solved quadratic equations—just like you, Dima! Dima smiled at such jokes but continued studying singularities.

The results of Arnold and his students in this area were very deep and diverse. He classified all singularities that appear in generic families depending on no more than 14 parameters and studied their moduli varieties and discriminants. He discovered the relations of the theory to symplectic, contact, and differential geometry. It had deep applications in topology (Vassiliev's invariants of knots), differential equations, and classical mechanics.

More or less at the same time, a widely popularized version of the singularity theory emerged under the colorful name of the theory of catastrophes. It was promoted by two remarkable topologists, R. Thom and E. C. Zeeman. "The most catastrophic feature of the theory of catastrophes is a full absence of references to the works of H. Whitney," Dima wrote in one of his books. Indeed, mathematically, the theory of catastrophes was based on a classification of singularities of generic smooth maps of a plane onto a plane. The classification was fully done in 1955 by Whitney [6], but the founding fathers of catastrophe theory preferred to pretend that the works of Whitney never existed. Still, Dima made his contribution to the popularization of catastrophes: he wrote a short popular book under the title "Theory of Catastrophes". It was written in 1983 and then translated into a dozen languages.

In 1990 I moved to a different country, and we met only four or five times after that. The last time that I saw him was in spring 2007, when he visited California. We travelled together through the Napa and Sonoma Valleys; he was especially interested in visiting Jack London's grave. He spoke endlessly of his new (was it new?) passion for continued fractions, numerical functions, and numerical experimentation. I boasted that I taught a course of history of mathematics, and he immediately began testing my knowledge of the subject: Who proved the Euler theorem of polyhedra? Who proved the Stokes theorem? To his apparent displeasure I passed the exam. (He was especially surprised that I knew that Descartes proved the Euler theorem more than one hundred years before Euler. Why do you know that? I said that Efremovich told me this some thirty years before.) More than that, I knew something that he did not know: the Stokes theorem as it is stated in modern books,  $\int_C d\varphi = \int_{\partial C} \varphi$ , was first proved and published by the French mathematician E. Goursat (1917). We discussed a bit our further plans, and Dima said that whatever he plans, he always adds, as Leo Tolstoy did, EEЖ = если буду жив, "If I am alive." I said that I also never forget to add this, but apparently neither of us took it seriously. Anyhow, we never met again.

My tale of Dima Arnold is becoming lengthy, although I feel that what I have said is a small fraction of what I could say about this tremendous personality. Still, the story would be incomplete if I did not mention something known to everybody who has ever communicated with him, if only occasionally: his universal knowledge of everything. Whatever the subject was—Chinese history, African geography, French literature, the sky full of stars (especially this: he could speak endlessly on every star in every constellation)—he demonstrated without effort a familiarity with the subject which exceeded and dwarfed everybody else's, and this, combined with his natural talent as a storyteller, made every meeting with him a memorable event. Some friends recollect a sight-seeing tour in Paris he gave a couple of months before his death. Obviously, no tourist agency ever had a guide of this quality. Instead of adding my own recollections, I finish my account with a translation of a letter I received from him a year after our last meeting and two years before his death.

#### Paris, March 26, 2008

Dear Mitya,

I have recently returned to Paris from Italy where I wandered, for three months, in karstic mountains working at ICPT (the International Center for Theoretical Physics) at Miramare, the estate of the Austrian prince Maximilian who was persuaded by Napoleon III to become the Emperor of Mexico (for which he was shot around 1867 as shown in the famous and blooddrenched picture of Edouard Manet).

I lived in the village of Sistiana, some 10 kilometers from Miramare in the direction of Venice. It was founded by the pope Sixtus, the same one who gave names to both the chapel and Madonna. Passing the POKOPALIŠCE<sup>1</sup> (the cemetery) some 3 versts<sup>2</sup> to the North, I reached a deer path in a mountain pine grove. These deer do not pay much attention to a small tin sign,  $\mathcal{A}EP\breve{3}ABHASMME\breve{3}A^3$  (the state border). After that it is Slovenia to which I ran, following the deer. But at the next sign, PERICOLO, the deer refused to go any farther. The local people

<sup>&</sup>lt;sup>1</sup>This word has a notable similarity to Russian *KOIIATL*, to dig.

<sup>&</sup>lt;sup>2</sup>*BEPCTA* is an old Russian measure of length,  $\approx 1.1 \ km$ .

<sup>&</sup>lt;sup>3</sup>Both words belong to old Russian.

(whose language is closer to Russian than Ukrainian or Bulgarian) explained to me that the sign is a warning that the nearby caves have not been demined. And they were mined during the FIRST world war when my deer path was called SENTIERA DIGUERRA and was a front line (described by Hemingway in "A Farewell to Arms").

I did not go down to these particular caves, but every day I visited tens of them, of which some (but not all) were shown on a map (where they were called YAMA,<sup>4</sup> GROTTA, CAVA, CAV-ERNA, ABISSA, dependingly of the difficulty of the descent). All these caves look pretty much the same (a colorful scheme is *provided*): there is a hole on the mountain, a meter in size, and down go walls, of not even vertical but rather a negative slope. The depth of the mine is usually around 10 or 20 meters (but I descended to YAMA FIGOVICHEVA with the officially declared depth of 24 meters and to the half of the height, or rather the depth, of GROTTA TERNOVIZZA whose depth is marked as 32 meters and to which one cannot descend without a rope). At the bottom of the YAMA a diverging labyrinth of passages starts, of the lengths on the order of 100 meters. They go to lakes, stalactites, etc. Sometimes there is even a descent to the Timavo river (which flows about 50 kilometers at the depth 100 or 200 meters, depending on the height of hills above). Before this 50 kilometers it is a forest river resembling Moscow River at Nikolina Gora<sup>5</sup> with a charming Roman name of REKA.<sup>6</sup>

This was a part of Jason's expedition (with argonauts). On his way back from Colchis (with the golden fleece) he sailed his ship Argo upstream Ister (Danube) and its tributaries to the Croatian peninsula named Capudistria (which is visible from my window at Sistiana), then they dragged the ship to REKA and, following Timavo, they reached to northernmost point of the Adriatic, where the Roman city of Aquileia was later built.

Near Aquileia, I discovered a goddess Methe, new to me, but this is a separate story. (She saves any drinker of drunkenness, however much he drank. Allegedly, she was the mother of Athena, and Jupiter ate her, since he was afraid that she would give birth to a son, and that this son would dethrone him, precisely as he himself had dethroned his father.) Aquileia is a Roman port of the first century, preserved as well as Pompeii, without any Vesuvius: simply Attila who destroyed the city left the port intact, including the canals, ships (which survive to our time), quays, knechts and basilicas (which became Christian in

<sup>&</sup>lt;sup>4</sup>Russian *MA* means a gap.

 $<sup>^5\</sup>mathrm{A}$  village some 30 kilometers from Moscow where many remarkable Russian people (including Dima) used to spend their vacations.

 $<sup>^{6}</sup>PEKA$  is the Russian for a river.

the IV century) with mosaics of  $50 \ m \times 100 \ m$  in size, and absolutely everything as in Pompeii. No room to describe everything, I am just sending my best (Easter) wishes.

On June 3, I go to Moscow, there will be a conference dedicated to the centenary of LSP.  $^7$ 

Dima

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<sup>&</sup>lt;sup>7</sup>Lev Semenovich Pontryagin

## CHAPTER 13

# V. I. Arnold, As I Have Seen Him

# Yulij Ilyashenko

A student, visiting his schoolmaster in math, the famous and severe Morozkin. A radiant slim youth, almost a boy. This was Arnold as I first saw him, more than fifty years ago.

A graduate student (in 1960), conducting tutorials in honors calculus (taught to freshmen at Mekhmat, the Department of Mechanics and Mathematics of the Moscow State University). There was a permanent kind of smile on his face, his eyes were sparkling, and when he looked at you, a wave of good will would come forth.

From 1968 to 1986 I had the privilege of working with Arnold at the same section of Mekhmat, called the "Division of Differential Equations". It was shaped by Petrovski and chaired by him until his premature death in 1973. When Arnold joined the division, it was full of the best experts in differential equations, partial and ordinary. Besides Arnold and Petrovski, the faculty of the division included stars of the elder generation (who were then in their thirties and forties): Landis, Oleinik, Vishik, as well as brilliant mathematicians of Arnold's generation: Egorov, Kondratiev, Kruzhkov, and others.

The first glorious results of Arnold are described in other papers in this collection. Let me turn to differential equations, a subject whose development I have been closely following. Needless to say, these are personal remarks, not a complete history.

In 1965 Arnold came back from France, where he spent almost a year. From there he brought a keen interest in the newborn singularity theory, of which he became one of the founding fathers. He also brought the philosophy of general position invented by René Thom, which became sort of a compass in Arnold's investigations in differential equations and bifurcation theory.

In the form that Arnold gave to it, this philosophy claimed that one should first investigate objects in general position, then the simplest degenerations, together with their unfoldings. It makes no sense to study degenerations of higher codimension until those of smaller codimension have been investigated.

In 1970 he published a short paper [1], in which a strategy for developing any kind of local theory based on the above philosophy was suggested. He also defined algebraically solvable local problems. He started to call them "trivial", but later stopped doing that. "Let us forget the overloaded term," he once told me about this word. In the same paper he also stated that the problem of distinguishing

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center and focus is trivial. Bruno challenged this statement, and I proved that the center-focus problem is algebraically unsolvable (1972).

Also in 1970 Arnold proved that the problem of Lyapunov stability is algebraically unsolvable. He constructed a 3-parameter family in the space of high-order jets, where the boundary of stability is nonalgebraic. In the same paper he wrote: "One may expect that the Lyapunov stability, having lost algebraicity and no more restricted by anything, may present some pathologies on the set theoretic-level...." He also suggested that the problem may be algorithmically unsolvable. This conjecture is still open. In the mid-1970s it turned out that a nonalgebraic boundary of Lyapunov stability occurs in unfoldings of degenerations of codimension three in the phase spaces of dimension four. This was discovered by Shnol' and Khazin, who investigated the stability problem in the spirit of Arnold and studied all the degenerate cases up to codimension three.

In 1969 Bruno defended his famous doctoral thesis about analytic normal forms of differential equations near singular points. One of his results is the so-called Bruno condition: a sufficient condition for the germ of a map to be analytically equivalent to its linear part. In dimension one, Yoccoz proved the necessity of this condition (1987); this result was rewarded by a Fields Medal, which he got in 1994. So the problem is still a focus of interest in the mathematics community. But let us get back to the late 1960s. In his review of the Bruno thesis, Arnold wrote: "The existing proofs of the divergence of normalizing series are based on computations of the growth of coefficients and do not explain its nature (in the same sense as the computation of the coefficients of the series  $\arctan z$  does not explain the divergence of this series for |z| > 1, although it proves this divergence)." Following this idea, Arnold tried to find a geometric explanation of the divergence of normalizing series when the denominators are too small. He predicted an effect which he later called "materialization of resonances". An "almost resonant" germ of a vector field that gives rise to "exceedingly small denominators" is close to a countable number of resonant germs. Under the unfolding of any such germ, an invariant manifold bifurcates from a union of coordinate planes and remains in a small neighborhood of the singular point of this almost resonant germ. These invariant manifolds, which constitute a countable number of "materialized resonances", accumulate to the singular point and prevent the linearization.

A. Pyartli, a student of Arnold, justified this heuristic description in his thesis in the early 1970s for vector fields with planar saddles. He continued the investigation and in 1976 found an invariant cylinder, a materialization of resonances for a germ of a planar map. Then he asked Arnold, "Why does such a cylinder prevent the linearization?" Why, indeed?! Arnold himself started thinking about the problem and came to the theory of normal forms for neighborhoods of embedded elliptic curves. An overview of this theory is given in his book [**3**]. As usual, this new path was paved by the followers of Arnold: Pyartli, myself, Saveliev, Sedykh, and others.

Arnold's approach to the local bifurcation theory produced a genuine revolution. In the late 1960s he suggested to his students two problems: to prove a reduction principle that excludes excessive "hyperbolic variables" from any local bifurcation problem and to study the first really difficult bifurcation problem in codimension two. The first problem was solved by A. Shoshitaishvili, the second one by R. Bogdanov. "It was not by chance that I launched two different people in two directions simultaneously," Arnold later said to me. Arnold was especially proud that Bogdanov proved the uniqueness of the limit cycle that occurs under the perturbation of a generic cuspidal singular point. F. Takens investigated independently the same codimension two bifurcation as Bogdanov; it is now named the "Bogdanov-Takens" bifurcation.

In [2] Arnold described the new approach to the theory and listed all problems that occur in the study of local bifurcations of singular points of vector fields in codimension two. This was a long-standing program. J. Guckenheimer and N. Gavrilov made important contributions to its development; final solutions were obtained by H. Zoladec (in the mid-1980s), again under the (nonofficial) supervision of Arnold.

In the mid-1970s Arnold himself considered another local bifurcation problem in codimension two, the one for periodic orbits. He discovered strong resonances in the problem and predicted all possible unfoldings occurring in generic perturbations of the Poincaré maps with these resonances (1977). There were four of them. The first case was reduced to Bogdanov-Takens; two other cases were investigated by E. Horosov (1979), a graduate student of Arnold, in his Ph.D. thesis. The fourth case, the famous resonance 1 : 4, was investigated by A. Neishtadt, F. Berezovskaya, A. Khibnik (influenced by Arnold), and B. Krauskopf, a student of Takens. The problem that remains unsolved for bifurcations of codimension two is the existence of very narrow chaotic domains in the parameter and phase spaces.

Later local bifurcations of codimension three were investigated by Dumortier, Roussarie, Sotomayor, and others. The bifurcation diagrams and the phase portraits became more and more complicated. It became clear that it is hopeless to get a complete picture in codimension four. The new part of the bifurcation theory started by Arnold and his school seems to be completed by now. What is described above is a very small part of the new domains that were opened in mathematics by Arnold.

One should not forget that Arnold also inspired many discoveries in oral communications, while no trace of this influence is left in his publications. For instance, he discovered "hidden dynamics" in various problems of singularity theory. This means that a classification problem for singularities often gives rise, in a nonevident way, to a classification problem for special local maps. Thus, he inspired the solution by S. Voronin (1982) of the local classification problem for singularities of envelopes for families of planar curves and the discovery of quite unexpected Ecalle-Voronin moduli of the analytic classification of parabolic fixed points (1981).

Arnold suggested a sketch of the proof of analytic unsolvability of the Lyapunov stability problem (Ilyashenko, 1976). Only later did I understand that, honestly speaking, it should have been a joint work.

In 1980 he pointed out that our joint work with A. Chetaev on an estimate of the Hausdorff dimension of attractors might be applied to the 2D Navier-Stokes equation. This gave rise to an explicit estimate of the Hausdorff dimension of these attractors (Ilyashenko, 1982–83), a first step in the subject later developed by O. Ladyzhenskaya and M. Vishik with his school.

This is only my personal experience, a minor part of the great panorama of Arnold's influence on contemporary mathematics. He had a very strong feeling of mathematical beauty, and his mathematics was at the same time poetry and art. From my youth, I considered Arnold as a Pushkin in mathematics. At present, Pushkin is a beloved treasure of the Russian culture, but during his life, he was not at all treated as a treasure.

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The same is true for Arnold. His life in Russia before perestroika was in no way a bed of roses. I remember very well how we young admirers of Arnold expected in 1974 that he would be awarded the Fields Medal at the ICM at Vancouver. He did not receive it, and the rumor was that Pontryagin, the head of the Soviet National Mathematics Committee, at the discussion of the future awards said, "I do not know the works of such a mathematician." For sure, it could not have been the personal attitude of Pontryagin only; it was actually the position of the Soviet government itself. Two medals instead of four were awarded that year. Much later, Arnold wrote that one of the others was intended for him, and then awarded to nobody.

In 1984 a very skillful baiting of Arnold was organized at Mekhmat. As a result, he had a serious hypertension attack. His election as a corresponding member of the Soviet Academy of Sciences stopped the baiting, but his enemies tried (though unsuccessfully) to renew it five years later.

In 1986 Arnold decided to quit Mekhmat and to move to the Steklov Institute. Yet he wanted to keep a half-time position of professor at Mekhmat. Only after considerable efforts did he get the desired half-time position. I tried to convince Arnold not to quit Mekhmat. I asked him, "Dima, who may say, following Louis XIV's 'L'etat s'est moi,' Mekhmat is me?" "Well," he answered, "I guess NN" (he named an influential party member at the department). "No, Dima, YOU are Mekhmat." But he did not listen.

In 1994 he quit Mekhmat completely. He was offended. He taught a course and a seminar, and suddenly he was informed that this load was insufficient for the half-time position of professor, but only for a quarter-time position (a status that does not, in fact, exist). He spoke with the head of Mekhmat Human Resources. This was an aged woman who maintained her position from the communist times. "She screamed at me," said Arnold with a sort of surprise. Then he resigned from the Moscow State University.

Needless to say, in such an environment the students of Arnold were not hired at Mekhmat. The only exceptions were N. Nekhoroshev and A. Koushnirenko, hired in the early 1970s, and much later A. Varchenko. I remember two other attempts, both unsuccessful. At the same time, the best of the best Mekhmat students asked Arnold to be their advisor. So, Mekhmat rejected the best of the best of its alumni. The same happened with students of Manin, Kirillov, Gelfand.... At the end of the 1980s, a critical mass of excellent mathematicians not involved in the official academic life had accumulated. Following a suggestion of N. N. Konstantinov, a well-known educator and organizer of mathematical olympiads, these mathematicians decided to create their own university. In 1991 a group of leading Russian mathematicians formed a council and established a new Independent University of Moscow, IUM. This group included the following members of the Russian Academy of Sciences: V. I. Arnold (chairman of the council), S. P. Novikov, Ya. G. Sinai, L. D. Faddeev; and the following professors: A. A. Beilinson, R. L. Dobrushin, B. A. Dubrovin, A. A. Kirillov, A. N. Rudakov, V. M. Tikhomirov, A. G. Khovanskii, M. A. Shubin. Professors P. Deligne and R. MacPherson of Princeton and MIT also played crucial roles in the founding of the Independent University.

Arnold was very enthusiastic about the new university, and in the first years of its existence he did a lot to shape its spirit and teaching style. Together with the first dean of the College of Mathematics of the IUM, A. Rudakov, Arnold thoroughly discussed the programs, and he himself taught a course on partial differential equations. Under his influence, the Independent University became one of the focal centers of Russian mathematical life.

In 1994 another educational institution, the Moscow Center of Continuous Mathematical Education (MCCME), was created. From the very beginning, Arnold was the head of the board of trustees of this center. The center, headed by I. Yashchenko, the director, became a very influential institution in Russian mathematical education and a powerful tool in the struggle against modern obscurantism. Arnold was one of the leaders of that struggle.

In 2005 Pierre Deligne, together with the IUM faculty, organized a contest for young Russian mathematicians. This contest was funded by Deligne from his Balzan Prize (and named after him) with the goal "to support Russian mathematics, struggling for survival." The funds of the contest were strictly limited. In 2006 Arnold met D. Zimin, the head of "D. B. Zimin's Charity Foundation Dynasty", and convinced him to establish a similar "Dynasty contest". Now the contest has become permanent, *Lord willing and the creek don't rise*, as the proverb says. This is only one of the examples of the long-lasting influence of Arnold on Russian mathematical life.

Arnold's talks were always special events. He began giving lectures at Mekhmat in September 1961 about the newborn theory later named KAM (Kolmogorov– Arnold–Moser). A rumor spread among the students that "Arnold has solved problems that Poincaré failed to solve." His lectures were very fast and intense, yet they attracted the best students in the department. He repeated this course twice, in 1962–63 and in 1963–64.

After that he gave brilliant courses in theoretical mechanics, ordinary differential equations, supplementary chapters of ODE, singularity theory, geometric theory of PDE, and many others. All these courses gave rise to world-famous books, written by Arnold, sometimes with his students. In 1968 Arnold started teaching a course in ODE that became, in a sense, a course of his life. He taught it every year until the late eighties, except for sabbaticals.

Arnold completely changed the face of the discipline. His presentation was coordinate-free: all the constructions were invariant with respect to coordinate changes. "When you present material in coordinates," he said, "you study your coordinate system, not the effect that you want to describe." His language was quite different from that of the previous textbooks and courses: diffeomorphisms, phase flows, rectification of vector fields, exponentials of linear operators... The language of pictures was even more important in his course than that of formulas. He always required a student to present the answer in both ways, a formula and a figure, and to explain the relation between them. He drastically renewed the problem sets for the course: propagation of rays in nonuniform media and geodesics on surfaces of revolution, phase portraits of the Newton equation with one degree of freedom, images of the unit square under linear phase flows—students were expected to draft all of these even without explicit calculations of the corresponding solutions. In the first years the course was difficult both for students and teaching assistants. Later on it smoothed out and became one of the highlights of the Mekhmat curriculum.

All his life V. I. Arnold was like a star that shines, sparkles, and produces new life around it.

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### CHAPTER 14

# My Encounters with Vladimir Igorevich Arnold

# Yakov Eliashberg

My formation as a mathematician was greatly influenced by Vladimir Igorevich Arnold, though I never was his student and even lived in a different city. When I entered Leningrad University in 1964 as an undergraduate math student, Arnold was already a famous mathematician. By that time he had solved Hilbert's 13th problem and had written a series of papers which made him the "A" in the KAM theory. Arnold was also working as an editor of the publishing house Mir, where he organized and edited translations of several books and collections of papers not readily accessible in the USSR. One of these books, a collection of papers on singularities of differentiable mappings, was an eye opener for me.

The first time I met Arnold was in January 1969 at a Winter Mathematical School at Tsakhkadzor in Armenia. I was eager to tell him about some of my recently proved results concerning the topology of singularities. Later that year he invited me to give a talk at his famous Moscow seminar. I remember being extremely nervous going there. I could not sleep at all in the night train from Leningrad to Moscow, and I do not remember anything about the talk itself.

In 1972 Vladimir Igorevich was one of my Ph.D. dissertation referees or, as it was called, an "official opponent". I remember that on the day of my defense, I met him at 5 a.m. at the Moscow Train Station in Leningrad. He immediately told me that one of the lemmas in my thesis was wrong. It was a local lemma about the normal form of singularities, and I thought (and, frankly, still do) that the claim is obvious. I spent the next two hours trying to convince Vladimir Igorevich, and he finally conceded that probably the claim is correct, but still insisted that I did not really have the proof. A year later he wrote a paper devoted to the proof of that lemma and sent me a preprint with a note that now my dissertation is on firm ground.

After my Ph.D. defense I was sent to work at a newly organized university in Syktyvkar, the capital of Komi Republic in the north of Russia. In 1977 we organized there a conference on global analysis which attracted a stellar list of participants, including V. I. Arnold. During this conference I asked Arnold to give a lecture for our undergraduate students. He readily agreed and gave an extremely interesting lecture about stability of the inverse pendulum, and even made a demonstration prepared with the help of one of our professors, Alesha Zhubr. Arnold had certain pedagogical methods to keep the audience awake. During his lectures he

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liked to make small mistakes, expecting students to notice and correct him. Apparently, this method worked quite well at the Moscow University. Following the same routine during his Syktyvkar lecture, he made an obvious computational error something like forgetting the minus sign in the formula  $(\cos x)' = -\sin x$ —and expected somebody in the audience to correct him. No one did, and he had to continue with the computation, which, of course, went astray: the terms which were supposed to cancel did not. Very irritated, Arnold erased the blackboard and started the computation all over again, this time without any mistakes. After the lecture, he told me that the undergraduate students at Syktyvkar University are very bad. The next day, after my regular class, a few students came to me and asked how is it possible that such a famous mathematician is making mistakes in differentiating  $\cos x$ ?

Whenever I happened to be in Moscow, which was not very often, Arnold usually invited me to visit the hospitable home he shared with his wife, Elya. When he moved to a new apartment in Yasenevo on the outskirts of Moscow, he told me over the phone how to get there. In particular, I was instructed to walk south when I got out of the metro station. When I got to that point it was a dark gray late winter afternoon, and it was quite a challenge to figure out in which direction I should go.

Once he ran a psychological test on me to determine which of my brain hemispheres is the dominant one. To his satisfaction, the test showed that it was the right one, which, according to Arnold, meant that I have a geometric rather than an algebraic way of thinking. During another visit, I was deeply honored when he told me that while he files most preprints systematically, I was among the few people who were assigned a personal folder.

Over the years I gave a number of talks at his seminar with variable success. The most disastrous was my last talk in 1985. Shortly before one of my trips to Moscow, Misha Gromov sent me a preliminary version of his now very famous paper "Pseudoholomorphic curves in symplectic geometry", which is one of the major foundational milestones of symplectic topology. I was extremely excited about this paper and thus volunteered to talk about it at Arnold's seminar. I think that I was at this moment the only person in the Soviet Union who had the paper. Arnold heard about Gromov's breakthrough but had not seen the paper yet. After a few minutes of my talk, Arnold interrupted me and requested that before continuing I should explain what is the main idea of the paper. This paper is full of new ideas and, in my opinion, it is quite subjective to say which one is **the** main one. I made several attempts to start from different points, but Arnold was never satisfied. Finally, towards the end of the two-hour long seminar, I said something which Arnold liked. "Why did you waste our time and did not start with this from the very beginning?", he demanded.

Vladimir Igorevich made two long visits to Stanford. During his first quarterlong visit Arnold was giving a lecture course, but he made it a rule for himself to go every morning for a long bike ride into the hills (called the Santa Cruz Mountains) surrounding Stanford. I have heard a lot of stories about Arnold's superhuman endurance and his extremely risky adventures, especially in his younger years. I can testify that at almost sixty years old, Arnold at Stanford was also very impressive. On a windy day after swimming in our cold Pacific Ocean, where the water temperature is usually around 13°C, he refused a towel. He had a very poor bike which was not especially suited for mountain biking. Yet he went with it everywhere, even over the roads whose parts were destroyed by a mudslide and where he had to climb clutching the tree roots, hauling his bike on his back. During one of these trips, Vladimir Igorevich met a mountain lion. He described this encounter in one of his short stories. Both Arnold and the lion were apparently equally impressed with the meeting. Many years later, during his second visit to Stanford, Arnold again went to the same place hoping to meet the mountain lion. Amazingly, the lion waited for him there! I am also fond of hiking in those hills, yet neither I nor any of people I know ever met a mountain lion there.

When he was leaving Stanford, Vladimir Igorevich gave me a present—a map of the local hills on which he had marked several interesting places that he had discovered, such as an abandoned apple farm or a walnut tree grove.

In between the two visits Arnold had a terrible bike accident in Paris which he barely survived. It was a great relief to see him active again when I met him in Paris two years later. He proudly told me that during this year he had written five books. "One of these books," he said, "is coauthored with two presidents. Can you guess with which ones?" I certainly could not guess that these were Vladimir Putin and George W. Bush.

During his last visit to Stanford and Berkeley a year ago, Arnold gave two series of lectures: one for "Stanford professors", as he called it, and the other for the school-age children at Berkeley Math Circles. There is no telling which of these two groups of listeners Vladimir Igorevich preferred. He spent all his time preparing for his lectures for children and even wrote a book for them. Lectures at Stanford were an obvious distraction from that main activity. Each Stanford lecture he would usually start with a sentence like "What I am going to talk about now is known to most kindergarten children in Moscow, but for Stanford professors I do need to explain this." What followed was always fascinating and very interesting.

It is hard to come to terms that Vladimir Igorevich Arnold is no longer with us. It is certainly true, though commonplace to say, that Arnold was a great and extremely influential mathematician, that he created several mathematical schools, and that his vision and conjectures shaped a large part of modern mathematics. But, besides all that, he was a catalyst for the mathematical community. He hated and always fought mediocrity everywhere. With his extreme and sometimes intentionally outrageous claims, he kept everybody on guard, not allowing us to comfortably fall asleep.

His departure is also painful to me because there are several unfulfilled mathematical promises which I made to him but never had time to finish. Though it is too late, I will do it now as a priority.

## CHAPTER 15

# On V. I. Arnold and Hydrodynamics

# BORIS KHESIN

Back in the mid-1980s, Vladimir Igorevich once told us, his students, how different the notion of "being young" (and in particular, being a young mathematician) is in different societies. For instance, the Moscow Mathematical Society awards an annual prize to a young mathematician under thirty years of age. The Fields Medal, as is well known, recognizes outstanding young mathematicians whose age does not exceed forty in the year of the International Congress. Both of the above requirements are strictly enforced.

This can be compared with the Bourbaki group, which is comprised of young French mathematicians and which, reportedly, has an age bar of fifty. However, as Arnold elaborated the story, this limit is more flexible: upon reaching this age the Bourbaki member undergoes a "coconutization procedure". The term is derived from a tradition of some barbaric tribe that allows its chief to carry out his duties until someone doubts his leadership abilities. Once the doubt arises, the chief is forced to climb to the top of a tall palm tree, and the whole tribe starts shaking it. If the chief is strong enough to get a good grip and survives the challenge, he is allowed to climb down and continue to lead the tribe until the next "reasonable doubt" in his leadership crosses someone's mind. If his grip is weak and he falls down from the 20-meter-tall tree, he obviously needs to be replaced, and so the next tribe chief is chosen. This tree is usually a coconut palm, which gave the name to the coconutization procedure.

As far as the coconutization in the Bourbaki group is concerned, according to Arnold's story, the unsuspecting member who reaches fifty is invited, as usual, to the next Bourbaki seminar. Somewhere in the middle of the talk, when most of the audience is already half asleep, the speaker, who is in on the game for that occasion, inserts some tedious half-a-page-long definition. It is at this very moment that the scrutinized ("coconutized") member is expected to interrupt the speaker by exclaiming something like, "But excuse me, only the empty set satisfies your definition!" If he does so, he has successfully passed the test and will remain a part of Bourbaki. If he misses this chance, nobody says a word, but he will probably not be invited to the meetings any longer.

Arnold finished this story by quoting someone's definition of youth in mathematics which he liked best: "A mathematician is young as long as he reads works other than his own!"

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Soon after this "storytelling" occasion, Arnold's fiftieth anniversary was celebrated: in June 1987 his whole seminar went for a picnic in a suburb of Moscow. Among Arnold's presents were a "Return to Arnold" stamp to mark the reprints he gave to his students to work on, a mantle with a nicely decorated "swallowtail". one of low-dimensional singularities, and such. But, most importantly, he was presented with a poster containing a crossword on various notions from his many research domains. Most of the questions were rather intricate, which predictably did not prevent Arnold from easily cracking virtually everything. But one question remained unresolved: a five-letter word (in the English translation) for "A simple alternative of life". None of the ideas worked for quite some time. After a while, having made no progress on this question, Arnold pronounced sadly, "Now I myself have been coconutized...." But a second later he perked up, a bright mischievous expression on his face: "This is a PURSE!" (In addition to the pirate's alternative "Purse or Life", the crossword authors meant the term "purse" in singularity theory standing for the description of the bifurcation diagram of the real simple singularity  $D_{4}^{+}$ , also called hyperbolic umbilic—hence the hint on "simple" alternative.)

Arnold's interest in fluid dynamics can be traced back to his "younger years", whatever definition one is using for that purpose. His 1966 paper in the *Annales de l'Institut Fourier* had the effect of a bombshell. Now, over forty years later, virtually every paper related to the geometry of the hydrodynamical Euler equation or diffeomorphism groups cites Arnold's work on the starting pages. In the next four or five years Arnold laid out the foundations for the study of hydrodynamical stability and for the use of Hamiltonian methods there, described the topology of steady flows, etc.

Apparently Arnold's interest in hydrodynamics is rooted in Kolmogorov's turbulence study and started with the program outlined by Kolmogorov for his seminar in 1958-59. Kolmogorov conjectured stochastization in dynamical systems related to hydrodynamical PDEs as viscosity vanishes, which would imply the practical impossibility of long-term weather forecasts. Arnold's take on hydrodynamics was, however, completely different from Kolmogorov's and involved groups and topology.

The Euler equation of an ideal incompressible fluid filling a domain M in  $\mathbb{R}^n$  is the evolution equation

$$\partial_t v + (v, \nabla)v = -\nabla p$$

on the fluid velocity field v, where this field is assumed to be divergence-free and tangent to the boundary of M (while the pressure p is defined uniquely modulo an additive constant by these conditions on v). In 1966 Arnold showed that this Euler equation can be regarded as the equation of the geodesic flow on the group SDiff(M) of volume-preserving diffeomorphisms of the domain M. The corresponding metric on this infinite-dimensional group is the right-invariant  $L^2$  metric defined by the kinetic energy  $E(v) = \frac{1}{2} ||v||_{L^2(M)}^2$  of the fluid. (The analysis of Sobolev spaces related to this group-theoretic framework in incompressible fluid dynamics was later furnished by D. Ebin and J. Marsden.) Arnold's geometric view on hydrodynamics opened a multitude of different research directions:

Other groups and metrics. Many other evolution equations turned out to fit this universal approach suggested by Arnold, as they were found to describe geodesic flows on appropriate Lie groups with respect to one-sided invariant metrics. This shed new light on the corresponding configuration spaces and symmetries behind the relevant physical systems, and such geodesic equations are now called the Euler-Arnold equations. Here are several examples developed by many authors. The group SO(3) with a left-invariant metric corresponds to the Euler top (this example appeared in the original paper by Arnold along with the hydrodynamical Euler equation). Similarly, the Kirchhoff equations for a rigid body dynamics in a fluid describe geodesics on the group  $E(3) = SO(3) \ltimes \mathbb{R}^3$  of Euclidean motions of  $\mathbb{R}^3$ . In infinite dimensions, the group of circle diffeomorphisms  $\text{Diff}(S^1)$ with the right-invariant  $L^2$ -metric gives the inviscid Burgers equation, while the Virasoro group for three different metrics,  $L^2$ ,  $H^1$ , and  $\dot{H}^1$ , produces respectively the Korteweg-de Vries, Camassa-Holm, and Hunter-Saxton equations, which are different integrable hydrodynamical approximations. The self-consistent magnetohydrodynamics describing simultaneous evolution of the fluid and magnetic field corresponds to dynamics on the semidirect product group  $\text{SDiff}(M) \ltimes \text{SVect}(M)$ equipped with an  $L^2$ -type metric. Yet another interesting example, known as the Heisenberg chain or Landau–Lifschitz equation, corresponds to the gauge transformation group  $C^{\infty}(S^1, SO(3))$  and  $H^{-1}$ -type metric. Teasing physicists, Arnold used to say that their gauge groups are too simple to serve as a model for hydrodynamics.

Arnold's stability and Hamiltonian methods in hydrodynamics. The geodesic property of the Euler hydrodynamical equation implied that it is Hamiltonian when considered on the dual of the Lie algebra of divergence-free vector fields. Arnold proposed using the corresponding Casimir functions, which are invariants of the flow vorticity, to study stability of steady fluid flows. Arnold's stability is now the main tool in the study of nonlinear stability of fluid motions and MHD flows. In particular, he proved that planar parallel flows with no inflection points in their velocity profiles are stable. (One should note that, for Hamiltonian systems, stability in linear approximation is always neutral and inconclusive about the stability in the corresponding nonlinear problem, so the result on a genuine Lyapunov stability of certain fluid flows was particularly rare and valuable.)

Study of fluid Lagrangian instability and curvatures of diffeomorphism groups. Negative sectional curvature on manifolds implies exponential divergence of geodesics on them. In the 1966 Ann. Inst. Fourier paper Arnold launched the first computations of curvatures for diffeomorphism groups. Negativity of most of such curvatures for the groups of volume diffeomorphisms suggested Lagrangian instability of the corresponding fluid flows. By applying this to the the atmospheric flows, he gave a qualitative explanation of unreliability of long-term weather forecasts (thus answering in his own way the problem posed by Kolmogorov in the 1950s). In particular, Arnold estimated that, due to exponential divergence of geodesics, in order to predict the weather two months in advance one must have initial data on the state of the Earth's atmosphere with five more digits of accuracy than that of the expected prediction. In practical terms this means that a dynamical weather forecast for such a long period is impossible.

The hydrodynamical Appendix 2 in the famous *Classical Mechanics* by Arnold,<sup>1</sup> where one can find the details of the above-mentioned calculation for the Earth's

<sup>&</sup>lt;sup>1</sup>Speaking of writing, once I asked Arnold how he managed to make his books so easy to read. He replied: "To make sure that your books are read fast, you have to write them fast." His own writing speed was legendary. His book on invariants of plane curves in the AMS University Lecture series was reportedly written in less than two days. Once he pretended to complain: "I tried, but failed, to write more than 30 pages a day....I mean to write in English; of course, in Russian, I can write much more!"

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atmosphere, also contains one of Arnold's widely cited phrases: "We agree on a simplifying assumption that the earth has the shape of a torus," which is followed by his calculations for the group of area-preserving torus diffeomorphisms. It is remarkable that the later curvature calculations for the group of sphere diffeomorphisms (performed by A. Lukatskii) gave exactly the same order of magnitude and quantitative estimates for the curvature, and hence for the atmospheric flows, as Arnold's original computations for the torus!

Topology of steady flows. . One of the most beautiful observations of Arnold (and one of the simplest —it could have belonged to Euler!) was the description of topology of stationary solutions of the 3D Euler equation. It turns out that for a "generic" steady solution the flow domain is fibered (away from a certain hypersurface) into invariant tori or annuli. The corresponding fluid motion on each torus is either periodic or quasiperiodic, while on each annulus it is periodic. This way a steady 3D flow looks like a completely integrable Hamiltonian system with two degrees of freedom.

The nongeneric steady flows include Beltrami fields (those collinear with their vorticity) and, in particular, the eigenfields for the *curl* operator on manifolds. The latter include the so-called ABC flows (for Arnold–Beltrami–Childress), the curl eigenfields on the 3D torus, which happen to have become a great model for various fast dynamo constructions.

Fast dynamo and magnetohydrodymanics. Arnold's interest in magnetohydrodynamics was to a large extent related to his acquaintance with Ya. Zeldovich and A. Sakharov. One of the results of their interaction at the seminars was the Arnold-Ruzmaikin-Sokolov-Zeldovich model of the fast dynamo on a 3D Riemannian manifold constructed from Arnold's cat map on a 2D torus. For a long time this was the only dynamo construction allowing complete analytical study for both zero and positive magnetic dissipation.

The asymptotic Hopf invariant. . Finally, one of the gems of topological hydrodynamics is Arnold's 1974 study of the asymptotic Hopf invariant for a vector field. He proved that, for a divergence-free vector field v in a 3D simply connected manifold M, the field's helicity,  $H(v) := \int_M (curl^{-1}v, v) d^3x$ , is equal to the average linking number of all pairs of trajectories of v. This theorem simultaneously generalized the Hopf invariant from maps  $S^3 \to S^2$  to arbitrary divergence-free vector fields in  $S^3$ , enriched K. Moffatt's result on the helicity of linked solid tori, described the topology behind the conservation law of the 3D Euler equation, and provided the topological obstruction to the energy relaxation of magnetic vector fields. This elegant theorem stimulated a tide of generalizations to higher-dimensional manifolds, to linking of foliations, to higher linkings, and to energy estimates via crossing numbers. In particular, there was substantial progress in the two directions suggested in the original 1974 paper: the topological invariance of the asymptotic Hopf numbers for a large class of systems was proved by J.-M. Gambaudo and E. Ghys, while the Sakharov–Zeldovich problem on whether one can make arbitrarily small the energy of the rotation field in a 3D ball by a volume-preserving diffeomorphism action was affirmatively solved by M. Freedman.

Virtually single-handedly Arnold spawned a new domain, now called topological fluid dynamics. His contribution to this area changed the whole paradigm of theoretical hydrodynamics by employing groups to study fluid flows. What doubles the awe is that this gem appeared almost at the same time with two other Arnold's foundational contributions—the KAM and singularity theories.

### CHAPTER 16

# Arnold's Seminar, First Years

Askold Khovanskii and Alexander Varchenko

In 1965–66, V. I. Arnold was a postdoc in Paris, lecturing on hydrodynamics and attending R. Thom's seminar on singularities. After returning to Moscow, Vladimir Igorevich started his seminar, meeting on Tuesdays from 4 to 6 p.m. It continued until his death on June 3 of 2010. We became Arnold's students in 1966 and 1968, respectively. The seminar was an essential part of our life. Among the first participants were R. Bogdanov, N. Brushlinskaya, I. Dolgachev, D. Fuchs, A. Gabrielov, S. Gusein-Zade, A. Kushnirenko, A. Leontovich, O. Lyashko, N. Nekhoroshev, V. Palamodov, A. Tyurin, G. Tyurina, V. Zakalyukin, and S. Zdravkovska.

V. I. Arnold had numerous interesting ideas, and to realize his plans he needed enthusiastic colleagues and collaborators. Every semester he started the seminar with a new list of problems and comments. Everyone wanted to be involved in this lively creative process. Many problems were solved, new theories were developed, and new mathematicians were emerging.

Here we will briefly describe some of the topics of the seminar in its first years, as well as the ski outings which were an integral part of the seminar.

Hilbert's 13th Problem and Arrangements of Hyperplanes. An algebraic function  $x = x(a_1, \ldots, a_k)$  is a multivalued function defined by an equation of the form

$$x^{n} + P_{1}(a_{1}, \dots, a_{k})x^{n-1} + \dots + P_{n}(a_{1}, \dots, a_{k}) = 0$$

where  $P_i$ 's are rational functions.

Hilbert's 13th Problem: Show that the function x(a, b, c), defined by the equation

$$x^7 + x^3 + ax^2 + bx + c = 0,$$

cannot be represented by superpositions of continuous functions in two variables.

A. N. Kolmogorov and V. I. Arnold proved that in fact such a representation does exist [2], thus solving the problem negatively. Despite this result it is still believed that the representation is impossible if one considers the superpositions of (branches of) algebraic functions only.

Can an algebraic function be represented as a composition of radicals and arithmetic operations? Such a representation does exist if and only if the Galois group of the equation over the field of its coefficients is solvable. Hence, the general

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algebraic function of degree  $k \ge 5$ , defined by the equation  $a_0x^k + a_1x^{k-1} + \cdots + a_k = 0$ , cannot be represented by radicals.

In 1963, while teaching gifted high school students at Moscow boarding school No. 18, founded by Kolmogorov, V. I. Arnold discovered a topological proof of the insolvability by radicals of the general algebraic equation of degree  $\geq 5$ , a proof which does not rely on Galois theory. Arnold's lectures at the school were written down and published by V. B. Alekseev in [1].

V. I. Arnold often stressed that when establishing the insolvability of a mathematical problem, topological methods are the most powerful and those best suited to the task. Using such topological methods, V. I. Arnold proved the insolvability of a number of classical problems; see [12], [10]. Inspired by that approach, a topological Galois theory was developed later; see [18]. The topological Galois theory studies topological obstructions to the solvability of equations in finite terms. For example, it describes obstructions to the solvability of differential equations by quadratures.

The classical formula for the solution by radicals of the degree four equation does not define the roots of the equation only. It defines a 72-valued algebraic function. V. I. Arnold introduced the notion of an exact representation of an algebraic function by superpositions of algebraic functions in which all branches of algebraic functions are taken into account. He proved that the algebraic function of degree  $k = 2^n$ , defined by the equation  $x^k + a_1 x^{k-1} + \cdots + a_k = 0$ , does not have an exact representation by superpositions of algebraic functions in  $\langle k - 1 \rangle$  variables; see [4] and the references therein. The proof is again topological and based on the characteristic classes of algebraic functions, introduced for that purpose. The characteristic classes are elements of the cohomology ring of the complement to the discriminant of an algebraic function. To prove that theorem V. I. Arnold calculated the cohomology ring of the pure braid group.

Consider the complement in  $\mathbb{C}^k$  to the union of the diagonal hyperplanes,

$$U = \{ y \in \mathbb{C}^k \mid y_i \neq y_j \text{ for all } i \neq j \}.$$

The cohomology ring  $H^*(U,\mathbb{Z})$  is the cohomology ring of the pure braid group on k strings. The cohomology ring  $H^*(U,\mathbb{Z})$  was described in [3]. Consider the ring  $\mathcal{A}$  of differential forms on U generated by the 1-forms  $w_{ij} = \frac{1}{2\pi i} d\log(y_i - y_j)$ ,  $1 \leq i, j \leq k, i \neq j$ . Then the relations  $w_{ij} = w_{ji}$  and

$$w_{ij} \wedge w_{jk} + w_{jk} \wedge w_{ki} + w_{ki} \wedge w_{ij} = 0$$

are the defining relations of  $\mathcal{A}$ . Moreover, the map  $\mathcal{A} \to H^*(U, \mathbb{Z}), \alpha \mapsto [\alpha]$ , is an isomorphism.

This statement says that each cohomology class in  $H^*(U, \mathbb{Z})$  can be represented as an exterior polynomial in  $w_{ij}$  with integer coefficients and the class is zero if and only if the polynomial is zero. As an application, V. I. Arnold calculated the Poincaré polynomial  $P_D(t) = \sum_{i=0}^k \operatorname{rank} H^i(D) t^i$ ,

$$P_{\Delta}(t) = (1+t)(1+2t)\cdots(1+(n-1)t).$$

Arnold's paper [3] was the beginning of the modern theory of arrangements of hyperplanes; see, for example, the book by P. Orlik and H. Terao.

**Real Algebraic Geometry.** By Harnack's theorem, a real algebraic curve of degree n in the real projective plane can consist of at most g + 1 ovals, where g = (n-1)(n-2)/2 is the genus of the curve. The M-curves are the curves for

which this maximum is attained. For example, an M-curve of degree 6 has 11 ovals. Harnack proved that the M-curves exist.

If the curve is of even degree n = 2k, then each of its ovals has an interior (a disc) and an exterior (a Möbius strip). An oval is said to be positive if it lies inside an even number of other ovals and is said to be negative if it lies inside an odd number of other ovals. The ordinary circle,  $x^2 + y^2 = 1$ , is an example of a positive oval.

In his 16th problem, Hilbert asked how to describe the relative positions of the ovals in the plane. In particular, Hilbert conjectured that 11 ovals on an M-curve of degree 6 cannot lie external to one another. This fact was proved by Petrovsky in 1938, see [7].

The first M-curve of degree 6 was constructed by Harnack, the second by Hilbert. It was believed for a long time that there were no other M-curves of degree 6. Only in the 1960s did Gudkov construct a third example and prove that there are only three types of M-curves of degree 6; see [17].

Experimental data led Gudkov to the following conjecture: If p and m are the numbers of positive and negative ovals of an M-curve of degree 2k, then  $p - m = k^2 \mod 8$ .

V. I. Arnold was a member of Gudkov's Doctor of Science thesis defense committee and became interested in these problems. He related Gudkov's conjecture and theorems of divisibility by 16 in the topology of oriented closed fourdimensional manifolds developed by V. Rokhlin and others. Starting with an Mcurve, V. I. Arnold constructed a four-dimensional manifold with an involution and using the divisibility theorems proved that  $p - m = k^2 \mod 4$ ; see [5]. Soon after that, V. A. Rokhlin, using Arnold's construction, proved Gudkov's conjecture in full generality.

This paper by V. I. Arnold began a revitalization of real algebraic geometry.

**Petrovsky–Oleinik Inequalities.** Petrovsky's paper [7] led to the discovery of remarkable estimates for the Euler characteristics of real algebraic sets, called Petrovsky-Oleinik inequalities. V. I. Arnold found in [6] unexpected generalizations of these inequalities and new proofs of the inequalities based on singularity theory.

Consider in  $\mathbb{R}^{n+1}$  the differential one-form  $\alpha = P_0 dx_0 + P_1 dx_1 + \cdots + P_n dx_n$ , whose components are homogeneous polynomials of degree m. What are possible values of the index ind of the form  $\alpha$  at the point  $0 \in \mathbb{R}^{n+1}$ ?

Let us introduce Petrovsky's number  $\Pi(n, m)$  as the number of integral points in the intersection of the cube  $0 \le x_0, \ldots, x_n \le m-1$  and the hyperplane  $x_0 + \cdots + x_n = (n+1)(m-1)/2$ . V. I. Arnold proved in [6] that

 $|\operatorname{ind}| < \Pi(n,m)$  and  $\operatorname{ind} \equiv \Pi(n,m) \mod 2$ .

His elegant proof of these relations is based on the Levin-Eisenbud-Khimshiashvili formula for the index of a singular point of a vector field.

Let P be a homogeneous polynomial of degree m + 1 in homogeneous coordinates on  $\mathbb{R}P^n$ . Petrovsky-Oleinik inequalities give upper bounds for the following quantities:

- a)  $|\chi(P=0)-1|$  for odd n, where  $\chi(P=0)$  is the Euler characteristic of the hypersurface P=0 in  $\mathbb{R}P^n$ , and
- b)  $|2\chi(P \leq 0) 1|$  for even n and m + 1, where  $\chi(P \leq 0)$  is the Euler characteristic of the subset  $P \leq 0$  in  $\mathbb{R}P^n$ .

V. I. Arnold noticed in [6] that in both cases a) and b) the estimated quantity equals the absolute value of the index at  $0 \in \mathbb{R}^{n+1}$  of the gradient of P. Thus, the Petrovsky-Oleinik inequalities are particular cases of Arnold's inequalities for  $\alpha = dP$ .

Furthermore, Arnold's inequalities are exact (unlike the Petrovsky–Oleinik ones): for any integral value of ind with the properties  $|\text{ind}| \leq \Pi(n,m)$  and  $\text{ind} \equiv \Pi(n,m) \mod 2$  there exists a homogeneous 1-form  $\alpha$  (not necessarily exact) with this index (proved by Khovasnkii).

Critical Points of Functions. Critical points of functions was one of the main topics of the seminar in its first years. V. I. Arnold classified simple singularities of critical points in 1972, unimodal ones in 1973, and bimodal ones in 1975. Simple critical points form series  $A_n, D_n, E_6, E_7, E_8$  in Arnold's classification. Already in his first papers V. I. Arnold indicated (sometimes without proofs) the connections of simple critical points with simple Lie algebras of the corresponding series. For example, the Dynkin diagram of the intersection form on vanishing cohomology at a simple singularity of an odd number of variables equals the Dynkin diagram of the corresponding Lie algebra, the monodromy group of the simple singularity equals the Weyl group of the Lie algebra, and the singularity index of the simple singularity equals 1/N, where N is the Coxeter number of the Lie algebra.

One of the main problems of that time was to study characteristics of critical points. The methods were developed to calculate the intersection form on vanishing cohomology at a critical point (Gabrielov, Gusein-Zade), monodromy groups (Gabrielov, Gusein-Zade, Varchenko), and asymptotics of oscillatory integrals (Varchenko). The mixed Hodge structure on vanishing cohomology was introduced (Steenbrink, Varchenko), and the Hodge numbers of the mixed Hodge structure were calculated in terms of Newton polygons (Danilov, Khovanskii); see [9], [14] and the references therein.

The emergence of extensive new experimental data led to new discoveries. For example, according to Arnold's classification, the unimodal singularities form one infinite series  $T_{p,q,r}$  and 14 exceptional families. Dolgachev discovered that the 14 exceptional unimodal singularities can be obtained from automorphic forms associated with the discrete groups of isometries of the Lobachevsky plane generated by reflections at the sides of some 14 triangles [15]. For the angles  $\pi/p, \pi/q, \pi/r$ of such a triangle, the numbers p, q, r are integers, called Dolgachev's triple. According to Gabrielov [16], the intersection form on vanishing cohomology at an exceptional unimodal singularity is described by another triple of integers, called Gabrielov's triple. V. I. Arnold noticed that Gabrielov's triple of an exceptional unimodal singularity equals Dolgachev's triple of (in general) another exceptional unimodal singularity, while Gabrielov's triple of that other singularity equals Dolgachev's triple of the initial singularity. Thus, there is an involution on the set of 14 exceptional unimodal singularities, called Arnold's strange duality. Much later, after discovery of the mirror symmetry phenomenon, it was realized that Arnold's strange duality is one of its first examples.

**Newton Polygons.** While classifying critical point of functions, Arnold noticed that, for all critical points of his classification, the Milnor number of the critical point can be expressed in terms of the Newton polygon of the Taylor series of that critical point. Moreover, an essential part of Arnold's classification was based on the choice of the coordinate system simplifying the Newton polygon of the corresponding Taylor series. (According to Arnold, he used "Newton's method of a moving ruler (line, plane)".) V. I. Arnold formulated a general principle: in the family of all critical points with the same Newton polygon, discrete characteristics of a typical critical point (the Milnor number, singularity index, Hodge numbers of vanishing cohomology, and so on) can be described in terms of the Newton polygon.

This statement was the beginning of the theory of Newton polygons. Newton polygons were one of the permanent topics of the seminar. The first result, the formula for the Milnor number in terms of the Newton polygon, was obtained by Kouchnirenko in [19]. After Kouchnirenko's report at Arnold's seminar, Lyashko formulated a conjecture that a similar statement must hold in the global situation: the number of solutions of a generic system of polynomial equations in n variables with a given Newton polygon must be equal to the volume of the Newton polygon multiplied by n!. Kouchnirenko himself proved this conjecture. David Bernstein [13] generalized the statement of Kouchnirenko's theorem to the case of polynomial equations with different Newton polygons and found a simple proof of his generalization. Khovanskii discovered the connection of Newton polygons; see [8] and the references therein. Varchenko calculated the zeta-function of the monodromy and asymptotics of oscillatory integrals in terms of Newton polygons; see [9].

Nowadays Newton polygons are a working tool in many fields. Newton polygons appear in real and complex analysis, representation theory, and real algebraic geometry; and the Newton polygons provide examples of mirror symmetry and so on.

Skiing and Swimming. Every year at the end of the winter Arnold's seminar went to ski on the outskirts of Moscow. This tradition started in 1973. While the number of seminar participants was between twenty and thirty people, no more than ten of the bravest participants came out to ski. People prepared for this event the whole winter. The meeting was at 8 a.m. at the railway station in Kuntsevo, the western part of Moscow, and skiing went on until after sunset, around 6 p.m. The daily distance was about 50 km.

Usually Arnold ran in front of the chain of skiers, dressed only in swimming trunks. He ran at a speed a bit above the maximal possible speed of the slowest of the participants. As a result, the slowest participant became exhausted after an hour of such an outing and was sent back to Moscow on a bus at one of the crossroads. Then the entire process was repeated again and another participant was sent back to Moscow after another hour. Those who were able to finish the skiing were very proud of themselves.

Only one time was the skiing pattern different. In that year we were joined by Dmitri Borisovich Fuchs, a tall, unflappable man, who was at one time a serious mountain hiker. Early in the morning when Arnold started running away from the station with us, Dmitri Borisovich unhurriedly began to walk in the same direction. Soon he completely disappeared from our view, and Arnold stopped and began waiting impatiently for Fuchs to arrive. Arnold again rushed to run and Fuchs, again unperturbed, unhurriedly followed the group. So proceeded the entire day. That day none of the participants of the run were sent home in the middle of the day. Several times we were joined by Olya Kravchenko and Nadya Shirokova, and every time they kept up the run as well as the best.

All participants of the ski-walk brought sandwiches, which they ate at a stop in the middle of the day. Before sandwiches there was bathing. In Moscow suburbs you will come across small rivers which are not frozen even in winter. We would meet at such a stream and bathe, lying on the bottom of the streambed as the water was usually only knee deep. We certainly did not use bathing suits, and there were no towels. The tradition of bathing in any open water at any time of the year Arnold had adopted from his teacher, Kolmogorov. This tradition was taken up by many participants of the seminar.

Arnold thought that vigorous occupation with mathematics should be accompanied by vigorous physical exercise. He skied regularly in the winter (about 100 km per week), and in summer rode a bicycle and took long walks.

There is a funny story connected to the tradition of bathing in any available open water. In 1983 the Moscow mathematicians were taken out to the Mathematical Congress in Warsaw. This congress had been boycotted by Western mathematicians. The large Soviet delegation was supposed to compensate for the small number of Western participants. A special Moscow-Warsaw-Moscow train had been arranged, which delivered us to Poland with Arnold. Once, walking across Warsaw in the evening with Arnold, we arrived at a bridge across the Vistula. While on the bridge we decided to bathe, as required by tradition. We reached the water in total darkness and swam for a few minutes. In the morning we found, to our amazement, that we were floating more in mud than water.

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## CHAPTER 17

# Topology in Arnold's Work

# VICTOR VASSILIEV

Arnold worked comparatively little on topology for topology's sake. His topological studies were usually motivated by specific problems from other areas of mathematics and physics: algebraic geometry, dynamical systems, symplectic geometry, hydrodynamics, geometric and quantum optics. So the (very significant) place of topological studies in his work is well balanced with the (equally very significant) place and applications of topology in the entirety of contemporary mathematics.

The main achievement in a number of his works is a proper recognition and formulation of a topological result, allowing topologists to enter the area with their strong methods. A huge part of Arnold's work is contained not in his own articles but in well-formulated problems and hints that he gave to his students and other researchers; see especially [8]. So I will discuss below such Arnold hints as well and what followed from them.

### Superpositions of Functions.

The case of real functions: Kolmogorov-Arnold's theorem and Hilbert's 13th problem. This theorem states that every continuous function of n > 2 variables can be represented by a superposition of functions in 2 variables (and the superposition can be taken in a particular form). The first approach to this problem (based on the notion of the Kronrod tree of connected components of level sets) was found by Kolmogorov (1956), who did not, however, overcome some technical low-dimensional difficulties and proved only the same theorem with 2 replaced by 3. The final effort was made by (then-19-year-old) Arnold.

This theorem gives a negative solution to (probably the most natural exact understanding of) the following Hilbert 13th problem:

...it is probable that the root of the equation of the seventh degree is a function of its coefficients which does not belong to this class of functions capable of nomographic construction, i.e., that it cannot be constructed by a finite number of insertions of functions of two arguments. In order to prove this, the proof would be necessary that the equation of the seventh degree

(2) 
$$t^7 + xt^3 + yt^2 + zt + 1 = 0$$

is not solvable with the help of any continuous functions of only two arguments.

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A widespread belief concerning this problem is as follows: "with the help of functions" in its last sentence means that a continuous solution t(x, y, z) of (2) should indeed be given by a function of the form described in the first one, i.e., by a superposition of continuous functions of two arguments. In this case the Kolmogorov-Arnold theorem would give a direct negative answer to this problem. Nevertheless, this understanding of Hilbert's question is probably erroneous, because (2) does not define any continuous function at all: the multivalued function t(x, y, z) defined by (2) does not have any continuous cross-section on the whole of  $\mathbb{R}^3_{(x,y,z)}$ . Indeed, such negative-valued cross-sections do not already exist in a small neighborhood of the polynomial

$$t^{7} - 14t^{3} - 21t^{2} - 7t + 1$$
  
$$\equiv (t+1)^{3}(t^{4} - 3t^{3} + 6t^{2} - 10t + 1).$$

Such a neighborhood admits two *positive-valued* cross-sections, but they obviously cannot be continued to the polynomial  $t^7 + 1$ . So this direct understanding of the Hilbert problem could be correct only under the (quite improbable) conjecture that Hilbert has included in this problem the question whether (or was confident that) (2) defines a continuous function on the entire  $\mathbb{R}^3$ ; in this case the problem would have a positive solution.

A more realistic assumption is that "with the help of continuous functions of two variables" means something more flexible, for example, that we can consider a triple of functions  $(\chi, g_1, g_2)$  in x, y, z, defined by such superpositions, and represent our function t(x, y, z) by  $g_1$  in the area where  $\chi > 0$  and by  $g_2$  where  $\chi \leq 0$ . However, in this case it is unclear why Hilbert did not believe that the desired representation (maybe with more functions  $\chi_k$  and  $g_i$ ) does exist for his particular function, which is piecewise analytic and certainly can be stratified by easy conditions into pieces with very simple behavior. The most realistic conjecture is that (like for many other problems) Hilbert wrote a slightly obscure sentence specifically to let the readers themselves formulate (and solve) the most interesting and actual exact statements: it is exactly what Kolmogorov and Arnold actually did.

*Complex algebraic functions and braid cohomology.* Hilbert's 13th problem, formally asking something about real continuous functions, is nevertheless evidently motivated by the study of superpositions of multivalued algebraic functions in complex variables. A dream problem in this area is to solve literally the same problem concerning such functions. Moreover, this problem was explicitly formulated in one of Hilbert's consequent works.

Arnold worked much on this problem, revising and reformulating the proof of the Ruffini-Abel theorem in topological terms of ramified coverings and their topological invariants and trying to extend it to superpositions of functions in more variables. Although the exact desired theorem was not proved, a byproduct of this attack was huge: among other topics, it contains the topological theory of generalized discriminants, homological theory of braid groups, and theory of plane arrangements. A particular result, the topological obstruction to the representation by *complete* superpositions of functions depending on few variables, was expressed in [4] in the terms of cohomology of braid groups. Indeed, the *d*-valued algebraic function  $t(x_1, \ldots, x_d)$  given by

(3) 
$$t^d + x_1 t^{d-1} + \dots + x_{d-1} t + x_d = 0$$

defines a *d*-fold covering over the set  $\mathbb{C}^d \setminus \Sigma$  of *nondiscriminant* points  $(x_1, \ldots, x_d)$ (i.e., of polynomials (3) for which all *d* values t(x) are different). This covering defines (up to homotopy) a map from its base  $\mathbb{C}^d \setminus \Sigma$  to the classifying space K(S(d), 1) of all *d*-fold coverings, thus also a canonical map

(4) 
$$H^*(K(S(d), 1) \to H^*(\mathbb{C}^d \setminus \Sigma).$$

If our algebraic function (3) is induced from another one, as in the definition of complete superpositions, then this cohomology map factorizes through the cohomology ring of some subset of the argument space of this new algebraic function. Hence the dimension of this space cannot be smaller than the highest dimension in which the map (4) is nontrivial.

This approach has strongly motivated the study of the cohomology ring of the space  $\mathbb{C}^d \setminus \Sigma$  (which is the *classifying space of the d-braid group*) and, much more generally, of the following objects.

**Discriminants and Their Complements.** Given a space of geometric objects (say, functions, varieties, subvarieties, matrices, algebras, etc.), the *discriminant subset* in it consists of all degenerate (in some precise sense) objects: it may be the set of non-Morse functions or self-intersecting spatial curves, or degenerate (another version: having multiple eigenvalues) operators. Usually one studies the complementary space of nonsingular objects. However, Arnold's seminal reduction replaces the homological part of this study by that of discriminant spaces. Namely, in [3], Arnold exploits the Alexander isomorphism

(5) 
$$H^{i}(\mathbb{C}^{d} \setminus \Sigma) \equiv \overline{H}_{2d-i-1}(\Sigma),$$

where  $\overline{H}_*$  means the homology of the one-point compactification and  $\mathbb{C}^d$  is considered to be the space of all complex polynomials (3) in one variable t. This reduction turned out to be extremely fruitful, because the set of nonsingular objects is usually open and does not carry any natural geometric structure. To study its topology, we often need to introduce some artificial structures on it, such as Morse functions, connections, families of vector fields or plane distributions, etc., which can have singularities helping us to calculate some topological invariants. On the other hand, the discriminant varieties are genuinely stratified sets (whose stratification corresponds to the hierarchy of singularity types); this stratification allows one to calculate various topological properties of these varieties and hence also of their complementary sets of generic objects. Already in [3] this approach has brought some progress, although the complete calculation of the group (5) was done only later by D. Fuchs for  $\mathbb{Z}_2$ -cohomology [9] and by F. Cohen and F. Vainshtein for integral cohomology.

Using the same approach, Arnold studied later many other spaces of nondegenerate objects, namely, spaces  $P_d \setminus \Sigma_k$  of real degree d polynomials  $\mathbb{R}^1 \to \mathbb{R}^1$ without roots of multiplicity  $\geq k, k \geq 3$ , spaces of functions  $\mathbb{R}^1 \to \mathbb{R}^1$  (with a fixed behavior at infinity) also having no zeros of multiplicity  $\geq k$  (1989), spaces of Hermitian operators with simple spectra (1995), spaces of generic (or generic Legendrian) plane curves (1994), etc.

Another very important idea of Arnold's in this area was his favorite *stabilization problem*, published first in 1976 and repeated many times in seminars; see problems 1975-19, 1980-15, 1985-7, 1985-22 in [8]. Formally speaking, the Alexander duality theorem is a finite-dimensional result. Also, all spaces of objects in



FIGURE 1. Stabilization of unfoldings.

which Arnold's approach originally led to more or less explicit results were finitedimensional spaces considered as unfoldings of some particular objects. For example, the space  $\mathbb{C}^d$  of complex polynomials (3) can be considered as an unfolding of the monomial  $t^d$ . When the degree d grows, the cohomology groups of spaces  $\mathbb{C}^d \setminus \Sigma$  of nondiscriminant polynomials stabilize (to the cohomology of the infinite braid group), but it was quite difficult to trace the stabilization process in terms of the original calculations. Moreover, it was unclear what happens with similar stabilizations for objects more complex than just polynomials in one variable, how to deal with similar infinite-dimensional problems, and what is "the mother of all unfoldings". To attack this set of philosophical problems, Arnold formulated a very explicit sample problem. First, he noticed that the stabilization of cohomology groups such as (3) is natural: if we have two singular objects, one of which is "more singular" than the other, then the parameter space of the unfolding of the simpler object can be embedded into that of the more complicated one. This map sends one discriminant into the other, thus inducing the pull-back map of cohomology groups of their complements. (For real polynomials  $t^3$  and  $t^4$  this embedding of parameter spaces of their unfoldings  $t^3 + at + b$  and  $t^4 + \alpha t^2 + \beta t + \gamma$  is shown in Figure 1. The discriminants drawn in this picture are the sets of polynomials having multiple roots.)

Arnold's respective problem was to determine the stable (under all such pullback maps) cohomology groups of such complements of discriminants of isolated singularities of holomorphic functions in  $\mathbb{C}^n$  (and to prove that they actually do stabilize; i.e., these stable cohomology groups are realized by such groups for some sufficiently complicated singularities). Solving this problem, I found in 1985 a method of calculating homology groups of discriminants that behaves nicely under the embeddings of unfoldings and thus gives an effective calculation of stable groups. Some elaborations and byproducts of this calculation method constitute a majority of my results on topology of discriminants, including my first works on knot theory. In the original problem on stable cohomology of complements of discriminants of holomorphic functions, this calculation gives us the following formula: the desired stable cohomology ring for singularities in n complex variables is equal to  $H^*(\Omega^{2n}S^{2n+1})$ , where  $\Omega^k$  is the k-fold loop space.

Moreover, this Arnold problem not only dealt with the stabilization of particular finite-dimensional objects, but it also gave an approach to the study of actual infinite-dimensional function spaces.

Topology of Pure Braid Groups and Plane Arrangements. Together with the cohomology of the usual braid groups (3), Arnold also investigated the pure braid group, i.e., the fundamental group of the set of ordered collections of d distinct points in  $\mathbb{C}^1$ . The classifying space of this group is just the space  $\mathbb{C}^d$ with all diagonal hyperplanes  $\{x_i = x_j \text{ for } i \neq j\}$  removed. Arnold's calculation of its cohomology group [2] became a sample and a starting point of numerous generalizations and initiated the so-called theory of plane arrangements. The Arnold identity

$$\omega_{ij} \wedge \omega_{jk} + \omega_{jk} \wedge \omega_{ki} + \omega_{ki} \wedge \omega_{ij} = 0$$

for basic classes of this cohomology ring later became one of the main ingredients of Kontsevich's construction of the universal finite-type knot invariant.

Maslov Index, Lagrange and Legendre Cobordism. Lagrange manifolds are specific *n*-dimensional submanifolds of the symplectic space  $\mathbb{R}^{2n}$  (or, more generally, of the cotangent bundle of an arbitrary manifold  $M^n$ ). They occur in problems of geometric optics as the manifolds into which all rays of light considered in such a problem can be lifted without intersections, and in quantum optics as a first step in obtaining an asymptotic approximation of light diffusion. However, further steps of this asymptotic description impose some consistency condition: the composition of transition functions relating their expressions in neighboring local charts should define the identity operator when we go along a closed chain of such charts. This condition is best formulated in terms of a certain 1-cohomology class of the Lagrange manifold, its Maslov index. If the Lagrange manifold  $L^n \subset T^*\mathbb{R}^n$ is generic, then this index can be defined as the intersection index with the singular locus of the projection  $L^n \to \mathbb{R}^n$  to the "physical" configuration space. It is important for this definition that, for generic Lagrangian manifolds, this locus has a well-defined transversal orientation (so that crossing it, we can always say whether we are going to the positive or the negative side) and its singular points form a subset of dimension at most n-3 in  $L^n$  (so that all homologous curves have one and the same Maslov index). If  $L^n$  is orientable, then this index is even; the above self-consistency condition requires that the value of this index on any closed curve should be a multiple of 4. Arnold [1] related this index with the topology of the Lagrange Grassmann manifold of all Lagrangian planes in the symplectic  $\mathbb{R}^{2n}\text{-}\mathrm{space},$  i.e., of all planes that can be tangent to some Lagrange submanifolds in this space. This settles immediately various problems related to the invariance of the definition of the Maslov index, as well as to its stability under deformations of the Lagrange manifold.

In 1980 Arnold initiated the theory of Lagrange and Legendre cobordisms [7]. Light distribution in the area defines light distribution on its border: for instance, the reflected light on the wall is defined by the light in the entire room. This means that a Lagrange manifold in the cotangent bundle of the room defines its Lagrange boundary, which is a Lagrange manifold in the cotangent bundle of the wall. The Legendre manifolds are known to us mainly as resolutions of wave fronts. The wave

front evolving in space defines a wave front of bigger dimension in the space-time. The fronts in  $M^n$  corresponding to some instants  $T_1$  and  $T_2$  are obviously defined by the big front in  $M^n \times [T_1, T_2]$ ; the way in which they are obtained from this big front can be generalized to the notion of the Legendre boundary. Notice that both Lagrange and Legendre boundaries of manifolds are not their boundaries and not even the subsets in the usual sense: they are obtained from these boundaries by symplectic and contact reductions.

Arnold introduced cobordism theories based on these boundary notions and calculated the 1-dimensional Lagrange and Legendre cobordism groups: they turned out to be isomorphic to  $\mathbb{Z} \oplus \mathbb{R}$  and  $\mathbb{Z}$ , respectively. The  $\mathbb{Z}$ -term in both answers is defined by the Maslov index, the  $\mathbb{R}$ -invariant of the Lagrange cobordism is given by  $\int pdq$ . Later, Ya. Eliashberg and M. Audin, using the Gromov-Lees version of the Smale-Hirsch *h*-principle for Lagrange manifolds, reduced the calculation of Legendre cobordism groups in any dimension to the standard objects of the cobordism theory, namely, to homotopy groups of appropriate Thom spaces (over the stable Lagrange Grassmann manifold).

At the same time, in the beginning of 1980, Arnold asked me whether it is possible to extend the construction of the Maslov index to cohomology classes of higher dimensions, dual to more degenerate singular loci of the Lagrangian projection  $L^n \to \mathbb{R}^n$  than just the entire singular set. The resulting cohomology classes were expected to be closely related to the higher cohomology classes of Lagrange Grassmannians and to give invariants of Lagrange and Legendre cobordisms. The answer was found soon: I managed to construct the desired characteristic classes in terms of the universal complex of singularity types. Later, this theory was nicely and strongly extended by M. Kazarian in terms of equivariant homology.

On the other hand, the work with 1-dimensional wave fronts led Arnold to many essential problems of contact geometry, such as the 4-cusps problem (see the photograph above). Solutions of these problems by Chekanov, Eliashberg, Pushkar'and others resulted in significant development of this area.

There are many other topological results in Arnold's works, including major breakthroughs in real algebraic geometry [5], [6]; Arnold's conjecture in symplectic topology; the asymptotic Hopf invariant; and the vanishing homology theory of boundary singularities. These topics are covered in other articles in this collection.

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### CHAPTER 18

# Arnold and Symplectic Geometry

## Helmut Hofer

V. I. Arnold was a character and a larger-than-life figure. I never knew him extremely well, but we became closer over the years, and I learned to know him a little bit more from the private side. He could be very charming.

As a student I read Arnold's wonderful book *Mathematical Methods of Classical Mechanics* and was impressed by the ease with which he was able to bring across important ideas. I never expected to meet him in real life.

I met him for the first time when I was a tenure-track professor at Rutgers University and was visiting the Courant Institute. This was between 1986 and 1987, so around three years before the Berlin Wall and the iron curtain came down. The Courant Institute had worked hard to make it possible for Arnold to visit. I attended one of Arnold's lectures, which was remarkable in two ways: there was great mathematics and something one would not expect in a mathematics lecture. At some point he went into a tirade about how Western mathematicians were not giving proper credit to Russian mathematicians. Most people in the audience took it with some kind of amusement, but not all. Somebody sitting beside me mumbled something along the lines that we should have left him in Moscow.

A year or so later he attended parts of the symplectic year (1988) at MSRI in Berkeley. What I remember from his visit was that at some point he decided to swim in San Francisco Bay. One has to know that the locals do not consider this the best idea, since the currents are quite unpredictable. The story which was told at that time was that he almost drowned fighting the currents. I thought to myself, "That is a really interesting multidimensional character pushing the envelope." I recently asked about this of Richard Montgomery, who had an account of this story from Arnold himself. He had concluded from the description that Arnold had tried to swim from Marina Green to Marin (linked by the Golden Gate Bridge) during ebb tide and at some point, in his own words, "It felt like I hit a wall of current" and "had to turn back." The maximum ebb out of the San Francisco Bay can be over six knots. If he hadn't returned, he would have been swept at least a mile out to sea. Talking to Richard I also learned about another story. He and Arnold went kayaking in the bay. After an involuntary Eskimo roll, Arnold insisted on entering orthogonally into the path of an ongoing yacht race, with 40-foot yachts going full speed being unable to dodge a kayak. Richard still remembers his fear of going down in mathematical infamy as the guy who killed Arnold. As I said before, Arnold pushed the envelope in real life as he did in mathematics.

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#### HELMUT HOFER

One year later, in 1989, I became a full professor at the Ruhr-Universität Bochum. Shortly afterwards the Berlin Wall came down, with dramatic changes in Eastern Europe. Soon a complete brain drain of the Soviet Union became a concern, and one day I found myself, together with my colleagues A. Huckleberry and V. Arnold, presiding over some research funding to allow Russian mathematicians to spend longer periods with a decent pay at Bochum. Arnold was very concerned, and I got to know him somewhat better. Professor Arnold became Dima.

Around 1994 I met him again; this time in Stanford. Dima, Yasha (Eliashberg), and I went looking for walnuts at the San Andreas Fault. I am sure it was Dima's idea. Knowing the "almost drowning version" of Dima's swimming expedition in San Francisco Bay, I had quite high expectations for the afternoon. However, there was no earthquake.

Around this point we started talking about mathematics, specifically symplectic topology. His opening bid was, "Helmut, you are using the wrong methods," referring to pseudoholomorphic curves, and I responded with, "I am sure you know something better. Make my day!" He liked to probe and enjoyed seeing people's reactions. I think I did well that day.

In 1998 he introduced my plenary lecture at the ICM in Berlin, and we had a friendly chat before the talk. The year before I had moved to the Courant Institute. He said, "Helmut, you should come back to Europe." I answered, "No, Dima, I love New York. But if it makes you feel better, consider me the agent of European culture in the U.S." I saw immediately that he liked this sentence. We talked about some more things which I rather thought would stay between us. Of course, I should have known better! He made it all part of his introduction and started by introducing me as the agent of European culture in the U.S., to the delight of many, but that was only the beginning; the rest is on video.

Dima had an amazing mathematical intuition and (which at this point shouldn't come as a surprise) was daring enough to make conjectures when others would not dare to stick their necks out.

There are quite a number of Arnold conjectures in symplectic geometry. However, there is one which even people outside of the field know and which was the initial driving force behind the development of symplectic geometry.

Arnold and Weinstein developed the modern language of symplectic geometry. This could, for example, be used to prove interesting perturbation results. However, there were no global results. Arnold was the one who raised these types of questions, and the Arnold conjecture I describe below is an example. Surprisingly, the breakthrough due to Conley and Zehnder came from outside the field.

In the following, I try to motivate the Arnold Conjecture. One can understand it as an analogy of the relationships between the Euler characteristic, the Hopf index formula, and the Lefschetz fixed point theorem. I haven't seen anything in his writings pointing out this analogy, but added it here as an intermediate step which helps to understand the conjecture better. Arnold describes a way of reasoning in Appendix 9 of his previously mentioned book. The Poincaré twist theorem can be seen as a special case of the two-dimensional torus case of his conjecture. The general case would be the generalization of the torus case to arbitrary closed symplectic manifolds. There is quite often a difference between the original thought process and the didactical cleaned version. From that point of view I regret that I never asked him how he arrived at his conjecture. The discussion below adds another point of view, constructing an analogy to a reasoning in topology. I very much believe that Arnold was aware of this analogy.

We start with a closed oriented manifold M and a vector field X. The Euler characteristic  $\chi(M)$  is a classical topological invariant, which is a generalization of the original concept introduced for polyhedra by Euler and which was fully generalized later by Poincaré. If M is a smooth manifold, Hopf's index formula establishes a relationship between the zeros of a vector field assumed to be transversal to the zero section and the Euler characteristic of M:

$$\chi(M) = \sum_m i(X,m),$$

where  $i(X, m) = \pm 1$  is the local index at a zero of X.

How can we generalize this? First we observe that a diffeomorphism can be viewed as a generalization of a vector field. Indeed, the collection of smooth vector fields can be viewed as the Lie algebra of the (Fréchet)-Lie group Diff(M), so as an infinitesimal version of the latter. It is, however, not true that the diffeomorphisms close to the identity are in the image of the group exponential map. This is a consequence of being only a Fréchet Lie group and a universal problem in dealing with various sorts of diffeomorphism groups. Let us make a conjecture, which will come out as first going from the infinitesimal to the local to gain some confidence. We fix as an auxiliary structure a Riemannian metric with associated Riemannian exponential map exp. Assume that  $\Phi$  is a diffeomorphism which is close to the identity. Then we can write  $\Phi$  in a unique way in the form

$$\Phi(m) = \exp_m(X(m)),$$

for a small vector field X. Tranversality of X to the zero-section is equivalent to  $\Phi$  not having 1 in the spectrum of its linearizations at fixed points. Most importantly, the fixed points for  $\Phi$  correspond to the zeros for X. Hence a generic diffeomorphism which is close to the identity has an algebraic fixed point count  $\chi(M)$ , where the sign is taken according to  $\Phi'(m)$  being orientation preserving or not. We can now make the "daring conjecture" that this should hold for all generic diffeomorphims isotopic to the identity. That turns out to be correct and is, of course, a special case of the Lefschetz fixed point formula.

What Arnold did in symplectic geometry is such a daring conjecture in a more complicated context. We start with a closed symplectic manifold  $(M, \omega)$ , and in analogy to the previous discussion we generalize the theory of functions on M rather than the theory of vector fields. If f is a smooth function with all critical points nondegenerate, then Morse theory says its number of critical points is at least the sum of the Betti numbers (for any coefficient field). Morse theory also tells us that the algebraic count of critical points is  $\chi(M)$ . Since we have a symplectic structure, we can associate to f a vector field  $X_f$  by the obvious formula

$$df = i_{X_f} \omega.$$

This is the so-called Hamiltonian vector field. Obviously we are now back to the first discussion. However, with the vector fields being more special, one would like a stronger statement for a certain class of diffeomorphisms. This particular class of diffeomorphisms should generalize functions as diffeomorphisms isotopic to the identity generalize vector fields.

#### HELMUT HOFER

Symplectic diffeomorphisms isotopic to the identity are not a good guess, since for  $T^2$  with the standard symplectic form a small translation would give no fixed points at all. We could, however, look at all symplectic diffeomorphisms obtained as time-1 maps for the family of vector fields  $X_{f_t}$  for a smooth time-dependent family  $f : [0,1] \times M \to \mathbb{R}$ , with  $f_t(x) := f(t,x)$ . This produces the group of all Hamiltonian diffeomorphisms  $\operatorname{Ham}(M,\omega)$ . Indeed the collection of smooth maps can be viewed as the Lie algebra for  $\operatorname{Ham}(M,\omega)$ .

How can we go from the infinitesimal to the local, as we did in the previous discussion? A basic and not too difficult symplectic result is that the neighborhood of a Lagrangian submanifold of a symplectic manifold is symplectically isomorphic to a neighborhood of the zero-section in its cotangent bundle with the natural symplectic structure. Now comes a little trick which replaces the use of the exponential map associated to an auxiliary metric. We define  $N = M \times M$  with the form  $\tau = \omega \oplus (-\omega)$ . Then the diagonal  $\Delta_M$  is a Lagrange submanifold of N, and an open neighborhood of it looks like an open neighborhood of  $\Delta_M$  in  $T^*\Delta_M$ . Every symplectic map that is sufficiently close to the identity has a graph which when viewed as a subset of  $T^*\Delta_M$  is a graph over the zero-section, i.e., the graph of a oneform  $\lambda$ . An easy computation shows that the original diffeomorphism is symplectic if and only if  $\lambda$  is closed. It is Hamiltonian if and only if  $\lambda$  is exact:

 $\lambda = dg$ 

for some smooth function. Hence the fixed points of a Hamiltonian diffeomorphism  $\Phi$  correspond to the intersection of its graph with the zero-section and hence with the critical points of g. Now we are in the local situation, similarly as in the previous case. We conclude that a generic element in  $\operatorname{Ham}(M, \omega)$  has at least as many fixed points as a smooth function has critical points if it is close enough to the identity map.

Knowing all this, Arnold makes the following daring conjecture (nondegenerate case, in my words).

**Arnold Conjecture:** A nondegenerate Hamiltonian diffeomorphism has at least as many fixed points as a Morse function has critical points.

It wouldn't be Dima if it actually was that straightforward. The most prominent statement "Arnold-style" of this conjecture is in his book *Mathematical Methods of Classical Mechanics*. In the Springer 1978 edition (being a translation of the 1974 Russian edition) it reads on page 419 (and this is a restatement of some published version of the conjecture in 1965):

> Thus we come to the following generalization of Poincaré's theorem:

> **Theorem.** Every symplectic diffeomorphism of a compact symplectic manifold, homologous to the identity, has at least as many fixed points as a smooth function on this manifold has critical points (at least if this diffeomorphism is not too far from the identity).

The symplectic community has been trying since 1965 to remove the parenthetical part of the statement. After tough times from 1965 to 1982, an enormously fruitful period started with the Conley-Zehnder theorem in 1982–83, proving the Arnold conjecture for the standard torus in any (even) dimension using Conley's index theory (a powerful version of variational methods). This was followed by Gromov's pseudoholomorphic curve theory coming from a quite different direction. At this point the highly flexible symplectic language becomes a real asset in the field. Finally, Floer combines the Conley-Zehnder viewpoint with that of Gromov, which is the starting point of Floer theory in 1987. As far as the Arnold conjecture is concerned, we understand so far a homological version of the nondegenerate case. A Luisternik-Shnirelman case (also conjectured by Arnold) is still wide open, though some partial results are known.

The development of symplectic geometry has been and still is a wonderful journey. Thanks, Dima!

#### CHAPTER 19

## Some Recollections of Vladimir Igorevich

### Mikhail Sevryuk

A very large part of my life is connected with Vladimir Igorevich Arnold. I became his student in the beginning of 1980 when I was still a freshman at the Department of Mechanics and Mathematics of Moscow State University. Under his supervision, I wrote my term papers, master's thesis, and doctoral thesis. At the end of my first year in graduate school, Arnold suggested that I write a monograph on reversible dynamical systems for Springer's Lecture Notes in Mathematical biography. For the last time, I met Vladimir Igorevich (V. I., for short) on November 3, 2009, at his seminar at Moscow State.

If I had to name one characteristic feature of Arnold as I remember him, I would choose his agility. He walked fast at walkways of Moscow State (faster than most of the students, not to mention the faculty), his speech was fast and clear, his reaction to one's remark in a conversation was almost always instantaneous, and often utterly unexpected. His fantastic scientific productivity is well known, and so is his enthusiasm for sports.

V. I. always devoted a surprising amount of time and effort to his students. From time to time, he had rather weak students, but I do not recall a single case when he rejected even a struggling student. In the 1980s almost every meeting of his famous seminar at Moscow State he started with "harvesting": collecting notes of his students with sketches of their recent mathematical achievements or drafts of their papers (and Arnold returned the previously collected ones with his corrections and suggestions). After a seminar or a lecture, he often continued talking with participants for another 2–3 hours. Arnold's generosity was abundant. Many times, he gave long written mathematical consultations, even to people unknown to him, or wrote paper reports substantially exceeding the submitted papers. In recent years, he used all his energy to stop a rapid deterioration of mathematical (and not only mathematical) education in Russia.

I tried to describe my experience of being V. I.'s student at Moscow State in [7]. I would like to emphasize here that Arnold did not follow any pattern in supervising his students. In some cases he would inform a student that there was a certain "uninhabited" corner in the vast mathematical land, and if the student decided to "settle" at that corner, then it was this student's task to find the main literature on the subject, to study it, to pose new problems, to find methods of their solution, and to achieve all this practically single-handedly. Of course, V. I.

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kept the progress under control. (I recall that, as a senior, I failed to submit my "harvest" for a long time, but finally made substantial progress. Arnold exclaimed, "Thank God, I have started fearing that I would have to *help* you!") But in other situations, Arnold would actively discuss a problem with his student and invite him to collaborate— this is how our joint paper [4] came about. When need be, V. I. could be rather harsh. Once I witnessed him telling a student, "You are working too slowly. I think it will be good if you start giving me weekly reports on your progress." Arnold never tried to spare one's self-esteem.

V. I. had a surprising feeling of the unity of mathematics, of natural sciences, and of all nature. He considered mathematics as being part of physics, and his "economics" definition of mathematics as a part of physics in which experiments are cheap is often quoted. (Let me add in parentheses that I would prefer to characterize mathematics as the natural science that studies the phenomenon of infinity by analogy with a little-known but remarkable definition of topology as the science that studies the phenomenon of continuity.) However, Arnold noted other specific features of mathematics: "It is a fair observation that physicists refer to the first author, whereas mathematicians to the latest one." (He considered adequate references to be of paramount importance and paid much attention to other priority questions; this was a natural extension of his generosity, and he encouraged his students to "over-acknowledge", rather than to "under-acknowledge".)

V. I. was an avid fighter against "Bourbakism", a suicidal tendency to present mathematics as a formal derivation of consequences from unmotivated axioms. According to Arnold, one needs mathematics to discover new laws of nature as opposed to "rigorously" justify obvious things. V. I. tried to teach his students this perception of mathematics and natural sciences as a unified tool for understanding the world. For a number of reasons, after having graduated from university, I had to work partially as a chemist, and after Arnold's school this caused me no psychological discomfort.

Fundamental mathematical achievements of Arnold, as well as those of his teacher, A. N. Kolmogorov, cover almost all mathematics. It well may be that V. I. was the last universal mathematician. My mathematical specialization is the KAM theory. V. I. himself described the contributions of the three founders; see, e.g., [5], [6]. For this reason, I shall only briefly recall Arnold's role in the development of the KAM theory.

KAM theory is the theory of quasiperiodic motions in nonintegrable dynamical systems. In 1954 Kolmogorov made one of the most astonishing discoveries in mathematics of the last century. Consider a completely integrable Hamiltonian system with n degrees of freedom, and let  $(I, \varphi)$  be the corresponding action-angle variables. The phase space of such a system is smoothly foliated into invariant n-tori  $\{I = \text{const}\}$  carrying conditionally periodic motions  $\dot{\varphi} = \omega(I)$ . Kolmogorov showed that if  $\det(\partial \omega/\partial I) \neq 0$ , then (in spite of the general opinion of the physical community of that time) most of these tori (in the Lebesgue sense) are not destroyed by a small Hamiltonian perturbation but only slightly deformed in the phase space. To be more precise, a torus  $\{I = I^0\}$  persists under a perturbation whenever the frequencies  $\omega_1(I^0), \ldots, \omega_n(I^0)$  are Diophantine (strongly incommensurable). The perturbed tori (later called Kolmogorov tori) carry quasiperiodic motions with the same frequencies. To prove this fundamental theorem, Kolmogorov proposed a

new, powerful method of constructing an infinite sequence of canonical coordinate transformations with accelerated ("quadratic") convergence.

Arnold used Kolmogorov's techniques to prove analyticity of the Denjoy homeomorphism conjugating an analytic diffeomorphism of a circle with a rotation (under the condition that this diffeomorphism is close to a rotation and possesses a Diophantine rotation number). His paper [1] with this result contained also the first detailed exposition of Kolmogorov's method. Then, in a series of papers, Arnold generalized Kolmogorov's theorem to various systems with degeneracies. In fact, he considered two types of degeneracies often encountered in mechanics and physics: the *proper degeneracy*, where some frequencies of the perturbed tori tend to zero as the perturbation magnitude vanishes, and the *limit degeneracy*, where the unperturbed foliation into invariant tori is singular and includes tori of smaller dimensions. The latter degeneracy is modeled by a one-degree-of-freedom Hamiltonian system having an equilibrium point surrounded by invariant circles (the energy levels). These studies culminated in Arnold's famous (and technically extremely hard) result [2] on stability in planetary-like systems of celestial mechanics where both the degeneracies combine.

Kolmogorov and Arnold dealt only with analytic Hamiltonian systems. On the other hand, J. K. Moser examined the finitely smooth case. The acronym "KAM" was coined by physicists F. M. Izrailev and B. V. Chirikov in 1968.

Arnold always regarded his discovery of the universal mechanism of instability of the action variables in nearly integrable Hamiltonian systems with more than two degrees of freedom [3] as his main achievement in the Hamiltonian perturbation theory. He also constructed an explicit example where such instability occurs. Chaotic evolution of the actions along resonances between the Kolmogorov tori was called "Arnold's diffusion" by Chirikov in 1969. In the case of two degrees of freedom, the Kolmogorov 2-tori divide a three-dimensional energy level, which makes an evolution of the action variables impossible.

All these works by Arnold took place in 1958–1965. At the beginning of the eighties, he returned to the problem of quasiperiodic motions for a short time and examined some interesting properties of the analogs of Kolmogorov tori in reversible systems. That was just the time when I started my diploma work. So V. I. forced me to grow fond of reversible systems and KAM theory, for which I'll be grateful to him forever.

I would like to touch on yet one more side of Arnold's research. In spite of what is occasionally claimed, Arnold did not hate computers: he considered them as an absolutely necessary instrument of mathematical modeling when indeed large computations were involved. He initiated many computer experiments in dynamical systems and number theory and sometimes participated in them (see [6]). But of course he strongly disapproved of the aggressive penetration of computer technologies into all pores of society and the tendency of a man to become a helpless and mindless attachment to artificial intelligence devices. One should be able to divide 111 by 3 without a calculator (and, better still, without scrap paper).

V. I. had a fine sense of humor. It is impossible to forget his somewhat mischievous smile. In conclusion, here are a couple of stories which might help to illustrate the unique charm of this person. I remember how a speaker at Arnold's seminar kept repeating the words "one can lift" (a structure from the base to the total space of a bundle). Arnold reacted: "Looks like your talk is about results in weight-lifting."

On another occasion, Arnold was lecturing, and the proof of a theorem involved tedious computations: "Everyone must make these computations once—but only once. I made them in the past, so I won't repeat them now; they are left to the audience!"

In the fall of 1987 the Gorbachev *perestroika* was gaining steam. A speaker at the seminar was drawing a series of pictures depicting the perestroika (surgery) of a certain geometrical object as depending on a parameter. Arnold: "Something is not quite right here. Why is your central stratum always the same? Perestroika always starts at the center and then propagates to the periphery."

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### CHAPTER 20

# Remembering V. I. Arnold

## LEONID POLTEROVICH

Those who know the material will not learn anything new, and those who do not know it will not understand anything.

V. I. Arnold about a badly written introduction.

I'd like to write a couple of words about Vladimir Arnold, a great man whom I had the privilege to know and to whom I owe a lot; the man whose name appears in virtually every mathematical discussion among my colleagues working in symplectic topology and dynamical systems: Arnold's conjecture, KAM theory with A for Arnold, Liouville-Arnold theorem, Arnold's tongue, Arnold's diffusion, Arnold's cat map, etc., etc.

Arnold was one of the major attractions, one of the wonders of Moscow mathematical life in the 1980s. He was a charismatic lecturer and the organizer of a famous seminar. He authored a groundbreaking book that turned classical mechanics (which, before Arnold's era, had been a vague subject full of monsters such as virtual displacement) into an exciting branch of modern mathematics. He was one of the founders of the singularities theory and of symplectic topology. He was a celebrity. He knew this and considered it as a very serious responsibility.

Moscow mathematical life of the 1980s had the following structure. The official layer included the Moscow State University and the Steklov Institute, both with a strong anti-Semitic flavor and strictly controlled by the Communist Party and the KGB. Numerous scientists with "Jewish roots" were doing mathematics as a hobby, in addition to their full-time jobs as engineers and researchers in obscure industrial research institutes. Fortunately, there was also an unofficial layer, a kind of mathematical oasis, where these "outsiders" had the luxury to be supervised by several world-acclaimed gurus (Arnold, Gelfand, Manin, Novikov, Sinai).

Arnold made an effort to turn his seminar into a great show. Speakers were props, while Arnold was the star. But usually the speakers benefited from this arrangement because Arnold explained to them their own results, so they could finally understand what they have proved.

Once Arnold, an hour before the seminar, asked me to talk "since the assigned speaker proved several new theorems while preparing his lecture and got so overexcited that he cannot speak today." I said, "Sure, I just proved a new theorem and will be happy to talk about it." Arnold disagreed and suggested to talk about

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another recent result. Not surprisingly, my talk was a bit disorganized. Arnold interrupted me in the middle and exclaimed, while looking at the audience, "You see, this speaker did not even bother to prepare his talk carefully!" Then everybody laughed including myself and himself. After the talk, Arnold said that I should write a paper on these results. When he saw the first draft, he did not like the introduction. He invited me to a meeting, where he had to be present. We sat in the last row of a huge lecture hall, and he started rewriting the introduction. He was writing calligraphically, leaving huge spaces between the lines and inserting corrections into these spaces from time to time. I returned home and read Arnold's text carefully. To my great surprise, Arnold had outlined more general and more interesting theorems than I had actually proved in the first draft of the paper. All of them were provable and correct, and at the end of the day the paper turned out quite different. Arnold OK'd the paper and helped me to publish it.

This story is not an exception. Arnold was always surrounded by a crowd of young people with whom he discussed mathematics. It was a different Arnold: not a showman, but a patient and eager-to-help teacher. He was *available* in a way, unimaginable by Western standards. It was fine to call him at home and discuss mathematics for an hour. He carefully listened and asked questions which in fact were so detailed that, to a high extent, contained the answers so all that what remained was to work them out. Needless to say, Arnold did not co-author the papers resulting from these discussions. On the contrary, he was creating an extra headache for himself, since afterwards he had to arrange for a publication, which was quite a non-trivial task, especially if the author was Jewish. Furthermore, the crowd often included students from other schools and groups who brought to Arnold their own mathematical problems, so Arnold's behavior was a clear-cut altruism.

Why was Arnold so fully dedicated to the time-consuming task of entertaining and supervising this crowd of "outsiders"? Was it a pure mathematical interest? Maybe, but only a few and not very often succeeded to surprise the Master. Was it a pedagogical interest? Maybe, but a group of his own Ph.D. students at the University would be more than enough for that purpose. So what was it? I actually think that it was Arnold's well-thought-out response to the oppressive official Soviet mathematical establishment. He considered this as his obligation towards Russia whose culture and tradition he loved - as opposed to the ruling communist clique. Much later, while visiting Tel Aviv, Arnold said explicitly that he "would not hesitate to go to the gallows for the crimes of his generation". That time I thought it was another joke by Arnold.

Around 1990, when the iron curtain finally fell and a crowd of Arnold's fans rushed to the US, Israel and Europe, Arnold did his best to help them find positions in various universities, and as usual he was very efficient. Once he proudly said that he had discovered a new technology of sealing envelopes that saved him a lot of time— he was sending out hundreds of recommendation letters (with no internet available).

Arnold was a Dadaist. He visibly enjoyed teasing the audience. For instance, Arnold concluded a discussion on an open problem in real algebraic geometry as follows: "Unfortunately, the algebraic geometers are unable to solve the real problems." As yet another expression of his Russian patriotism, he once asked a speaker at his seminar, "Why you are using the Roman letter F for this class of functions? Was it hard to find a Cyrillic letter?" The speaker was speechless. I will remember different facets of Arnold's personality: a kind and patient teacher, a sarcastic and funny showman and a ground-breaking scientist. Arnold used to say that any given problem which occupies one at the present moment should not be considered as *the only problem*. "You get stuck? You feel depressed? Stop thinking about this problem and go pick mushrooms!" I cannot stop thinking about Arnold.

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### CHAPTER 21

## Several Thoughts about Arnold

## A. Vershik

1. If a concept of "a leader" means anything in science, Vladimir Arnold should be called one. Tremendous natural talent, unbelievable convincing power, multi-faceted vision of subject matter, and perhaps, the most important trait of a leader – the ability to inspire everyone around him with his conjectures and problems – all of that he had in abundance.

From the generation of mathematicians born in the 30s and 40s, he was the only one who managed, perhaps to the greatest extent, to assume a challenging role of not only an excellent scientist, but an ideological leader of a large part of the international mathematical community.

The impact of his ideas was exceptionally powerful. Singularity theory, which he had brought from France in the mid 60s, in his writings and in his hands became the strongest principle of mathematical analysis when viewed in its broadest sense.

He was able to bring to a new level the theory of dynamical systems, from small denominators and Hamiltonian systems to ergodic theory and hydrodynamics, no matter what one might say now about the punctuality of his considerations.

The series of problems in real algebraic geometry was born thanks to his pioneering observations based on the works of the classical researchers and their followers, and the innovative nature of his observations was probably most significant after the formulation of the 16th Hilbert problem. One can easily continue further with this list.

Particularly important is his conceptual approach to mathematics. In this sense, the way I see it, he followed not so much his own teacher, A. N. Kolmogorov, but rather V. A. Rokhlin (who was also my teacher), his longtime friend.

In some other aspects, Arnold was more restrained. For example, he almost deliberately dismissed algebra as a philosophy of mathematics, and he liked to say that what he was studying was Analysis. It is obvious; however, that his knowledge of algebra was very substantial.

He did have this amazing ability to quickly grasp anything new, and even as a mature person he managed to learn a lot of new mathematics. Striking was the speed with which he could adapt new concepts and place them in his own contexts.

And yet the relationship he built with the area of algebra, especially later in life, was not right. Perhaps this fact, rather than methodological reasons, made him such a tough opponent of N. Bourbaki. And the passion, at times even exaggerated, with which he fought N. Bourbaki, alienated many mathematicians who had been willing and eager listeners to his concepts before. I feel that he certainly grossly

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exaggerated the role of Bourbaki and his influence on teaching mathematics in France and around the world. And I see no contradiction with the rest of his philosophy if algebra would find the place it deserves in his mathematical universe.

Arnold used to say, and not without grounds, that he (as well as his teacher) was not a "pure mathematician" but an experimentalist or a naturalist and compared his lists of singularities to herbaria and butterfly collections. These words were not merely a pose but his desire for mathematics to be seen, rather, as one of the experimental sciences.<sup>1</sup>

Overall, the results of Arnold's scientific and public activities, the school he created, and the caliber of his personality place him among the most prominent scientists of our time.

2. Some of his concepts were brilliant indeed. I remember his program "Local problems of analysis" in the early 70s, which began with indisputable, but still quite novel, and at the very least never before proclaimed principles. As an example, he brilliantly defeated the vast array of "about 1000 works" where their authors had just randomly studied (i.e., without taking into consideration either the general position or the co-dimension, etc.) various cases under such titles as "On a certain property of a certain case of a certain equation".<sup>2</sup>

This attack was a quite noticeable, if not an absolute, success. In my view, in this program he used an inherently fundamental principle in mathematics at large – finding a universal approach to a mosaic of specific problems in the context of a broader concept. This principle is realized by the singularity theory.

The following idea, which Arnold first expressed in the 60s, made quite an impression on me and, as far as I know, on many others: The Euler equations for the top and the Euler equation of motion for an ideal incompressible fluid are one and the same thing. I remember that in the late 60s he and I were discussing whether Euler himself had realized this. We will never know this, but the formulation of the dynamics on a general Lie group with a quadratic Hamiltonian belongs to Arnold, as does a classical problem of the motion of tops in higher dimensions.

About the same time he picked up and developed the classical idea of topological interpretation of deep facts about groups represented as fundamental groups. The series of his works and works of his followers on braid groups is a beautiful chapter of mathematics of the 1960s.

I remember how in 1970 or a bit earlier V.A. Rokhlin told me and also wrote somewhere that "Arnold had infected me with his enthusiasm". This referred to another Arnold concept, that of real algebraic geometry from the complex point of view, and it since had so many remarkable events to occur.

I am not going to continue with this list, as it is too long. Dynamics and ergodic theory, the topics close to my heart, are, of course, also on this list. Below I will talk about combinatorics, perhaps, his last area of interest.

While his authorship of a huge number of works is not much of a mystery, what is really surprising is the large number of his published problems, that he used to enjoy formulating on a regular basis. The traditions of Mathematical Olympiads the traditions he cherished and was open about—are clearly reflected in the very

<sup>&</sup>lt;sup>1</sup>With which I do not agree.

 $<sup>^2\</sup>mathrm{A}$  friend of mine eventually titled his work exactly this way as a protest against this gross generalization.

style of these problems. Almost all of his problems are simple in formulation and profound in content, there were no "empty problems".

His love for a concrete setting and for a selection of "the simplest non-trivial case" in the subject, as opposed to the "most general one", somewhat competed with, but did not contradict, his desire for universality. It would be curious to browse through his book of problems and to examine at least some part of them from this point of view. The number of concrete examples he was always ready to produce on each topic was really amazing. I only participated a few times in his seminar, and I think that regular members of Arnold's seminar should write about this and make an informal analysis of this flow of problems.

**3.** V. I. Arnold (from now on, Dima) has always occupied an important place in my thinking about mathematics and its people. We did not see each other very often and our conversations were not very lengthy, but in addition to personal encounters, I observed him while attending his presentations and talks at conferences, and, most importantly, read his papers (probably, almost all of them, but only skimmed some of them, of course).<sup>3</sup>

We were introduced to each other by V. A. Rokhlin during or after the All-Union Mathematical Congress in Leningrad in 1961. I remember Dima's then almost youthful appearance, as he gave a talk on small denominators at that congress.

First Rokhlin and then myself, would invite Dima for many years to give talks at the Mathematical Society and at seminars in Leningrad and probably more often than any other visitors. A few times (for instance, for Rokhlin's anniversaries in 1969 and 1979, as well as for other conferences), he would come and stay for several days. His talks gathered a huge audience, and the public was never disappointed.

He was one of the best mathematical speakers I have ever known. The ease with which he handled the material, his witty and lively language, and, most importantly, his rich-in-content and to-the-point manner of speaking were the qualities that characterized him as a speaker.

Over the years this all evolved into a certain image. It is important that these are my own impressions based on my own experience on different occasions; when writing something I never rely upon hearsay.

The difference in our ages is not significant, we both belong to the same generation. And so I have to start my story about Dima, an outstanding representative of our generation, with a couple of words about the generation itself.

According to a very precise expression of Joseph Brodsky (who was a little younger than us, but this is not essential), "...we came to the ground that had been trampled down". Although Brodsky mainly meant the literary and poetic "ground", this term may refer to science and to life in general, in terms of the succession of generations.

In mathematics, the succession was also disrupted, although not as drastically as in other fields of sciences. The reason for this was the colossal loss of talent

<sup>&</sup>lt;sup>3</sup>In my article for his 70th anniversary I wrote the following dedication, which later, in Dubna in 2008, he quoted back to me with noticeable pleasure:

DEDICATION. At the 70th anniversary of A.I. Raikin, one actor, referring to the celebrant, said something like this: "Some of us, and from time to time, go to some of the performances of some of our fellow actors. But ALL of us, with no exception, watched ALL of your performances."

I translate this statement into the mathematical context: "Some of us, mathematicians, sometimes read some of the work of some of our colleagues, but we ALL, with no exception, read ALL works of Arnold!"

during WWII and in various forms of Soviet purges (in mathematics, lower than in other sciences). The age gap between our generation (born in the 30s) and the generation born in 1900–1910 was filled very poorly. But that was not the main point.

In the second half of the 1950s, at the time when our generation was maturing and coming of age, we suddenly discovered an abyss which cracked open before us. The mendacity of life in a Soviet era, the suppression of original thought and the interest in the free world became apparent to us. We realized that the world was not what had appeared to us and what had been presented to us in our childhood and adolescence.

We also realized that the best representatives of the previous generation (who survived the war, the purges and the era of totalitarian terror and fear) were intimidated and terrified. Their personalities were deformed to a large degree, they were careful not to talk about something you were not supposed to talk about, and what they said was not necessarily what they actually thought. Maybe their devotion to science and to their mission, which they managed to pass on to us, was particularly strong because this replaced for them the intellectual pursuits which were prohibited in Soviet times.

The generation of the 50s had to choose their own paradigm in science and in life. Certainly this choice was very personal and different for everyone. I often quote a phrase that, according to Dima, his teacher A.N.<sup>4</sup> said in the mid 1950s: "There appeared a hope", in response to Dima's question why only then A.N. got interested in such classical problems as "small denominators" and others. It is hard to overestimate the importance of this revelation. For us, life became easier and there were more hopes, so we had to understand a lot more.

I believe that Dima, as well as myself, was greatly influenced by his friendship (mostly in the 1960s) with a remarkable mathematician V. A. Rokhlin, who was my and, in part, Dima's, teacher and not a typical representative of the previous generation.

His family was exiled, his father was executed during the years of terror. Rokhlin himself was a brilliant student in the Moscow State University, then a soldier of the militia, a prisoner of war in a German concentration camp, then a prisoner in a Soviet isolation camp, released after the appeal of A.N. Kolmogorov and L.S. Pontryagin to the KGB. His brief work at the Steklov Mathematical Institute and his expulsion from there during the "anti-cosmopolitanism"<sup>5</sup> campaign, teaching in provincial universities, and, finally, relatively regular academic life in Leningrad, until his heart attack in 1974 and his early retirement.

*Memories of Rokhlin* by Arnold is an outstanding brilliantly written essay (in the collection "V. A. Rokhlin: Selected Works", ed. A. Vershik, second edition, MCCME, Moscow, 2009; see the translation in the present book). In his essay one can clearly see Dima's caring and sympathy for Rokhlin and the latter's role in the formation of Dima's views on mathematics and life.<sup>6</sup>

It was Rokhlin who introduced us to each other in 1961. Before that, I saw Dima briefly at the Mathematical Congress in Moscow in 1956. Then in 1957 I heard the praise from Kolmogorov himself, when A.N. came to give a few lectures

<sup>&</sup>lt;sup>4</sup>Kolmogorov

<sup>&</sup>lt;sup>5</sup>A term used in the anti-semitic campaign in Russia after WW2 and until Stalin's death.

 $<sup>^{6}\</sup>mathrm{By}$  the way, Dima was one of the initiators of the first edition of this book in 1999.

in Leninigrad: "The strongest mathematician in his generation" said Kolmogorov talking about his series of papers on superposition of functions.

We rarely discussed social and political issues with Dima, mainly because it was clear that our views were the same. I remember how at the IHES we peace-fully talked about mathematics, occasionally trying to calm down Elya and Rita<sup>7</sup> disputing politics in another room.

He always adhered to the principle of supremacy of science, and he did not want to participate in something that could prevent him from following this principle. The story of signing the letter in defense of Esenin-Volpin in 1967 showed that the "era of fear" in our country was far from over. By signing it and then withdrawing his signature upon the request of I. G. Petrovskii, who was very much respected by Dima and who had done so much for him, was, in my opinion, a natural thing to do. All the same, the fact of signing the letter dignifies the signatories, who expressed, even if temporarily, their civil courage.

4. In his behavior and appearance one could often spot a certain youthful innocence and passion. It was never gone but softened over the years.

On the one hand, he would not accept fraud and what he considered immoral. Dima used to help and did help so many people, including his students, offended or unjustly forgotten. He was indomitable in doing it. Around 1990 Arnold resigned from the Academic Council of the Mechanical-Mathematical Faculty in protest against the shameful failure of Ph.D. thesis defense of one of Dima's graduate students orchestrated by a part of this Council.

But often he did not foresee (or chose not to see) unintended consequences of his actions or denunciations, often quite opposite to his original intentions. Sometimes an overly aggressive defense of unjustly offended may make things worse for them, while a particularly vicious attack on the big shots, on the contrary, might help them in advancing their careers.

Dima's actions and reactions would be understandable if they occurred in a normal social climate, but they were not always appropriate if the climate was deformed. Perhaps, some of his aggressiveness in pursuing his principles can simply be explained by his honesty. However, when I once told Dima that I regarded his desire to necessarily express his opinion as originating in his honesty, expressed in the words: "Whatever I think about that person, business, etc., I have to state it openly", he replied: "I do not accept your Freudian explanation".

His naivety and passion were particularly evident in the case with the journal "Functional Analysis and Applications" in 2004, which I will not discuss here at length, I will just mention the main points. He was understandably outraged by the behavior of some important people in the Academy of Sciences in relation to the excellent mathematical journal founded by I. M. Gelfand. Arnold was a brilliant editor, and he loved this work. The ambiguous behavior of the Academy of Sciences that did not allow the journal to function normally and authorized fleecing of the journal by certain people was resented by most of the editorial board and by me in particular. We actively fought for the independence of the journal from the actions of the mafiosi. However, the way of resisting the attacks on the journal which Dima had chosen (or which was suggested to him) was absolutely not viable and was not

<sup>&</sup>lt;sup>7</sup>Arnold's and Vershik's wives.

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shared by many similarly-minded colleagues or by me: to insist on it was useless. On the contrary, it became clear that it would lead to the opposite result.

The consequences were sad, and I still think that this story heavily influenced Dima's mood and worsened even further the state of affairs in the journal. I think that the new journal, which he later founded, was not likely to survive.<sup>8</sup> Notwith-standing all the passion of the polemic (I honestly wrote to him about my opinion), these events did not negatively affect our relationship, even temporarily, and it remained friendly.

5. Dima knew and read incredibly much, and his memory was exceptional. He mused a lot about the history of science, and he had his own conceptions of history. He could and should be considered an original (albeit non-professional) historian of science (now M. Gromov follows a similar path). Arnold's articles about Newton, Poincaré, Kolmogrov and others are read as reports on the latest developments.

I remember how I invited him to present a lecture at the newly opened Math-Mech in Peterhof. And I asked him to guess which mathematicians were placed on the front of the Math-Mech building. He managed to guess all but two or three and then heavily criticized most of the choices.

All in all, he had his favorite characters in the history of mathematics, and they almost never changed with time. Later, more than once, the aberrations would occur and they would be the subject of many disputes.

But if there may be different opinions about the events of the past, it is not the case when it comes to the recent events. Here is a small but typical example: Dima repeatedly wrote and said that he learned about my work on Young diagrams from Linnik. As, in fact, Linnik would not be able to tell Dima about the diagrams, as he had died before I started working on this topic. Actually, Linnik told him about my work on the statistics of permutations, which he presented for the "Doklady" (Proceedings of the USSR Academy of Sciences). However, I was never able to convince Dima, and the evidence of it was his article dedicated to me in the book published by the American Mathematical Society in 2006, although written in a somewhat humorous manner. Alas, he could not read my humorous response in the "Functional Analysis".

He had a great sense of humor, could be sarcastic, but he accepted jokes on himself. Once after a long conversation with him in Paris and after the consequent reading of one of his manifestos (that "Mathematics is the cheapest part of physics"), I wrote a parody on his text. Not only did he accept the parody, but he also mentioned it in his next article in "Physics-Uspekhi". I remember that I was writing the parody on the plane, and upon arrival to Russia, I had learned about his terrible bicycle accident. For some reason, I had no doubt that he would come out of it safely. And yet much more terrible was to learn about his totally unexpected death 10 years later!

Since Rita and I visited Paris often and for extended periods of time in the 90s, we often visited Dima and Elya. It is astounding how well he knew and how much he loved France. I never saw this in any of my French acquaintances. Dima carefully studied French history, and he knew Paris so well that it was simply exhausting to just take a walk with him in Paris, he could tell so much about every place in

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<sup>&</sup>lt;sup>8</sup> "Functional Analysis and Other Mathematics" was published by Springer, since 2006; as of January 1, 2013, it is no longer published by Springer. A new "Arnold Mathematical Journal", based in the Stony Brook University, was recently established.

the Latin Quarter and elsewhere. I forgot at which French mathematician's home Dima studied the history of Paris; apparently, it was Cerf's.

The story of the epigraph to "Eugene Onegin" is now well known; this is also a story about France. He published an article in "Proceedings of the Russian Academy of Sciences: Philological series" about a quote from Choderlos de Laclos (of the widely acclaimed novel "Dangerous Liaisons"), which was the source of Pushkin's epigraph, and which was not noticed by the most meticulous Pushkinists. He asked me to find out, in the Pushkin House in Leningrad, what had been known on this subject. All I could find out was that nothing had been known.

Only later Larissa Volpert, a well-known Pushkinist and a student of Yu. M. Lotman in Tartu (and also a former USSR chess champion) wrote to Arnold that the phrase of Arnold's (it was his subtle move) that "as a mathematician, he trusts more common sense than the proofs, and therefore he thinks that Pushkin used this very phrase from Choderlos de Laclos, although he does not have a direct proof" was too modest and, in fact, he gave a perfect proof of this, and therefore should deservedly be regarded as the solver of this old problem in Pushkin studies.

6. I had an impression that Dima was interested in my results. In the beginning, this happened apparently because of his conversations with Rokhlin (and later with Linnik and maybe Gelfand, I do not know for sure; there is no one to ask anymore.)

Later, we saw each other several times a year and talked for a long time. And each such conversation invariably lead me to new insights and associations. I could name very few people with whom conversations have been as fruitful for me. Furthermore, my topics of study were often quite far from his. I could give some specific examples, but it hardly makes sense to do it here.

When writing many of my papers, I imagined someone as the "main" future reader, and very often for such a reader I automatically selected Arnold. Our tastes were not always the same, and I knew which subjects would not be met enthusiastically by him.

But there were many problems that we both liked. In particular, this was applicable to combinatorial and asymptotic problems. Among those were some "limit shape problems", which I actively popularized since the 70s and I always enjoyed his overall support.

I would like to describe such a case. Dima read an article in the *Notices* of the American Mathematical Society and found there some questions which I had discussed with him long before that. He wrote to me that for some reason the authors had not quoted me, and if I did not mind, he would write a letter to the editor about it. He wrote that it was not the only case when the works of Russian authors were not cited and what was worse, sometimes it was even intentionally ignored.

I knew of a case like this with his former student whose work Dima greatly valued. I told Dima that if he were to write about that case, and if he were to mention my article at the same time, I would not mind. Although, in my opinion, it was not necessary, since I knew the authors and did not see any ill intent behind their lack of citation.

After a while, I received a letter from the editor of the *Notices* of the American Mathematical Society asking to read the enclosed letter from V. I. Arnold to the editor. That letter was devoted to my case alone and contained nothing about

other cases of insufficient citation. The article was called "Vershik Work Needs Acknowledgement" and it was incredibly harsh.

I had to write my own letter to the editor, in which I tried to hold back Dima's righteous anger and justify everything by entirely harmless reasons. This is yet another example of what I wrote about earlier – Arnold's uncompromising commitment to the ethics in science would lead him to overly stringent assessments even in cases where it was not necessary.

Now I would like to discuss the last series of Arnold's works, those of numbertheoretic and combinatorial nature.

A radical change in scientific topics ("change of the code", as linguists would put it) is not very typical for mathematicians of an advanced age, as people do not want to change anything, including themselves. But we know many examples of such change among very prominent mathematicians. Perhaps it is easier for them to do so, since their horizons are broad, they possess great experience and mastery of the techniques, while their ideas have not yet been exhausted.

In my opinion this happened with Dima late in the last century (20th) or at the beginning of this one. He clearly made his choice in favor of discrete mathematics, and in particular, the classical number theory, combinatorics, and geometry. Of course, his traditional themes stayed with him. I counted about 20 papers on this new topic. We never discussed the reasons for this shift, but it increased the number of our common interests.

With his characteristic fresh vision and his ability to see "open spaces", he was extremely successful in finding new problem settings in the seemingly beaten topics, for example, in the arithmetics of quadratic forms, in variations on Fermat's little theorem and Euler's function, statistical issues related to the Galois fields, substitutions, etc.

One can find answers to some of his questions in the old literature, but the main direction of his questions was new and there is no doubt that the subject will be picked up and developed by others.

He protected his untainted mindset. Here is a little detail, quite typical for him. I told him about a not very well-known work on the topic of the Euler function, and he immediately replied that he would not read it because he did not want to "wander off" away from his own line of thinking.

He computed tons of examples. I can hardly imagine how he did it (seemingly without a computer). I am sure that if he were given more time, a new theory would have arisen from his recent works, full of exceptionally rich experimental material and various insights. And it will happen anyway.

The last time we had lengthy conversation with him was at the summer school in Dubna in 2008. He talked a lot about his ancestors, then we talked about permutations (see above).

I remember his strange, with a touch of black humor, and totally unprovoked speech at the final meeting of the school participants. He recalled the story told by a local resident, complaining that catfish in the Volga river almost became extinct, because there were too few drowned persons, and that something had to be done about it.

By the way, there are plenty of legends about his swimming, while the story about his attempt to cross the Golden Gate strait under the Golden Gate Bridge in San Francisco is confirmed by many. In general, good luck helped him in his many challenging pursuits.

Later we accidentally bumped into each other at the Steklov Institute for a few minutes at the end of 2009, and he started telling me about his new paper, which would probably interest me....

### CHAPTER 22

# Vladimir Igorevich Arnold: A View from the Rear Bench

## Sergei Yakovenko

Just ten days before reaching his 73th birthday our teacher, Vladimir Igorevich Arnold, or VIA, as we used to abbreviate his name between ourselves in correspondence, died in Paris from foudroyant peritonitis. The shock and feeling of evisceration was so strong that for several days those of us who were scattered around the globe were bombarded by phone calls and emails from those who happened to be in Paris or in Moscow. What? How could that happen? In rather good physical shape? Seemingly having fully recovered from the terrible bike accident that left him incapacitated for so long... The consciousness rejected the impossible. Yet in hours the news became a sad reality: VIA was indeed no more.

I felt a personal loss, though I could not pretend to be one of his intimate friends. I was not even his student in any sense of the word. I felt a spiritual loss: never again I would be able to learn from him anything beautiful and inspiring, curious or instructive, funny or mysterious. I felt a professional loss: the central pillar, around which so many events occurred and so many old friends and colleagues orbited, had fallen. The mathematical world as I knew would never be the same without VIA, without his encyclopedic knowledge and immense intuition, without his special charm.

The following is an edited version of the text which I wrote ten days later, on the birthday of Vladimir Igorevich.

Today, June 12, 2010, Vladimir Igorevich Arnold should have turned 73. Today, as many times on this day in the past years, I should have been writing a short informal "Happy Birthday" email that never was acknowledged; VIA was not known for wasting time on polite conversations, yet I knew he would have read it. If I were in Paris, I would call and drop by, as all of his students would do. Instead, today we are waiting for our Teacher to be laid to rest: The funeral in Moscow is scheduled for June 15.

The mere thought of Arnold being ill contradicts his personality as we remember him. All his life VIA projected strength, confidence, perfection, beauty, elegance. Physical, spiritual, mathematical, human. He was all motion, all burst. I remember him teaching the second-year class on Ordinary Differential Equations in the huge 16-24 hall of the Moscow University main tower building, during the 1977/8 academic year: VIA was then at the "Fields age", considered the prime

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age for mathematicians. At the beginning of each class, with the soundbite of the bell, he rushed in, his trademark briefcase in-hand, he started the first phrase of his lecture while still 3-4 meters from the blackboard. In a fraction of second his briefcase was thrown on the table, a piece of chalk appeared in his hand, and when the first phrase was completed, we already saw a carefully drawn picture on the blackboard and a few formulas written in his calligraphic handwriting near it. His lectures were practically impossible to write down, as impossible it is to record by a cell-phone a superb performance of your favorite music. Besides, it was very difficult to record the insight: As Arnold speaks, draws, writes, you suddenly see how different things are getting connected and the whole picture transpires through the initial fog. Fortunately, at that time his famous textbooks were already published; in these books he succeeded in doing the impossible and putting these revelations on paper.

In fact, it was probably my first hands-on experience with a working mathematician of such caliber, which forever left an imprint on my world view. Later encounters with VIA's peers (there was a unique constellation of great minds at this point in space-time) fascinated me but VIA always remained singular, even against such background. One should note, however, that his style of presentation of undergraduate subject traditionally considered as technical and boring, peppered with huge formulas and heavy computations, was not equally good for everybody. The feeling of crystal clarity that one got from VIA's exposition, was no substitute to the ability of restoring all missing "technical" details, and simplicity might well turn misleading. Many years later VIA mocked the "Bourbakist" way of spelling out mathematical statements in his famous quip, saying that the fact stated by Poincaré in the simple sentence "Pierre had washed his hands" in the formal Bourbakist rendering would sound like a description of the transition of Pierre from the set of dirty-handed to that of clean-handed at some moment in the past.<sup>1</sup>

Poignant and subtle, this quip does not obliterate the need for students to be able to translate "humanly understandable" phraseology into precise statements

<sup>&</sup>lt;sup>1</sup>To the best of my memory, the first time this quip appeared was a footnote in the Russian (1986) edition of the survey paper *Catastrophe theory* (Russian), Current problems in mathematics. Fundamental directions, Vol. 5, 219–277, (Итоги Науки и Техники. Современные проблемы математики. Фундаментальные направления, ВИНИТИ), 1986. In a footnote on p. 233 VIA writes:

К сожалетию, бесхитростные тексты Пуанкаре трудны для математиков, воспитанных на теории множеств. Пуанкаре сказал бы "Петя вымыл руки" там, где современный математик напишет просто: "Существует  $t_1 < 0$  такое, что образ точки  $t_1$  при естественном отображении  $t \mapsto \Pi \text{етя}(t)$  принадлежит множеству грязноруких и такое  $t_2 \in (t_1, 0]$ , что Петя $(t_2)$  принадлежит дополннению вышеуказанного множества".

Unfortunately, unsophisticated texts of Poincaré are thorny for mathematicians raised upon set theory. Poincaré would have said "Pierre has washed his hands" where a contemporary mathematician would simply write instead "There exists  $t_1 < 0$  such that the image of  $t_1$  by the natural map  $t \mapsto \text{Pierre}(t)$  belongs to the set of dirty-handed, and  $t_2 \in (t_1, 0]$  such that Pierre $(t_2)$  belongs to the complement of the above set.

The Russian language has only one simple past tense and possessive pronouns are often omitted, thus the Russian phrase is more concise than its accurate English translation, making the contrast even sharper. But ironically exactly because of these grammar features the ridiculous "mathematical rendition" in fact adds to the initial Russian phrase the precision it missed.

equipped with all proper quantifiers, this is a task that not all were up to. Nevertheless, this could be considered as a part of VIA's teaching legacy: first the main and difficult things should be explained in simple terms, and only later the necessary technical details and subtleties should be addressed. Unfortunately, this approach goes against the mainstream of the current tradition of writing mathematical texts, where lemmas and preparatory technical stuff precedes the instances where they are required, and so lack motivation. VIA himself compared this "formal" style to cryptic biblical parables, which had to be expounded to disciples in seclusive meetings. Arnold's books are a unique example of mathematical literature where this traditional order is reversed. While keeping the trademark freestyle of presentation of the main issues, always accompanied by numerous drawings, he resorted to the fine print and "exercises for the reader" to deal with technical details. At one such instance he coined the phrase "It is easier to prove this statement singlehandedly than read a written proof" which indicates the level of detail, below which no lecturer should descend.

Later I started attending the famous Arnold's Seminar (with a capital "S"). It will certainly be described by many people who were both closer to VIA and have sharper pens, yet this phenomenon was so unique that no detail should fall into oblivion. The Seminar was scheduled so that people could attend it after the standard office hours, as many (probably, the majority) of the participants were not officially affiliated with the Moscow University. Arnold rushed in the room and took his permanent seat in the middle of the front row next to the blackboard. The seminar did not begin until VIA got from his briefcase a bunch of recent preprints and reprints and handed them out to the elder participants of the Seminar: "Vitya (to Vassiliev)! The author claims that he proved so-and-so, but I could not find any appearance of the contact structure in his computations. This simply cannot happen, we both know that it should be somewhere there!" (And in a couple of weeks Vitya would indeed return the manuscript to VIA with margins peppered by remarks explaining where the "missing" structure was concealed and showing how its explicit use may simplify the proof...). This "home assignment" could take quite a bit of time, yet at some moment Arnold opened his "school-like" copybook, entered the speaker's name and the title of the talk, and the Seminar began.

The choice of speakers and the titles, apparently, reflected the current interests of VIA himself; for me (at that time a 4th year undergraduate student) neither was telling, yet this was largely irrelevant since each Seminar was a one-man performance. A typical scenario was as follows. For the first 15-20 minutes the speaker talked "practically uninterrupted"—that's to say, no more than once in 1-2 minutes—when VIA asked questions seemingly technical or even bordering on chicanery. Gradually the exposition turned into an agitated conversation between the speaker and Arnold; this ping-pong match could last long enough for the rest of the audience to get completely lost. Then a culmination occurred: VIA jumped from his place to the blackboard and shouted "No, this is impossible to understand your way. The right picture should be as follows..." And then he explained in 5 minutes both the origin of the initial problem addressed by the speaker, its links and connections to other problems (and at that moment it became clear why Arnold invited this particular speaker to talk on this particular subject), and what the main result is proving (or disproving, or corroborating). In a few moments Arnold would explain how he would try to prove this result, and often the speaker, changing colors from red to white, would nod in acquiescence... At such moments Arnold was literally shining from pleasure and suddenly would chuckle with his inimitable laughter, as a child who "just did it!".

This might well look like a derision of the speaker, yet it was not. The "retribution" could come instantly, when Arnold would start fantasizing about possible ramifications, generalizations and further developments that may come out of the result just learned. The speaker, regaining his balance by that time, could cut short these fantasies: "This corollary is indeed true, but the proof is by no means as simple as you think, VIA, for such and such reasons. And the generalization you suggest is simply wrong: just two weeks ago I constructed a counterexample" (of which the speaker did not plan to talk at all). At such moments VIA's excitement rose to a maximum: he would jump in again and start explaining why the speaker was wrong and what underwater rocks and unexpected phenomena manifest themselves in "so innocent a problem". It was these moments which justified attending the Seminar for two hard hours (sometimes longer). Even the youngest participants (like me) left the room exhausted yet with some clear mathematical message to take home.

This childish chuckle, instantly transforming the face of Arnold, in my eyes, reflected some part of his mathematical personality. He was very much like a prodigy child in Aladdin's treasure vault: enjoying mathematical reality in all its brilliance. Mathematical anecdotes mention great mathematicians whom examples only distracted from developing general theories. Arnold was the opposite: examples were the alpha and omega of his approach. Of course, it was impossible to look inside this beautiful mind, yet I have a feeling that he knew mathematical objects (small dimensional varieties, Lie groups, fundamental dynamical systems, ...) the way a zoologist knows and loves his bees, beasts, birds, etc. This was based on his tremendous erudition and, in turn, allowed him to see connections between seemingly very distant things. Probably, about any natural number less than one hundred, he remembered all mathematical results and constructions in which this number occurred.

One of the strongest impressions from the Seminar was the feeling of unity of Mathematics that literally radiated from VIA and the more senior participants. True, the similar feeling was also present on other seminars which I occasionally attended, but there it often was in the form of expanding horizons and relations with the branches not yet familiar to undergraduates. In Arnold's world, geometry of planar quadrics was connected with diophantine equations, Jordan form of matrices with the operator of derivative, functions of complex variable with probability. I remember that at a certain moment the difference between topological connectedness and arc connectedness appeared in the discussion of holomorphic dynamics: until then I was absolutely certain that examples illustrating the difference between these notions were specifically designed for exams in Calculus.

Discussing mathematics with Arnold was a unique experience. VIA was astoundingly sharp and quick-minded. I remember discussing with him a question indirectly related to one of his "Problems for the Seminar", on which I worked for quite some time. The problem was difficult (its complete solution took a further 25 years), and I tried to explain to VIA some partial results I had. The feeling was as if I was talking to a person who knows the answers to all questions; he seemed to be able to continue *my story* from any point, and in exactly the same way I did. It was even embarrassing: all my efforts, weeks of banging my head against the wall could have been so easily spared, if only VIA would himself have decided to attack the problem! Only much later did I realize that Arnold instantly identified the key ideas from the very first phrases and then, with all his huge experience and intuition, he could indeed easily jump from hilltop to hilltop where I had to walk a difficult terrain in fog.

The impact of VIA on the generation of Moscow mathematicians, who are now approximately between 40 and 65, is enormous. His direct students exhibit a quasi-religious feeling towards him: no adjective (alone or in a combination) suffices to convey the impression he left. Lightning-fast thinking, sharp reaction, incredible intuition, ...—all attributes of a superhuman; he himself contributed to this image, stressing his physical skills like swimming, hiking, skiing, which also were well beyond "ordinary" capacity. Yet the child inside him was pretty much human: like many children, he loved to tease people, and many who didn't know him closely were understandably offended. For his students he often did (without saying) things that prove a deep personal involvement he felt towards them.

But even for those who "simply" happened to witness Arnold the Mathematician in action and enjoy the beauty and elegance of his view of the subject, the impact was catastrophic in the bifurcational sense of the word. At the time when I decided about the field of mathematical specialization, because of the unique atmosphere of the Moscow University in those days, the choice was tantalizing. Algebra and algebraic geometry with Yurii Ivanovich Manin, geometry or mathematical physics with Sergei Petrovich Novikov, probability and dynamical systems with Yakov Grigorievich Sinai, complex analysis with Anatoly Georgievich Vitushkin, Representations theory with Alexander Alexandrovich Kirillov-Sr., all in their prime, all bursting with energy, all doing beautiful mathematics. And of course, there was the proverbial figure of Israel Moiseevich Gelfand!

Instead I chose the subject which "before Arnold" many considered as boring, dull and non-inspirational; "A theorem on one property of one solution of one differential equation", quoting another of VIA's quips on "bad" Differential Equations. Since then I had not a single regret for falling in-love with such a wonderful part of Mathematics: its centrality and most diverse connections with almost all other areas is what I learned to enjoy, featuring a clear imprint of VIA's taste. My professional career was practically predetermined by the fact that it began in the epoch of Vladimir Igorevich Arnold illuminating my entrance to the universe of Mathematics.

The above memories (ranging from 1976 to the late 80s) describe what I would consider to be the absolute zenith of Arnold as mathematician, leader of a school and supreme commander of elite troops ready to follow him in attack on any mathematical fortress, fearless and ambitious. The subsequent changes in the country and the world obviously changed also many things in VIA's life. As I already mentioned, I did not belong to his most narrow circle of disciples and coworkers, myself having left Moscow in 1991, so necessarily the memories become stroboscopic and much more relying on hearsay rather than on my own first-hand experience.

VIA resisted the temptation to leave the USSR/Russia despite a desperate economic situation which rendered academic salaries practically nil. For some time a partial solution for many was to look for visiting positions in the Western universities, work for several months a year abroad leaving families behind, and convert the accumulated salary into the source of modest subsistence, playing on the crazy exchange rates of the rouble at that time. However, such dynamic equilibrium was clearly unstable: some mathematicians from VIA's Seminar accepted permanent positions abroad, some gave up altogether. Quite a few exceptional people managed to arrange "permanent part-time positions" allowing them to spend one of the two semesters abroad, the other at home, in Moscow. Arnold resisted longer than many, but in 1993 he accepted such an offer from CEREMADE, a French CNRS unit at Université de Paris-Dauphine specializing in applied (sic!) mathematics. This has inevitably impacted the Moscow Seminar, although VIA himself made all efforts to ensure the continuity; e.g., he tried to re-create his Seminar in the spring semester to take place at exactly the same week day and time (Tuesdays, 16:20 till 18:00) in the École Normale Superieure.

However, the environment did matter, and the Paris Seminar did not rise to the place its Moscow prototype occupied in the mathematical world. The composition was different, the Parisian mathematical community did not reveal such acute interest in what was going on there, who knows what else went wrong... VIA, having a very dominant and assertive personality, felt the difference in the atmosphere and understandably grew more and more bitter about "the Western style" of doing mathematics. His criticism (very often more than well deserved) took forms which, apparently, many of his French colleagues had deemed offensive: for instance, he would never miss an opportunity to stress the fact that a certain problem, on which a respectable (and strong) French professor worked with only partial success, was "completely solved" by some young Moscow prodigy undergraduate. Both completeness of solution and the role Arnold himself could play in reaching it was conveniently stretched to produce infuriating effects. Another sad (in my view) crusade VIA launched about that time was against what he called "Bourbakism" and "pure mathematics". While the opposition to the formal axiomatic exposition of mathematical results was always characteristic of Arnold's trademark style (as I already mentioned), he gradually went overboard with ridiculing what he considered formalism and unnecessary abstractions. The mere names of Bourbaki and Hardy became anathema for Arnold, and the logical construction of solid foundations for future building (the trademark Bourbaki style) became the subject of ridicule more and more frequently. He went as far as to claim on several occasions that "there is no Mathematics, only a branch of Physics". Clearly, he did not mean these things literally, being himself a most subtle mathematician, but the chorus of jingoists of all stripes cheered these provocative statements, much to the chagrin of the genuine mathematical community.

After his tragic biking accident, VIA slipped more in this direction. Citing several rather anecdotic cases, he extended his (again, often perfectly legitimate and profound) criticism of the French high school and undergraduate education system to a blanket condemnation of the whole enterprise. Very often this was juxtaposed in VIA's diatribes to the (idealized at times) Soviet system of education; these writings were cheered by many, beyond all proportion. Eventually this side of his multifaceted activity took a very prominent place in the public perception of VIA: "Russian most-cited mathematician castigates the formal Western education system which perpetrates shallowness, and praises the Russian way of getting to the heart of things!" VIA was made an icon of anti-Western rhetoric, completely ignoring the fact that he in fact was one of ecumenical figures in the modern science, universally recognized and respected by physicists, astronomers, topologists, algebraists, analysts of various traditions of all countries...It would be very sad if the monochromatic image of an iconoclast would be perpetuated, shading the uniqueness of VIA in his ability to get to the core of things in all their diversity. He himself could learn and teach this way, only a few could follow in his footsteps.

According to Arnold, the last words of Isaac Barrow, the adviser of Isaac Newton, were "Oh Lord! Soon I will know solutions to all differential equations". Today we know how naïve this wish was, yet more important things stay forever. Vladimir Igorevich, I wish you to know that the seeds you planted all your life will yield hundredfold harvests. Any other outcome would be unfair, ugly, and hence, simply, wrong, as the truth is always beautiful...

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Vladimir Arnold, an eminent mathematician of our time, is known both for his mathematical results, which are many and prominent, and for his strong opinions, often expressed in an uncompromising and provoking manner. His dictum that "Mathematics is a part of physics where experiments are cheap" is well known.

This book consists of two parts: selected articles by and an interview with Vladimir Arnold, and a collection of articles about him written by his friends, colleagues, and students. The book is generously illustrated by a large collection of photographs, some never before published. The book presents many a facet of this extraordinary mathematician and man, from his mathematical discoveries to his daredevil outdoor adventures.



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