Russian Math. Surveys **65**:4 795–802
Uspekhi Mat. Nauk **65**:4 201–207 DOI 10.1070/RM2010v065n04ABEH004700

MATHEMATICAL LIFE

Qualitative theory of dynamical systems and foliations in the Moscow school of mathematics in the first half of the 1960s

S. P. Novikov

(Dedicated to the memory of V. I. Arnold)

I would like to tell you about my view of this area, a view formed to a significant degree due to interaction with my recently deceased friend Vladimir Igorevich ('Dima') Arnold. In the middle of 1961, after the Leningrad All-Union Congress of Mathematicians, Arnold was already the risen star no. 1 of the Kolmogorov school in the Faculty of Mechanics and Mathematics (Mekhmat) at Moscow State University (MSU): behind him were both one of Hilbert's problems (superpositions of continuous functions) and perturbations of completely integrable Hamiltonian systems (KAM theory). These areas of activity of Kolmogorov and his school were far from me. But Arnold began actively looking around in mathematics, searching for new areas. We were childhood friends: already our parents had been friends many years back. He was one year older than I, and after he enrolled in Mekhmat in 1954, we did not keep in contact with each other. Our association at a new level began in the summer of 1961, when I had begun to stand on my own legs in modern algebraic topology, and was successfully entering the then new area of the differential topology and geometry of manifolds. I was greatly helped in firmly finding my feet in this area by J. Milnor and F. Hirzebruch, stars from the West who came to Leningrad for the Congress in June of 1961. As for myself, I began to study more deeply analysis, the qualitative theory of dynamical systems, and partial differential equations. My friends helped me in this. In the qualitative theory it was D. V. Anosov. He and I also studied topology together — earlier, in the seminar of M. M. Postnikov and in the courses taught by A. S. Schwarz.

At the end of the summer and beginning of the autumn of 1961, S. Smale came to Moscow, straight from the conference in Kiev where he had presented his celebrated paper that started a new era in the qualitative topological theory of multidimensional dynamical systems (see Anosov's book [1]). On arriving at the Steklov Institute, Smale said that he wanted to meet Novikov. They looked for my father, who was at the 'dacha' (they did not find him). But it turned out that Smale wanted to meet me. This helped me: at that time my official supervisor Postnikov was strongly resentful towards me — I did not want to follow any of his advice. His remarkable papers were already 10 years past, and I regarded him as 'exhausted'. In the West there is in general a poor understanding of the importance of the official

AMS 2010 *Mathematics Subject Classification.* Primary 01A65.

supervisor for a beginning mathematician, student, or Ph.D. student in the USSR of that time — both during the undergraduate years and during graduate studies. The supervisor 'owns' you; the authorities act in relation to you as he says, with the exception of cases of special repute. He was 'putting me in my place'. In the eyes of the authorities, Smale's request put me in the number of young researchers of this special repute, the 'untouchables'.

We (Anosov, Arnold, and I, and possibly Sinai) met with Smale on the 2nd floor of the Steklov Institute building (now the Computer Centre), in a room of the Department of Differential Equations. This was precisely the first scientific activity in which I was involved together with Arnold. I got what I needed from the meeting with Smale: missing information 'from the front lines' and a clear realization that my command of the new smooth topology was at the cutting edge. This was very important for me, being isolated in the USSR, where the modern level of this area had already been lost for a number of years, while in the West there had been tremendous achievements. In a month or two after Smale's departure I produced a paper on the classification of multidimensional ($n \geqslant 5$) simply connected manifolds ([2], paper no. 4). This was my future D.Sc. dissertation ('Habilitation') ([2], paper no. 10). As a Ph.D. dissertation I presented in two years a nice separate digest of it devoted to homotopy groups of the diffeomorphism group of multidimensional spheres ([2], papers no. 7, 20), and Arnold became my official opponent so that I would teach him differential topology.

In our discussion in 1961 Smale stated a remarkable conjecture about the structural stability of certain Hamiltonian systems (geodesic flows on manifolds of negative curvature) and of hyperbolic automorphisms of tori. He said that he had a proof in a special case, and we asked him about it. He presented the whole scheme of his proof, but Arnold's sharp eye found a gap in his arguments. Smale left, saying that he would think about how to fill this gap. After several months Arnold (together with Sinai) had an idea about filling the gap. They published a note [3] about this in *Doklady Akad. Nauk SSSR* in 1962, citing Smale's initiative (by my standards, it could have been cited better). Soon Anosov found a gap in their filling of Smale's gap, and their paper disappeared from the scene [4]. Dima Anosov gave a complete solution of the problem, proving Smale's conjecture. His beautiful proof shows clearly the role of foliations. Since that moment these foliations became a part of the modern qualitative theory of dynamical systems (they are called Anosov foliations). Let me make two remarks about this instructive story. First, in 1967 Smale told me in the USA that on returning he had filled the gap but did not publish his proof because Anosov's method had appeared, and it completely covered the entire circle of these problems. However, later I did not find any confirmations of the existence of a proof alternative to Anosov's in the above-mentioned special case, after asking experts at the beginning of the present century. Second, about that mistake of Arnold and Sinai: it was a mistake involving a technique in the theory of functions of a real variable (TFRV). I am personally far from this technique, but they both came from a first-class school of TFRV — the school of Kolmogorov, who had a remarkable feel for a TFRV-type proof. One learns from one's mistakes, although not everyone does. Subsequently Yasha Sinai, in developing the theory of non-smooth ('billiard-type') systems, which are similar to Anosov systems, as well as in other areas demonstrated a high-class mastery of this technique. If he made mistakes, then he found them himself or with the help of his students, and rather quickly. And he had the courage to admit his mistakes publicly. Dima Arnold, on the contrary, soon started moving to areas close to topology, going away from TFRV. Unfortunately, the experience did not teach him to be more careful, to go back and (together with his students) recheck again and again his papers, verifying the completeness of TFRV-type proofs. Many years later, other people distant from him found gaps in his papers from the beginning of the 1960s.

Arnold liked topological ideas: they were better suited to his remarkable talents than TFRV; here the understanding of the essence of a fact is closer to a rigorous proof.

I started attending Arnold's seminar. In that period he began actively popularizing the theory of foliations. Apparently, different factors played roles here: the role of foliations in the study of Anosov-type systems, the papers of Reeb and Haefliger on the qualitative theory of foliations of codimension 1, and finally, the papers of Petrovskii and Landis that were conceptually close to foliations and concerned the number of limit cycles of differential equations on the plane with polynomial right-hand sides (one of Hilbert's problems) [5]. We shall return to the last subject later. Probably Petrovskii, to whom Arnold had become close, asked him to look into this. For one reason or another, I sat in Arnold's seminar, listening but not understanding these ideas very well until the autumn of 1963. At that time something happened inside me, and I began thinking about the qualitative theory of foliations of codimension 1. I was becoming more and more interested in this. Finally, I achieved some noteworthy results ([2], papers no. 11, 12, 13, 23). In particular, my theorem on a closed leaf was a success. This was not conjectured by Arnold, but his activity undoubtedly stimulated me to work on it. One of my theorems on manifolds admitting Anosov automorphisms contained a gap in the proof (found by Anosov). My student A. Brakhman helped me fill this gap. Only after the autumn of 1963, having acquired experience working with foliations, did I turn to the paper of Petrovskii and Landis on limit cycles. I studied this paper together with Anosov. Apparently, this began only around the end of 1963 or the first of 1964. Before that I was not at all ready scientifically: I did not understand the geometry and topology of foliations. How long did this last? When did I first consider the possibility of meeting with Landis and asking him questions? How many times did I meet with him? It is difficult to reconstruct this definitively. Probably it all took place in 1964–65. At my lectures Arnold became acquainted with the idea of transversality and how to work with it. This was developed by Whitney, Pontryagin, and Thom. Differential topologists knew this topic, but at the beginning of the 1960s no one else did. My lectures took place in the small room of P. S. Aleksandrov, with the note "I kindly request that you not smoke here even in my absence". For people like me this was a significant restriction, but could I do? There were two of my students (one of them was V. Golo), along with Arnold. He was struck by this idea, he liked it awfully much. It seems that it was the winter of 1962/63. He began studying this, and then with the help of Petrovskii he went to Paris for almost a year in 1964/65, where he learned these ideas more deeply from Thom. His later papers on singularities are widely known. But to me he left the legacy of analyzing the Petrovskii–Landis paper — I was then ready for this.

I think it was Arnold who left me the text of the manuscript written by Landis, but I do not remember for sure. I started working on it together with Anosov.

However, this story deserves a more consistent telling. It had great public resonance in Moscow mathematics.

Petrovskii was a remarkable scientist, and without a doubt the best mathematician-administrator of his time. In the 1950s there were stories among students that when his name was brought to Stalin in 1951 as a candidate for the rector of the future 'Temple of Science' — the new MSU — Stalin was told that Petrovskii was good in all respects but not a Party member. Stalin said with his characteristic Georgian accent: "It does not matter, we shall re-educate him if it becomes necessary," and signed the appointment. Much later I learned that the candidates were nominated by Beriya. Petrovskii was known, in particular, for combining the methods of algebraic geometry, analysis, and differential equations. He seems to have correctly understood back in the 1940s that introducing the notion of a limit cycle into a complex domain gives rise to what we now call the topology of foliations. He decided to apply this to Hilbert's problem and invited for this work the very good mathematician Landis (whom his teacher Kronrod enthusiastically publicized). Apparently, Petrovskii was distracted after 1951 by the titanic administrative activities that were so important for all of us: he resurrected Mekhmat from the ruins. It seemed to Landis that he could successfully complete Petrovskii's ideas — and they wrote the paper [6] published in 1957. Kronrod went around Moscow referring to "the last result of Petrovskii, obtained by Landis." A teacher of, in particular, Vitushkin and Landis, he was aggressive, talented, and unwise. This last feature facilitated the failure of some of his extremely valuable initiatives in computational matters.

Around 1960 the idea arose to award a Lenin Prize to Petrovskii and Landis for this paper. However, Pontryagin said at a committee meeting that the paper had not been verified. I heard this from my father at the time. In general, Pontryagin and Petrovskii were always quite unfriendly, but here Pontryagin was in the right. In any case, Arnold, as it seems to me, began studying these papers not without a request from Petrovskii's side at the beginning of the 1960s, as I already mentioned above. Then after the end of 1963 he passed the baton on to me (and I invited Anosov). For about two years we periodically returned to this. However, only I met with Landis. Time after time he prepared new texts. I do not remember any other participants in this.

This was a long process. Unfortunately, most mathematicians, including my friend Anosov, avoid situations where a public confrontation arises (Arnold was among the few who were not afraid of this, something I admired in him). Looking back, I remember that I had the impression that Landis was being helped by someone, but I did not then understand by whom exactly. It is possible that this was a plain and simple concealment of a mistake. It became clear that the king had no clothes: in this paper there were no arguments for an upper estimate of the number of limit cycles. There was nothing, apart from ideas in the theory of foliations. I began talking about this fairly openly. Landis was a very good mathematician and a nice person. He was my examiner in mathematics at the entrance examination to Mekhmat in 1955. However, there was nothing to be done: he had simply lost his way in an area that was far from his interests. He is a strong specialist in subtle questions of the theory of differential equations with non-smooth coefficients and in other questions of TFRV, but here it was something quite different. After some time he informed Petrovskii that the paper was erroneous. There was then a rumour that he told Petrovskii that the mistake was found by him together with his student Il'yashenko. However, Petrovskii learned (possibly, from Gelfand) that I (more precisely, Anosov and I) had arrived at this conclusion. Gelfand was loyal to Petrovskii. He very nervously asked me sometime in 1965–66, and I told him clearly about the situation that had emerged. I did not give any talks about this at his seminar. Beginning in 1963 Gelfand and I formed a growing deep scientific mutual understanding. He realized that there was nothing that could be done — and most probably told Petrovskii.

Petrovskii invited me for a discussion about the matter and tried to persuade me to take up this problem and carry it to completion. I told him that I did not see any clues for getting an upper estimate of the number of cycles, and that 50 years would pass before the problem will be solved. Petrovskii always supported me. He was a great man.

By the way, it will soon be 50 years already, but no progress is in sight. About 10–15 years later Arnold proposed an interesting idea for estimating the number of cycles in a neighbourhood of completely integrable systems, thinking that it is probably here that their number is maximal. His students Petrov and Varchenko carried out interesting research on this subject, but even in this case the estimate is very ineffective.

In 1967 a letter of Petrovskii and Landis appeared [7] where it was admitted, with a reference to me, that there was no theorem.

Supplement.
Several questions addressed to S. P. Novikov

(the questions were asked by V. M. Buchstaber)

V. M. Buchstaber: *Sergei Petrovich, in* `Polit.ru` (*see* `http://www.polit.ru/science/2009/07/14/ilyashenko1.html`) *an interview with Yu. S. Il'yashenko was published. Here is an excerpt from this interview, in which the Petrovskii–Landis paper and you are mentioned*:

> "On the other hand, my own relations with differential equations were somewhat special. I started studying at the university simultaneously under V. I. Arnold and E. M Landis. Evgenii Mikhailovich Landis at that time was writing a book devoted to the solution of Hilbert's 16th problem which was found by him together with I. G. Petrovskii (rector of MSU from 1951 to 1973) and which later proved to be false...
>
> And after a year or a year and a half of such studies I was, as fate willed, involved in re-examining the Petrovskii–Landis paper and re-proving the result. When I was in my third year, in the spring of 1963, my re-examination process reached a dead end. I saw that, even with the guidance of Landis, I did not understand and could not reproduce a certain part of his paper with Petrovskii. I came to E. M. and told him about this; he tried to give an answer, became pensive, and did not answer right away. After that he and

I had regular discussions, in the course of which he tried to give a new proof, but I would find an error…

At that time, in 1963, Arnold, Novikov, and Shafarevich were very interested in the Petrovskii–Landis paper, which was being studied in several seminars. The young Sergei Petrovich Novikov gave a talk about it in Gelfand's seminar. This was already in the autumn of 1963."

Correspondent: *Was this a fresh proof*?

"This proof was written in the unpublished book of Landis and Petrovskii. Novikov gave two talks about things that I knew well and which were true, but in a third talk he was going to discuss something that I knew was false. I came up to him before the talk and said: 'Serezha, there is a certain mistake in what you are going to speak about.' He was then a graduate student and I was an undergraduate, and I addressed him by his first name, but he did not know my name. In a minute he understood what the matter was, nodded, and when the seminar started, Novikov, walking forth and back, said: 'Well, we looked into this and saw that there was a mistake here.' Gelfand asked: 'Who is *we*?' Novikov said: 'We, there' and pointed to the audience. The large auditorium was full. Gelfand looked at the audience and asked once more: 'Who is *we*?' 'We, there'—said Novikov. 'Who is *we*?'—again asked Gelfand, now starting to become irritated. Novikov simply did not know my name; he pointed towards where I was sitting, but people were sitting close together. At the third or fourth time it occurred to Gelfand to turn to the audience and ask: 'Who is *we*?', whereupon I timidly stood up and introduced myself."

Correspondent: *Were you the only one who found the mistake*? *Over all those years since the proof appeared*, *did no one discover it*?

"No one. And then it was Novikov who countered all attempts of Landis to save the proof, and when in 1967 the authors retracted their proof in a letter in *Matematicheskii Sbornik*, Novikov was named as the person who found the mistake, because at that time he was already an internationally known scientist, and I was merely a Ph.D. student who did not have any achievements, and it would have been simply inappropriate if our two names were present in the letter…"

Question 1. *What do you have to say about this*?

Answer. The poetic fantasy of Yulik Il'yashenko has carried him far from the real facts. I never gave any talks on this subject at Gelfand's seminar. In 1963 I even had not yet had time to master the subject. Yulik may possibly have heard the word *we* from me, but only later, and then I meant Anosov, of course, but instead of Anosov he substitutes himself. In particular, I said to Gelfand approximately in 1965 that Anosov and I worked on this. Our conversation took place publicly before Gelfand's seminar, that is, in room 14-08 or nearby, and many could have heard it. Apparently, Gelfand informed Petrovskii (though perhaps after some time), to whom he was close.

Imagine the picture painted by Il'yashenko: "A nameless student comes hesitantly up to me in order to tell me about a key mistake in the famous paper of his supervisor. And I, quick on the uptake, at once understood everything and immediately put it to use under my name." This scene merits a movie version. In Il'yashenko's curriculum vitae on the Internet there is no indication of his official scientific supervisor. Let me inform you: it was Landis and not Arnold who was his supervisor. What do you think: that knowing all about the mistake since 1963 and discussing it very privately with his supervisor, he began letting it come to light, and behind the back of Landis? But of course Il'yashenko did not betray Landis. And why did Petrovskii not know this before 1967? Either there is simply nothing to this, or he and Landis were concealing what they knew, until it became widely known from me. The letter of Petrovskii and Landis was submitted to *Mat. Sbornik* on 21 March 1967 with a reference to me, when Petrovskii learned about the matter. There is no doubt about this.

The last paragraph quoted here from the interview with Il'yashenko has upset me very much. He accuses Petrovskii of violating scientific ethics: according to Il'yashenko, his name was deleted from the text of the letter in which the mistake was admitted because he was still young and had insufficient authority in mathematics. I declare that Ivan Georgievich Petrovskii would never have allowed himself to do anything like this. Of course, he might have been angry if he thought that Landis and Il'yashenko had concealed from him the failure of this paper after learning that the mistake was already being discussed in public. However, Petrovskii took Il'yashenko into the department after his Ph.D. studies, and now no gratitude is shown. Well, no good deed goes unpunished, as the philosophers say.

Recently in various editorial boards I have already managed to correct quite biased poetic deformations of real facts on the part of Yulik Il'yashenko. In all these cases it was about concealing mistakes of people close to him. Many do not realize that this is a violation of scientific ethics against those researchers who have completed the correct solution of the corresponding problem. In the text of the interview he uses my name arbitrarily, inserting it into scenes created by his imagination. I am therefore forced to answer him.

Question 2. *What can you say about a much later paper of Il'yashenko* (*about* 1990) *devoted to the proof of Dulac's theorem on the finiteness of the number of limit cycles*?

Answer. Yes, I heard about this. Consider that I have not returned to this subject for more than 40 years, so I may not have certain important information. While there is no general estimate for the number of cycles for all systems of a given degree, this problem reduces to a subtle analysis of deeply degenerate situations. Il'yashenko presented a proof on several hundred pages. His French competitors independently presented a proof based on different ideas and several times shorter, but still their text exceeds a hundred pages. I cannot give you an answer to the following basic question: is there evidence that these proofs have been verified by the world mathematical community? Are their methods used by other researchers? Are they expounded in lecture courses? When such evidence appears, I will say that these are very good papers — of course, only about the proof that has been verified. Anosov informed me that there was some earlier work by some other author who solved this problem for the case of 2nd-degree polynomials.

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Translated by E. KHUKHRO