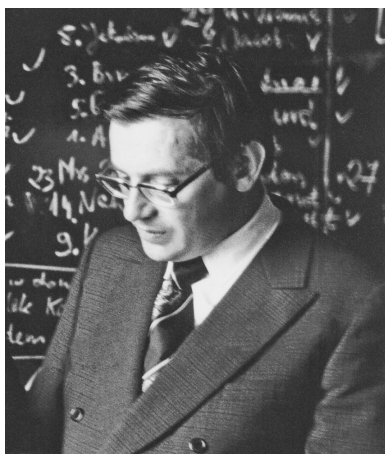


## Dmitry Viktorovich Anosov: His life and mathematics

Anatole Katok

Dmitry Viktorovich Anosov (Dima for his friends) died on August 7, 2014 at the age of 77. This article is a tribute to his memory. It consists of two parts, different in style and purpose.



D.V. Anosov in 1977

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The first part contains personal recollections touching on both professional and social matters. All information there is first-hand; all opinions are strictly my own. I did not try to ask other people or do any research to provide any kind of coherent narrative. My goal is to preserve memories of events and attitudes that not many people have ever known, and even fewer remain in possession with passage of time. I tried to present and preserve an image of a pretty remarkable man who lived through complex times and whose views of the world and people around him were lucid and free of illusions without becoming cynical. He followed a certain implicit code of honor more strictly than many of his contemporaries, even some of those who had reputations of being more progressive and liberal.

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I'd like to emphasize that my recollections concern the man I knew in the late Soviet period, namely from mid-1960s till 1982 (the latter year is the date of our meeting in Germany, the first after my 1978 emigration from the Soviet Union). I also met Anosov several times in the post-Soviet period in the US and in Russia. I do not include any recollections of those meetings; in fact I do not remember much of great interest. I'd like to point out though that, judging by Anosov's later writings, especially the historical surveys [8] and [9], his outlook changed in later years, probably in the direction somewhat away from the picture I try to present.

The second part is a brief sketch of Anosov's work, primarily from the same period that is covered in the first part, and its influence on the broader mathematical community.

## 1. Personal recollections

**Algebraic topology course.** I first met Anosov at some time in the mid-sixties when he was already a very accomplished and well-established mathematician and I was still an undergraduate student although our age difference is less than eight years. I do not remember the first meeting or first introduction. What I do remember is that our first serious interaction was in 1966 (I believe in the fall of that year) when my thesis adviser Ja. G. Sinai asked me to take what now would be called a "reading course" in algebraic topology with Anosov.

I was not a novice in algebraic topology at the time. Although there was no regular undergraduate course on the subject at the Moscow State University when I was a student, algebraic topology was considered then and there the "queen of mathematics" (or at least one of very few principal ladies) and every self-respecting student was supposed to learn quite a bit of the subject somehow. I sat through the remarkable special (topics) course given by D.B. Fuks attended for most of the semester by 200-250 people, and fairly carefully read few books, both classical and modern.

We used the classical book by Hilton and Wiley as a text. In fact, my task was to solve independently all problems/exercises from that book (that I did successfully) and also ponder about a specific then unsolved problem relating topology and dynamics: rationality of  $\zeta$ -function for Anosov diffeomorphisms. Most of the time during our meetings was taken by discussions of various topics and issues emerging from those problems. So I had an ample opportunity to develop my views of Anosov as a topologist. He was a master of the subject in full possession of all essential results, topics and techniques. The reader should keep in mind that topology was never Anosov's principal mathematical area; he published only one expository paper on the subject [10], albeit in the prestigious *Uspehi*. This first impression is consistent with the opinion that I formed and held later when we interacted closely and extensively. If Anosov claimed to know a major or minor mathematical subject, he knew all its ins and outs, otherwise he would either profess ignorance or dismiss the topic.

**Anosov-Katok method.** If our interaction during the topology course gave me an impression of Anosov as a scholar, some time after that I had a superb opportunity to observe and appreciate his creativity. In retrospect this was the high point of original creative thinking that Anosov displayed during the period of our close contacts, from 1966 till early 1978, and, I believe, also afterwards. In front of my eyes Anosov invented the core of what has become known as "Anosov-Katok

method”<sup>1</sup> for construction of dynamical systems with interesting, often exotic properties.

I will tell the story with minimal but necessary mathematical technicalities in the second part of this paper. This joint work published as [12] is considered by many as the (probably distant) second most important mathematical contribution by Anosov after his major role in the creation of the modern theory of dynamical systems with hyperbolic behavior immortalized by ascribing his name to several important classes of such systems.

It’s a pity I do not remember exact date of Anosov’s inspired invention; I am pretty sure this was during the second half of 1968. I very quickly added my essential and extensive contributions that greatly extended the power of the method and several weeks of discussions followed. Then I remember vividly having written a complete draft of the paper just from my head in three successive evenings on Friday, Saturday and Sunday (I never reached a comparable level of productivity in my life, before or after) and having extensive discussions with Anosov that lasted for many weeks and resulted in the final version. Typing (by a professional typist) from the manuscript and inserting formulas and drawing pictures by hand (the last task was performed by my wife) was not a very fast process either and the only hard date is that of the journal submission: May 20, 1969. We published a short announcement in *Uspehi* [11], but I think it was written after the main text and in fact it appeared in print the same year (1970) as the main text.

For more than ten years that preceded my emigration from the Soviet Union in February 1978 my contacts with Anosov, both professional and social, were frequent and extensive. In fact, my wife and I became close friends with Anosov. I will not follow exact chronology but rather try to address various facets of Anosov’s personality, his attitudes and characteristic actions or sometimes inaction.

Mathematically there were strong mutual influences. Our joint work at the beginning of the period owes its framework to the theory of periodic approximations that we developed few years earlier with A.M. (Tolya) Stepin. On the other hand, my own interests during the period were moving more and more toward hyperbolic dynamics and its variations and was greatly influenced, directly and indirectly, by Anosov and his work. Conversely, an observation in one of my papers [20] that developed new applications of our method led Anosov to his next major interest, variational methods in Finsler geometry.

**Our seminar.** The principal vehicle around which our professional interaction was organized was a weekly afternoon seminar that most likely met at the university during the 1969-70 academic year, and then definitely at the Steklov institute from the fall of 1970 till 1975, and through the spring of 1977 at CEMI (The Central Economics-Mathematics Institute of the USSR Academy of Sciences) where I worked. I had an opportunity earlier to write in detail about this seminar including Anosov’s role in it [22]; see also [14]. Anosov was already perceived as a “senior statesman” although he only turned forty toward the end of this period. Here is a relevant quote from [22] about Anosov’s role: “He was brilliant and quick, and possessed a very perceptive and critical mind. Everybody, around him, including myself, greatly benefited from from his comments, criticism, and help with pointing out and correcting errors.”

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<sup>1</sup>Since I still use this method in my work I prefer to call it descriptively the “approximation by conjugation method”

**Anosov's moral code.** Let me try to describe Anosov's moral position in professional life at the time. Mathematics was not exempt from general trends that dominated the life of the country during the late Soviet (post Khrushchev–pre Gorbachev) period. Two of those tendencies, most relevant for the life of the mathematical community, were, first, discrimination against Jews, and, to a lesser extent, other groups, ethnic, social or professional, and, second, pervasive corruption that led to erosion of professional standards. Any mathematician who had a formal or informal standing and influence faced issues related to those tendencies constantly, and had to determine his or her position and line of behavior. Needless to say, many people behaved inconsistently and opportunistically so often there was a gap between convictions (openly stated or not) and actual behavior.

To Anosov's great credit, his position at the time was both consistent and explicit and his behavior on all specific occasions, known to me, fully agreed with this position. It can be summarized like that: (1) do no evil; (2) do right things if there is no danger of direct clash with authorities immediately responsible for the matter; (3) act within established institutional structures; (4) do not try any endeavor that is either doomed or would require a moral compromise beyond certain narrowly defined bounds.

This code of behavior may not look heroic; many intellectuals, including mathematicians, at the time declared more radically progressive views. From their standpoint Anosov was a conformist and to a certain extent a “collaborator”. The problem with this position is that most of those holding and declaring progressive views were not able to act according to those views and ended up doing nothing at best or making grave moral compromises at worst.

Before illustrating this general description with examples let me formulate my own attitude toward Anosov's moral code. I was greatly impressed by (1); he followed this fundamental principle rigidly. I do not know whether its origins had a Christian admixture or were fully secular. He was aware of the necessity of moral compromises to advance a good cause (I will mention an example later) but stayed within strict limits, and, while a few of these compromises had harmful consequences, those were results of mistakes in his original judgement or unforeseen outside circumstances. (2) resulted in many good things in practice, some of which I will mention in due time. It is important to emphasize that Anosov always considered as “authorities” only people occupying particular positions on whom relevant decisions depended: an editor-in-chief of a journal, a department head in an institute, a dean in a university or the chair of a scientific council responsible for accepting dissertations. Thus his apprehensions were strictly limited and based on the knowledge of concrete persons and their positions. To the best of my knowledge, he was never prevented from acting for a good cause by any general trends or policies by academic, let alone party, authorities. (3) I took matter-of-factly; boldness in organizational matters, that was not completely impossible under the circumstances of the time, was not in Anosov's character. (4) annoyed me a bit, since sometimes Anosov's sober estimate of difficulties of a certain undertaking led to inertia. To his credit, he never resisted initiatives by others that concerned him; moving of our seminar to CEMI after Pontryagin's attempt to sabotage it that is described in [22] is an example.

**Helping young mathematicians.** Anosov's personality was less engaging or flamboyant than that of several of his contemporaries. Besides, his main position

was in a non-teaching institution so he did not have constant contact with students. As a result of this there are not many mathematicians for whom Anosov was a primary Ph.D. adviser in the way that is usually understood. During the period covered by this recollections Anosov had two such students: A.B. Krygin and A.A. Blohin. The former published several very good papers related to our approximation by conjugation method and to Anosov's program on cylindrical cascades during 1970s but unfortunately stopped publishing soon afterwards. The latter showed early promise but published only one paper and did not even defend his Ph. D. due to illness.

There are however several successful and even highly accomplished mathematicians whom Anosov helped both with their mathematics and with their careers and for whom this help was crucial. Two best known and most impressive among those are of course M. (Misha) Brin and Ya. (Yasha) Pesin. Their names will appear later in this article. More information can be found in their article in this volume [14] and in my article [22]. For both of them, aside from very significant help with their work and acting as the official thesis adviser, Anosov provided invaluable help that was needed to overcome the difficulties of being "out of the system" due to their Jewish origin and strong anti-semitic tendencies of the time.

**E.A. Sataev.** Now I'd like to tell a story of another mathematician, E.A. (Zhenya) Sataev, who died in 2015 whose accomplishments are somewhat less known than they deserve. This story shows that Jewish decent was not the only source of difficulties young people faced in the Soviet Union. Sataev was a student at the Moscow State University; he came from a village or a small town in the Volga area and was not an ethnic Russian but belonged to one of the Finnish peoples native to the area. Unlike the majority of successful students in the university he really came from the midst of ordinary people. Sataev's first undergraduate adviser was Stepin who left for Egypt around 1970 for what was supposed to be a multi-year appointment that was cut short by the famous expulsion by Anvar Sadat of all Soviet personnel from Egypt. Stepin left his two students, R.I. Grigorchuk (later of groups of intermediate growth or "Grigorchuk groups" fame) and Sataev, to me. When Stepin returned, Grigorchuk continued to work with him but Sataev stayed with me. Sataev did not have a residence permit for Moscow or Moscow district so he was not able to get a job in Moscow or nearby. My standing at the time of his graduation (1972) was not sufficient to recommend Sataev for the graduate program at the university. In any event Sataev decided to go to work in one of the notorious Soviet "secret towns" (Arzamas-16) that paid a good wage and was located not too far from his family home. Very quickly Sataev realized that this had been an unwise decision since it greatly restricted whatever limited freedoms ordinary Soviet citizens enjoyed. At the end of his initial contract Sataev was able to leave for a graduate program in mathematics. But I still was not able to help him with admission to the university program<sup>2</sup>. Anyway, I mentioned Sataev's situation to Anosov and he generously offered to have Sataev admitted to the graduate program at the Steklov Institute on the understanding that Sataev will continue

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<sup>2</sup>I am not sure why I did not try to have him admitted to the program at my place of work, CEMI (Central Economics and Mathematics Institute of the USSR Academy of Sciences). This was feasible. Maybe I already started to think about emigration and did not want to leave Sataev unprotected. Or, more likely, since the pressure to do work related to the institute mission increased, I did not feel I could have a student working in pure mathematics.

to work with me. Unlike the cases of Brin and Pesin, where Anosov was a genuine co-adviser, this was a “cover”: Sataev worked then on topics unrelated to Anosov’s interests. This “cover” was at least as valuable for Sataev as theirs was for Brin and Pesin. In the event, Sataev did an excellent thesis work on Kakutani equivalence theory that came earlier than a similar project by the giants of ergodic theory D. Ornstein, D. Rudolph and B. Weiss and was only marginally weaker than theirs. This followed his similarly impressive Masters thesis (on a quite different topic); both were published in *Izvestija*. I like to believe that Sataev’s Ph.D. defense took place on February 15, 1978, the day I left The Soviet Union for good. I still have a pretty huge twelve-layers matryoshka as his parting present. While this date may be wrong by a couple of weeks, having Anosov as an official adviser guaranteed that Sataev was not tainted by too close an association with an emigre.<sup>3</sup> Sataev still had no residence permit for Moscow or the district so the best he could do was to get a job in Obninsk, a restricted but not fully secret town less than a hundred miles from Moscow. He had a successful career there becoming a Doctor of Science and Department chair and consistently doing good work in a particular area of hyperbolic dynamics, but I have reasons to believe judging by his brilliant early work that the relative isolation of the place prevented him from realizing his full potential.

**Anosov as journal editor.** In practical terms, the great majority of situations where Anosov had to exercise his professional judgement and face moral dilemmas appeared in connection with consideration of papers for publication and defense and approval of dissertations, both candidate (equivalent to Ph. D.) and the higher level Doctor of Science. At the time Anosov was on editorial boards of two journals: *Izvestija* of the USSR Academy of Sciences, and *Zametki* (Mathematical Notes of the Academy).

Those days virtually all papers by mathematicians in the Soviet Union were published in domestic journals; the leading journals were quickly translated into English cover-to-cover and published in the West. Hierarchy of journals was quite important. The top tier consisted of *Izvestija*, *Sbornik*, *Uspehi*, and *Doklady* to which is often added irregularly published *Trudy MMO* (Transactions of the Moscow Mathematical Society).<sup>4</sup> Among those only the first two (and *Trudy*) were regular vehicles for publishing complete original papers; *Izvestija* in general was considered the most prestigious among the three. *Doklady* published research announcements with the strict limit of four printed pages and *Uspehi* at least officially was dedicated to publishing surveys, although in reality often those “surveys” contained a high proportion of original results. *Zametki* was among the best journals in the next tier.

Both discrimination and corruption issues appeared in the context of publication of mathematical papers. Discrimination pressure was felt more acutely in the journals published by the Academy due to pronounced anti-semitic attitudes of the academy bureaucracy and some leading academicians, including I.M. Vinogradov,

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<sup>3</sup>We had published a joint paper in *Zametki* in 1976 but this degree of association was not harmful at the time.

<sup>4</sup>“Functional Analysis and its application”, established in the sixties, de facto maintained as high and occasionally even higher level than any of those journals; it, however, had a reputation of being the mouthpiece of Gelfand’s school.

L.S. Pontryagin and A.N. Tykhonov.<sup>5</sup> Influence of corruption was felt through preferential treatment of papers by certain authors, and, conversely, keeping away the work of their competitors, loose refereeing standards, and suchlike.

Thus on boards of two leading Academy journals Anosov faced not the most friendly environment. Still during the ten-year period I observed him in this capacity he never swayed from the highest standard on research and scholarship and never hesitated to promote high quality papers, independently of personalities of their authors.

Careers of Brin and Pesin were launched by the publication of their major paper on partial hyperbolicity in *Izvestija* [13], a landmark in the field. This occurrence is hard to imagine under standard circumstances existing at the time with two unknown young Jewish authors who were not even graduate students but worked in institutions unrelated to mathematics (for more on their circumstances at the time see [22]). Another paper of Pesin, [29], that contained the core technical results of celebrated “Pesin theory” appeared in *Izvestija* a couple of years later.

A convincing, albeit indirect, illustration of my thesis comes from difficulties both Brin and Pesin faced in submitting and defending their Ph.D.’s based on their world class work (two *Izvestija* papers, one *Uspehi* paper plus publications in other first-rate journals, including *Zametki*) that under normal circumstances would qualify each of them for the Doctor of Science degree. In both cases anti-semitic attitudes and policies prevented them from having their dissertations accepted for defense in the leading places such as Moscow State University, Steklov Institute or even other places in the capital, and forced them and their backers to look for places outside of Moscow. Brin was able to defend his thesis in Kharkov in 1975, of course with strong backing by Anosov, but using some key connections of his own to overcome even more pronounced anti-semitism that existed in the Ukraine at the time. Pesin had to wait till 1979, when his work already became world famous, and it was entirely due to Anosov, that he was able to defend his dissertation in Gorky (now Nizhnij Novgorod). Very interesting story of Pesin’s defense is told in another article in this volume [14]; notice in particular the moral compromise that Anosov consciously made to achieve success. As far as I know, a previous attempt by Anosov to have Pesin’s dissertation accepted for defense in the university of Rostov was unsuccessful, but this setback did not discourage Anosov.

I published two major papers in *Izvestija* during the period [20, 21]. The story of publication of the second paper is worth telling since it vividly illustrated some of the features of Anosov’s approach to the difficulties existing at the time that I described above. The paper presents a core of what I called “monotone equivalence theory” in ergodic theory and is now commonly called Kakutani equivalence theory. It is based on results that I obtained in 1975 and early 1976. It was an extensive body of work and by no means exhausted the subject by that time. A *Doklady* announcement of key results was published in 1975 and I asked Anosov about feasibility of submitting the paper with complete proofs (some of which were not yet written then) to *Izvestija*. Anosov had a very high opinion of the results and asked me how long the paper would be. I answered that after everything is completed it

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<sup>5</sup>*Uspehi* and *Trudy* were published by the Moscow Mathematical Society; *Sbornik* was a joint publication of the Society and the Academy; and *Doklady*, while an Academy publication, published papers communicated by individual academicians without additional reviews.

would be about 100 pages. Anosov explained that he would not be able to have a paper of such length published in *Izvestija*.

The editor-in-chief of the journal was elderly I.M. Vinogradov, then 84 years of age but still director of the Steklov institute and the N1 in the hierarchy of mathematicians in the Soviet Union. As I already mentioned, he was a convinced and inveterate anti-semite. His anti-semitic attitude was apparently not of opportunistic nature as was the case with many Soviet officials and even scientists but was a deeply held conviction stemming from attitudes of the hard right in the late czarist times.

His deputy was I.R. Shafarevich, a great mathematician who held and in fact expressed strong anti-communist views, and, surprisingly, was only mildly reprimanded for that. Of course, later he became infamous for his chauvinistic and anti-semitic writings and became an icon of the Russian nationalist hard right. Still, in fairness, his anti-semitism at the time (and, I believe, later too) was of a theoretical nature and did not descend into hatred of individual Jews simply because they were Jews. In any event, as a de facto editor-in-chief of *Izvestija*, Shafarevich followed a high-minded and fair policy. He could approve all articles with strong positive recommendation by other editors up to a certain length independently of the authors' nationality and other extraneous features without showing them to Vinogradov.

But for exceptionally long articles (and 100 pages was over the limit) Vinogradov's direct approval was required. Anosov was ready to argue merits of my work with Shafarevich and other members of the editorial board (not all of them friendly to Jews) but not ready to confront Vinogradov who would almost surely veto publication.

Anosov's suggested solution was simple: to split the work into two papers about 50 pages each and publish them with some time interval. For that, approval of Shafarevich, which would be forthcoming, was sufficient. In order for this scheme to succeed an interval of about a year between publication of two articles was required; if the interval was too short Vinogradov may be informed that splitting of the paper was a ploy.

This conversation took place at the end of 1975 or at the beginning of 1976. By then my tolerance of life in the Soviet Union was wearing thin and I already decided to leave the country after some necessary preparations. When in 1971-72 for the first time emigration became a realistic possibility with a tolerable level of risk and our friends and colleagues started leaving, my wife and I seriously discussed the possibility and decided to stay put. This changed by the late 1975 due to a variety of factors.<sup>6</sup> Thus I knew that I may not have time to publish two papers with an interval of a year. On the occasion, the 54-page long paper [21] was submitted to *Izvestija* on March 3, 1976 and appeared in print in the first issue of 1977, i.e. around February of that year. On February 15 of 1978 I left the Soviet Union with my family for good as stateless persons stripped of the Soviet citizenship. I applied for emigration in July of 1977 and this quickly became public knowledge. Thus if I submitted the second part early in 1977 or even late in 1976 it would not have appeared in print by that time.

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<sup>6</sup>I hope to discuss the matter of emigration in detail on another occasion since it has only tangential relation to my present subject.



What happened with submitted or even accepted papers of would-be emigrants was well-known. A paper by B.G. Moishezon, a brilliant algebraic geometer, one of the favorite students of Shafarevich and my CEMI colleague, who emigrated in 1973, that was not only accepted but already typeset for an issue of *Izvestija*, was removed and the issue came out thinner than usual. My wife Svetlana Katok, whose short paper was scheduled to appear in the more friendly *Uspehi*, was asked by a very honorable and decent editorial board member to withdraw the paper when we applied for emigration. The motivation was not to expose the journal and the society to attacks and criticism by the party watchdogs.

Thus I decided to call off the still unfinished second part of my paper, pack into the last section of the fist (and, as it turned out, the only) paper the announcements of remaining results, and submit it as fast as I could. As is seen from my narrative, I had just a few months to spare. I got a consolation from simultaneous publication by *Izvestija* in the first issue of 1977 of Sataev's thesis paper that was in a way a continuation of mine albeit in a quite different direction than my projected second part.

**Refereeing and approving dissertations.** Anosov was a member of the Higher Attestation Board that had to approve all mathematics dissertations defended in the USSR. For readers not familiar with the Soviet/Russian academic system here is a brief summary that ignores issues related to discrimination and corruption.

The candidate degree is usually considered an equivalent to Ph.D. but signifying a slightly higher level of achievement and accordingly carrying more prestige. The main requirement was a dissertation accompanied by publications. The dissertation was to be submitted to a scientific council with fixed membership (typically 15 to 25 members) attached to a particular university or a research institute. It has to be accepted for defense that was public with two referees (somewhat misleadingly called "official opponents") and outside review from a "leading organization in the field". Defense included a presentation by the candidate, speeches by the opponents, reading of outside review, usually also a speech by the thesis adviser, and a free-format discussion in which both the council members and guests could participate. The vote was secret and a two-third majority of positive votes was required for approval. Active mathematicians usually defended their candidate theses within 3-8 years after graduation from the university. Three-year (post)-graduate studies program was meant to prepare the candidate thesis; it could be taken even right after graduation or after an interval of time. It was not a precondition for submitting the thesis. In fact, both Brin and Pesin did not attend the graduate program [22].

Doctor of Science degree was very prestigious, at the time there were no preparatory courses, defense procedure was similar but with three opponents, all of whom naturally should hold the degree. There were fewer councils that were authorized to accept Dr. S. dissertations.

All dissertations have to be approved by a central authority called the Higher Attestation Board (VAK). Under normal circumstances its function was to exercise quality control that was necessary due to great variation of standards across the degree-granting institutions. I believe in the seventies it performed this function to a certain extent but in addition to that the Board (or its particularly eager individual members) also watched after dissertations whose authors were Jewish and on a

number of occasions rejected them. Needless to say, that Anosov's position as a member of the VAK's mathematics panel was based on strict professional standards.

While to guarantee a successful outcome of Pesin's defense in Gorky Anosov had to make a promise to see a sub-standard dissertation through VAK [14], he was fully aware of the moral cost and realized that this was the only way to help Pesin whose work by then was several levels above the accepted standards for a PH.D. Overall Anosov looked after a number of excellent dissertations by Jewish mathematicians (they were mostly people who had some local connections to pass the hurdle of defense) that would not have passed VAK without his intervention.

**Warsaw 1977 conference.** Here is another episode from the same period that demonstrates Anosov's attitude.

In the summer of 1977 an international dynamical systems conference was organized in Warsaw by a group of Polish mathematicians among whom Wieslaw Szlenk played the principal role. An explicit purpose of that conference was to arrange a major encounter between "the East" and "the West". This was spectacularly successful.

There were two groups of participants from Moscow. An officially approved "delegation" was headed by Anosov and included also Stepin and E.B. Vul. Three other participants, Brin, M.V.(Misha) Jakobson, and myself, came ostensibly on private invitations of our Polish colleagues. This was the only realistic way for us to travel outside of the Soviet Union; any attempt to obtain permission to go on official business would be blocked by one of the numerous bureaucratic offices or party committees whose approval was required. The principal but unstated reason would be that the applicants were Jewish. One that might be stated and did have relevance, was that the subject of the conference did not fit with the principal specialties of our places of work. Of course, from the point of view of the conference organizers we were fully-fledged participants of the conference; maybe even somewhat more interesting than the official delegates since we had not traveled to the West before and were new for the Western participants.

So I come to the punchline. Anosov, as the head of the official delegation, was supposed to write a report to appropriate authorities in Moscow, I presume the administration of the mathematics division of the academy, or the Steklov Institute. He was in a bind: to acknowledge the presence of unauthorized participants from Moscow who spoke at the conference with a considerable success, or to lie. He found an imaginative solution, very much in his style. While he socialized and closely interacted with us during the conference, he was conspicuously absent from our talks. In a sense this was mocking his official status and obligations. But looking from another viewpoint, he took a certain risk. Obviously, he planned to ignore our presence in his official report. But, if there was a KGB informer in the audience, Anosov could be denounced for socializing with unauthorized conference participants from Moscow and not admitting their presence. Such a possibility could not be excluded given a large number of local people (and probably some Soviet visitors unrelated to the conference) in the audience.

This is a good example of "passive resistance" that Anosov practiced in a variety of situations.

**Relations with Pontryagin.** Anosov was a student of L.S. Pontryagin, one of the greatest among the first great generation of mathematicians of the post-revolutionary (Soviet) period. Pontryagin's evolution, both as a mathematician and as human being, presents a somewhat sad sight. In the 1930s and 1940s he was a brilliant creative and very broad mathematician who made a great impact in algebraic topology during its formative period, created duality theory for locally compact abelian groups and, together with the physicist A.A. Andronov, became one of the creators of modern theory of dynamical systems. He was also known for his independent and on the whole honest and courageous behavior in professional life that is not a small compliment for someone who lived in the Soviet Union through that terrible period. Sometime during the 1950s, when he was still in his mid-forties, his mathematical interests moved toward more applied direction and he made a major impact in that area as one of the creators of the modern theory of optimal control. After that, his mathematical standards dropped and he was in general surrounded by people of less than stellar quality, if not outright hacks. Pontryagin wrote a textbook in ordinary differential equations and he taught the regular ODE course to my sophomore class. His presentation was heavy and not very illuminating and it was hard to believe that was the same man who more than twenty years earlier had written the masterpiece "Continuous groups" that still remains as good and illuminating presentation of the Pontryagin duality and related subject as any. It was also strange that Pontryagin tried to supersede then standard in the Soviet Union ODE text by the great I.G. Petrovsky that, even though a bit outdated by now, still makes an excellent and lucid reading.<sup>7</sup> One had a nagging feeling that Pontryagin pushed his approach and his book out of spite of Petrovsky and that feeling unfortunately has some support in the story of evolution of Pontryagin's personal views. More or less simultaneously with his turn toward applied mathematics Pontryagin's views and behavior became quite retrograde and reactionary. He became a pronounced anti-semitic and this attitude found an expression in his professional behavior.<sup>8</sup>

Anosov was Pontryagin's graduate student at the Steklov Institute on the ODE side. He was clearly Pontryagin's favorite. He was greatly influenced by Andronov-Pontryagin seminal work on structural stability and his presentation, both in the early papers on averaging and in the classical work on hyperbolic dynamics, was clearly influenced by Pontryagin's style of the period. Pontryagin was the head of the ODE department of the Steklov Institute and Anosov stayed in the department after the graduate school. Pontryagin not only highly valued Anosov's work but also from the beginning considered Anosov his trusted lieutenant. Steklov Institute as a whole and its individual members, especially those in senior positions, wielded considerable influence, if not outright control, over research enterprise in mathematics. Anosov, despite his young age and a somewhat reticent personality, quickly became a member of this senior elite. Even though I do not know details of the inner workings of the Steklov elite at the time, it is clear (and is confirmed

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<sup>7</sup>Petrovsky's approach indeed needed some updating and that was brilliantly accomplished a bit later by V.I. Arnold

<sup>8</sup>My opinion of Pontryagin's course and his text is at variance with what Anosov wrote several decades later in [9]. Anosov, while acknowledging tensions between Petrovsky and Pontryagin, and certain decline in Pontryagin's standards, takes Pontryagin's side and in fact makes some disparaging remarks about Petrovsky.

by Anosov's occasional remarks) that this quick accent was due to patronage and protection of Pontryagin.

That at the time Anosov disapproved of Pontryagin's attitudes and behavior on the Jewish issue was quite obvious and he expressed this disapproval in private conversations, as well as in some actions. Here is what I wrote on another occasion [22]: "Anosov, a former student of Pontryagin, considered for a while as his picked successor, refused to follow the hard line of his bosses, and, short of open rebellion, was sabotaging their agenda with considerable success."<sup>9</sup> I continue with details about the move of our seminar from Steklov to CEMI that Anosov approved after Pontryagin refused to authorize the list of persons for admission to the building with many Jewish names. At the time I was under the impression that cooling down of relationships between Anosov and Pontryagin went further and came close to a formal break. Unfortunately, I never asked Anosov about that when I met with him in post-Soviet times. According to S.P. Novikov, who possesses lots of inside information that is not always 100% reliable, no visible cooling down or a break has ever taken place. So I rest here.

**Private life.** Anosov came from an academic family. Both of his parents were chemistry professors/researchers of considerable repute. In fact, there is an article about his father Viktor Yakovlevich Anosov in a respectable Russian series "Scientific heritage of Russia" where V.Y. Anosov is called "one of the most important specialists in the area of physical chemistry analysis". The mother Nina Konstantinovna Voskresenskaya also held a Dr. S. degree. I have known a number of Soviet academic families of that generation when material rewards for upper-crust academics were very high compared to the overall living standards, e.g. the base pay of two professors/Dr. S.'s was about 12-15 times the average earning of a person with a college degree. Such families in the 1940-50s and, to a lesser extent, in 1960-70s typically enjoyed lifestyle with pronounced bourgeois overtones: a spacious well-furnished city apartment, with valuable items, often even good works of art, good food, a car, often a country house (dacha), until about 1960 a live-in maid; later a part-time maid/cook. The Anosov family that at the time I got to know it, had three, not just two, high-earners, presented a great contrast to that stereotype. They did live in a large by the Moscow standards, apartment in a good (but not great) location and they did have a rather sorry looking woman helper (the parents were well in their seventies), but there it stopped. The furniture was spartan, to put it charitably, or plain shabby, the food very simple. The lifestyle of the family can be described as ascetic. When I first got to know them the father was still alive but looked very old and fragile but the mother was still quite active. The father died in 1972, the mother around 1975. The only luxury was an excellent for the time stereo-system and a collection of high quality records of classical music, mostly foreign made. Dima was a great lover and connoisseur of classical music. Those records were practically the only things Dima brought from his relatively frequent foreign trips. At the time when Levi's blue jeans were both the badge of distinction and almost an alternative currency it is extremely remarkable that he did not buy abroad any clothing items and dressed in an old-fashioned and somewhat awkward way in domestically made clothing.

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<sup>9</sup>Anosov did become the department head but already under somewhat different circumstances

I see two reasons for this contrast. The principal one is the difference between the old Russian and Soviet “intelligencia”. The former defined itself mostly by moral and intellectual attitudes and very often, although not always, was indifferent to the material comforts, let alone luxury. It was characterized by great sensitivity to the plight of the poor and the disadvantaged and often made material sacrifices to alleviate it. The latter, to allow for a certain oversimplification, thought of itself as an elite of the middle class whose material and spiritual interests were in a sort of balance or equilibrium. Thus, those of its members (by no means a majority) who could afford good things in life, usually went for those, conforming to the generalized picture presented above. Anosov’s family belonged to intelligencia at least in the third generation and it quickly became clear to me, that the parents spiritually belonged to the old Russian intelligencia although their careers spanned the Soviet period. I heard that Anosov’s parents used to support poor students and other destitute people in keeping with the Russian intelligencia traditions. I will comment on Dima’s generosity later. Still I believe the expenditures of the family were much smaller than their earnings. While the parents may have been genuinely disinterested in material comforts Dima, who belonged to a different generation, was not averse to enjoying some of those.

And here comes another subsidiary reason. At the time (the late Soviet period) there was great scarcity of quality items of almost any kind (food, clothing, furniture, books etc) through regular distribution channels. Money as such could buy little. One needed in addition “connections” in the form of access to official (special stores) semi-official (wholesale distribution chain) or unofficial (black market) alternative distributions channels. Dima lacked skills necessary to obtain such access to an astonishing degree. Only when he married shortly after his mother’s death was this problem alleviated.

Let me finish this sketch by describing an instance of Dima’s generosity. By 1971 I and my wife Svetlana had two children and we lived in a single room (about 220-240 sq. ft.) in a communal apartment with two more families and the fourth room occupied until 1970 by Svetlana’s grandparents and idle after her grandmother’s death and grandfathers’ move with her parents. That was obviously inadequate, even more so since I grew up in a single-family apartment and was accustomed to better living conditions. So we started to look for a co-op apartment. Although those were built by various enterprises, leftovers, that could not be filled by the employees, were available to the general public. There were also restrictions that prevented most people, even those who had money, from buying larger apartments. Fortunately a Ph.D. holder had a considerable extra allowance so our family was eligible for approximately an 750-800 sq. ft apartment. And larger apartments were often left out due to administrative restrictions and cost so we were able to quickly find a decent apartment of about that size. But then money became an issue. Downpayment was strictly fixed at 40% and that amounted to 4500 rubles or one-and-a-half year gross earnings of myself and my wife at the time. After a relatively routine promotion that was expected in a year or two that would go down to just my own gross earnings for the same period. We had no savings to speak of and neither Svetlana’s parents nor my mother could help us. While mortgages existed and the rate for remaining 60% of the cost was quite low (we were able to afford monthly payments) there was no way to borrow money for the

downpayment, and besides, there were no realistic chances of repaying, even the principal, within several years.

So we pondered this problem. I had an aunt who had considerable savings and who obviously loved me but I could not approach her with a request of that magnitude. At the end I asked and received from her 1000 rubles, nominally as a loan. We were quite close with Anosov at the time and once when he visited us in our room we started to discuss the issue in his presence not having in mind to ask him for anything. We were astounded when Dima offered to borrow the whole amount from him, naturally without interest and with an indefinite term of repayment. Needless to say, we accepted and later decreased the amount by borrowing 1000 rubles from my aunt. The fact is that, had we stayed in the USSR, we would probably have not been able to repay the money before serious inflation started. As it was, we repaid in full from the money we received after selling our apartment before leaving the USSR in 1978.

So what was the reason for such an extraordinary generosity? Yes, we were friends but our friendship at the time was only three years old and our closeness was considerable but still not very great. I believe the answer is this: in Anosov's value system the welfare and comfort of a family he cared about stood much higher than this amount of money for which after all he did not have an immediate use. And the love of money as such was completely alien to him.

## 2. Mathematical legacy

### **Chronology of Anosov's principal publications and his expository and historical writing.**

Control theory and averaging in ODE: three papers in 1959-60 and a paper in 1996.

Hyperbolic dynamics: Six works (including a monograph) published in 1962-70 although they mostly cover work done before 1964-5.

Approximation in smooth dynamics: four papers in 1970-74.

Various aspects of geodesics in Riemannian and Finsler geometry: six papers in 1975-85.

Nielsen numbers: a 1985 paper.

Behavior of lifts of orbits of flows on compact surfaces to the universal cover: twelve papers in 1987-2005.

Return to hyperbolic dynamics: a 1996 paper and five papers in 2010-14.

Anosov's output, especially in his later years, contains a substantial amount of expository, historical and biographical writing. Those range from numerous jubilee articles and obituaries (usually signed by many people), to presentations of some classical topics, to analysis of historical developments, to attempts at broad surveys of recent history. Anosov possessed an excellent and very lucid understanding of many subjects as well as a somewhat peculiar wit and this makes some of his writings very interesting. A good representative example is [7].

As a matter of general fairness to the dynamical community, I feel compelled to comment on Anosov's most ambitious attempts in this genre, two historical surveys [9] and [8] written in his later years. The former gives his view of the "hyperbolic

revolution” of the 1960s and his personal participation in it. While the personal recollections are of obvious interest, the general picture is somewhat distorted and, as I will explain later, not necessarily in Anosov’s favor. Worse, his evaluation of contributions of various people then and later is distorted by omission of some essential names that were well known to him. The long survey [8] that covers the last quarter of the twentieth century suffers from similar deficiencies even more. Selection and even more omissions of names and topics for the survey make a strange impression; see the detailed critical MR review [17]. Evidently, Anosov’s outlook and some of his principles evolved in the post-Soviet period when in Russia he received a lot of recognition and achieved fairly high visibility even outside of the mathematics community (in contrast with restrained reception in the West), and did not have to face moral dilemmas of the previous period.

**Anosov’s contribution to hyperbolic dynamics.** In this paper I will only discuss Anosov’s works on hyperbolic dynamics and approximation in smooth dynamics. One reason is that those works are the most influential among Anosov’s contributions. Another is that they and their immediate aftermath correspond to the period of my close interaction with Anosov described in the first section.

I apologize for patchy character of my bibliography. I tried not to overload this completely non-technical paper with references. In the era of internet in general and Google Scholar and MathSciNet in particular an interested reader can fill the gaps with only moderate efforts.

Anosov’s name is forever connected with hyperbolic dynamics, one of the principal parts of the theory of dynamical systems. Hundreds of papers study and thousands mention Anosov systems, various versions of this notion such as Anosov diffeomorphisms and Anosov flows, as well as later variations: pseudo-Anosov maps, Anosov group actions, and so on. Anosov originally named this object  $U$ -systems, but soon afterwards Smale re-christened them Anosov systems, and this term immediately took off in the English language literature, and in a few years substituted the original terminology in the Russian language publications as well. Is this an accidental luck or are there deeper reasons? I think that popularity of these mathematical objects, and hence their names after their discoverer, is not accidental.

The origins of the modern view of dynamical systems with finite-dimensional phase space can be traced to the works of H. Poincaré at the end of the 19th century. During the first half of the 20th century development of the theory followed several, mostly parallel, courses. Here are key names from that period: J. Hadamard, G.D. Birkhoff, O. Perron, M. Morse, G. Hedlund, A. Denjoy, A.A. Andronov, L.S. Pontryagin, J. von Neumann, E. Hopf. Fundamental progress that led to a new synthesis that created foundations of the modern theory of dynamical systems took place in the 1950s and early 1960s. Its principal elements are two discoveries of Kolmogorov (KAM theory and entropy in dynamics) and “hyperbolic revolution”, as Anosov called it in his historical survey [9]. Retrospectively, that was not that much of a revolution if one properly synthesizes the points of view of Hadamard, Morse, Hedlund and Hopf. Principal actors of this revolution came from different mathematical backgrounds: S. Smale from topology, Anosov from the theory of differential equations, Sinai from probability theory, V. M. Alexeyev from classical mechanics. As is well known, the first impulse came from Smale, as Anosov colorfully describes in [9]. One should not, however, overestimate the role of this

impulse that provided a description of a somewhat artificial example of a structurally stable diffeomorphism that contains the “Smale horseshoe” as an invariant set. Almost immediately the leadership in the “hyperbolic revolution” passed to a group of young mathematicians from Moscow (Alexeyev, Anosov, Sinai; V.I. Arnold also showed lively and fruitful interest). The reason for this (other than obvious talents of the main actors) was that Moscow mathematicians brought to this new area deep understanding and intuition from different areas of analysis, differential equations and probability theory while Smale’s motivation came almost exclusively from topology.

Anosov’s principal contributions to hyperbolic dynamics are contained in his monograph [3] based on his 1965 Doctor of Science thesis and published in 1967. Most of its results, including the main ones are announced in two Doklady notes [1] and [2] published in 1962 and 1963 correspondingly. Notice that the first of these papers that contains, among other things, the structural stability of Anosov systems, was submitted for publication in March of 1962, i.e. less than 6 months after Smale’s famous appearance at the Kiev non-linear oscillations conference that Anosov describes in [9]. Smale at the time had no ideas how to prove structural stability of hyperbolic toral automorphisms, let alone geodesic flows on negatively curved manifolds, while Anosov’s results go far beyond Smale’s structural stability program. So, from my point of view, in his own account, Anosov gives too much credit to Smale and puts his own achievements into the shadow. The reasons for that are not clear to me; while modesty may have played a role, there may have been some additional motives.

Anosov developed principal technical tools of hyperbolic dynamics, first of all, the theory of stable and unstable foliations, and approach based on considerations of  $\varepsilon$ -orbits (pseudo-orbits) and their families. Let me try to describe briefly the essence of these two main discoveries of Anosov in this area.

An Anosov system is characterized by the presence of two invariant sub-bundles of the tangent bundle to the phase space that (in the continuous time case, together with the orbit direction) generate the tangent bundle. Vectors in one sub-bundle (called contracting or stable) are contracted with exponential speed under the time evolution in the positive direction, while in the other one (called expanding or unstable) are contracted with exponential speed under the time evolution in the negative direction. A priori those sub-bundles are not even assumed to be continuous, but continuity, and even Hölder continuity, follow; see [4] for the proof of the Hölder property. On the other hand, even for infinitely differentiable or analytic systems they are not even  $C^1$ .<sup>10</sup>

Still, those sub-bundles are uniquely integrable to two foliations with smooth leaves that, however, often do not change in a differentiable way in the transverse direction. Their integrability follows from classical results of Hadamard and Perron (Anosov emphasizes that); and, at least in the analytic case, can be traced even farther back to even earlier work of Darboux, Poincaré and Lyapunov. Existence of these foliations is very useful for the investigation of topological properties of Anosov systems, including structural stability. However, for the study of more subtle analytic as well as ergodic properties, the absence of differentiability, from the

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<sup>10</sup>While the  $C^1$  property holds for large open sets of Anosov systems,  $C^2$  is already highly exceptional and related to phenomena of rigidity; Anosov pioneering insight in this direction is contained in Section 24 of [3].



first glance, presents an unsurmountable obstacle. E. Hopf in [18] proved the ergodicity of the geodesic flows on the surfaces of variable negative curvature.<sup>11</sup> In this case stable and unstable foliations are constructed geometrically as horocyclic foliations. Hopf used the fact that the horocyclic foliations are  $C^1$ .<sup>12</sup> This is also true in higher dimension if the curvature is “pinched”: the ratio of the minimal (largest in absolute value) curvature to the maximal one is strictly less than 4. Otherwise the horocyclic foliation on negatively curved manifolds of dimension greater than 2 are usually not  $C^1$ . The proof of ergodicity of geodesic flows on such manifolds was one of the principal goals of Anosov’s work. He discovered a property that holds in this case and is sufficient for applicability of the Hopf argument, and hence allows to prove ergodicity. In a somewhat simplified way, the property of absolute continuity of foliations, discovered and proved (for general Anosov systems) by Anosov, states that the holonomy map between two nearby stable leaves along the unstable (weak-unstable in the continuous time case) foliation is absolutely continuous with respect to Lebesgue measure. Here the weak-unstable foliation is obtained by integrating jointly the unstable sub-bundle and the orbit direction. This property of absolute continuity and its various versions and generalizations played a central role in the development of the hyperbolic dynamics in the last half-century.

Anosov’s second discovery provides a convincing explanation of “chaotic” behavior of trajectories in an even more general class of systems with hyperbolic behavior than Anosov systems. I am talking about hyperbolic sets introduced by Smale, where existence of contracting and expanding sub-bundles is postulated not for the whole phase space but only on a closed invariant set. To avoid non-essential technicalities I will discuss the discrete time case only. If in such a system one can find a sequence of points (not even in the hyperbolic set itself but in its small neighborhood) such that every subsequent point is close to the image of the previous one (such a sequence is called an  $\varepsilon$ -orbit or a pseudo-orbit) then close to this sequence there exists a unique genuine orbit of the system. Furthermore, if a family of  $\varepsilon$ -orbits, naturally parametrized and continuous by elements of a topological space then the corresponding family of orbits is also continuous in that topology. This principle of shadowing is one of the most important, if not the most important organizing principle of hyperbolic dynamics. It directly implies the widely known Anosov closing lemma, structural stability, existence of Markov partitions and many other things. This principle was present implicitly in [3], was explicitly formulated in [5], became the central element of the fundamental series of papers by R. Bowen (1947-1978), the most brilliant representative of the Smale school, was made the centerpiece of the presentation of the hyperbolic dynamics by the author [19] from where it found its way to [23] that became a standard text.

Early influence of Anosov’s ideas can be seen both from the development of the Smale school and from work of such outstanding mathematicians as Ju.Moser [27] and J.Mather [26] who interpreted and developed some of those ideas.

Two mathematicians of the next generation, who played a central role in the development of hyperbolic dynamics, M. Brin and Ja.Pesin, were joint students of

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<sup>11</sup>For the constant negative curvature case ergodicity had been proved earlier by Hedlund using geometric methods.

<sup>12</sup>His method that is justifiably known as the “Hopf argument” still plays the central role in the study of ergodic properties of various classes of systems with hyperbolic behavior.

Anosov and the author. Their work is foundational for two major directions of hyperbolic dynamics that continue as active research areas to this day: partially hyperbolic dynamics [13] and non-uniformly hyperbolic dynamics [30] which is often called the “Pesin theory”. Anosov’s role in their mathematical development included strong conceptual influence, constructive criticism and editing of their work, as well as great help at early stages of their mathematical careers mentioned in the first section that was highly non-trivial in the complicated and unfriendly environment of 1970s.

Interestingly, the most important direct follower of Anosov was Smale’s student J. Franks. In his thesis [16] he proved basic results about global structure of Anosov diffeomorphisms and formulated a program for their further study that greatly influenced subsequent work on the subject. Important early contributions to the realization of this program are due to S. Newhouse [28] and A. Manning [25]. The problem of global topological classification of Anosov diffeomorphisms remains one of the most interesting open problems in the theory of dynamical systems. In my 2004 Berkeley-MSRI lecture I listed it among “Five most resistant problems in dynamics”.

Brin and Pesin found important applications of their general work on partially hyperbolic systems but otherwise the area lay dormant for a while, Twenty years after the pioneering work of Brin and Pesin [13] the next big development in the theory of partially hyperbolic systems appeared in the work of C. Pugh, M. Shub and their students among whom A. Wilkinson stands out. Notice that Anosov’s concept of absolute continuity plays the central role in these developments. This has become a major research area with many outstanding practitioners. Some of the top names are M. Viana, F. Rodriguez Hertz and Wilkinson.

Non-uniformly hyperbolic dynamics developed by Pesin immediately attracted great attention. Among the early work there are important papers by D. Ruelle, R. Mañé, Pugh and Shub, M. Herman, A. Fathi and J.-C. Yoccoz

This area remains one of the central in the theory of dynamical systems and listing even most important papers will take too much space.

Smooth ergodic theory deals with ergodic properties of smooth conservative dynamical systems on smooth manifolds (usually compact) that preserve volume or, more generally, an absolutely continuous measure. Anosov made two major contribution into this area. The first of them is a description of ergodic properties of Anosov systems preserving a smooth measure. The foundation of this work is a deep analysis of properties of stable and unstable foliations that were discussed above. Those properties imply that in the discrete time case an Anosov system is a  $K$ -system and, by using later results of Ornstein and his school, also a Bernoulli system. In the continuous time case, Anosov’s result is even more remarkable since it connects ergodic properties of Anosov flows with topology. Namely, a volume-reserving Anosov flow either has a continuous spectrum or is a suspension over an Anosov diffeomorphism  $f$  defined over a certain global smooth transverse section  $S$ . Here suspension is understood as in algebraic topology, i.e. the return time to the section  $S$  is constant. Using the terminology of the theory of dynamical systems, this is the special flow of the diffeomorphism  $f$  with a constant roof function. This is the famous “Anosov alternative” that, in particular, implies that any eigenfunction of an Anosov flow is smooth. As in the case of discrete time, an Anosov flow with continuous spectrum is a  $K$ -flow and a Bernoulli flow. Anosov’s approach

to the study of ergodic properties of conservative dynamical systems based on the investigation of deep and subtle properties of stable and unstable foliations is the foundation of several central directions of this area that greatly developed in the last 50 years.

**Approximations in smooth dynamics and ergodic systems on arbitrary manifolds.** Anosov's second contribution to smooth ergodic theory is of a different nature. Beginning from the appearance of ergodic theory as a method of analysis of classical dynamical systems in the early 1930s and till the end of 1960s ergodic properties had been successfully studied only for a limited class of systems of algebraic origin (translations and linear flows on the torus, nil-flows, horocycle flows on surfaces of constant negative curvature, and so on), for some systems that are closely related to those algebraic systems (e.g. flows obtained by a time change in a linear flow on a 2-torus), and, of course, for Anosov systems. In all these cases, topology of the phase space is rather special. For example, there were no methods of constructing area-preserving ergodic diffeomorphisms on the 2-dimensional sphere or the 2-dimensional disk. This problem was solved in our joint work with Anosov [11] that was mentioned in the first part of this article. We developed a new method of constructing systems with interesting, often exotic, ergodic properties on every manifold that allows a non-trivial action of the circle, i.e. the compact group  $\mathbb{R}/\mathbb{Z}$ . As I described above, the original highly unusual idea of this method that does not have analogues in dynamics or in analysis, was suggested by Anosov. Later both authors developed applications of this method that became the central tool in the study of so-called Liouvillean phenomena in dynamics. Anosov [6] published a proof that every manifold of dimension greater than 2 admits a volume-preserving ergodic flow. Results like that require another ingredient that allows to pass from some simple manifolds that allow a circle action to arbitrary manifolds.

For a brief description of the method we follow [15]. Let  $M$  be a differentiable manifold with a nontrivial smooth circle action  $\mathcal{S} = \{S_t\}_{t \in \mathbb{R}}$ ,  $S_{t+1} = S_t$ . Every smooth  $S^1$  action preserves a smooth volume  $\nu$  which can be obtained by taking any volume  $\mu$  and averaging it with respect to the action:  $\nu = \int_0^1 (S_t)_* \mu dt$ . Similarly  $\mathcal{S}$  preserves a smooth Riemannian metric on  $M$  obtained by averaging of a smooth Riemannian metric. Fairly representative models are rotations of the disc around its center and of the two-dimensional sphere around any fixed axis that preserve Lebesgue measure.

Denote by  $C_q$  the subgroup of  $S^1$  with  $q$  elements, i.e the  $q$ th roots of unity.

Volume preserving maps with various interesting topological and ergodic properties are obtained as limits of volume preserving periodic transformations

$$(2.1) \quad f = \lim_{n \rightarrow \infty} f_n, \quad \text{where } f_n = H_n S_{\alpha_{n+1}} H_n^{-1}$$

with  $\alpha_n = \frac{p_n}{q_n} \in \mathbb{Q}$  and

$$(2.2) \quad H_n = h_1 \circ \dots \circ h_n,$$

where every  $h_n$  is a volume preserving diffeomorphism of  $M$  that satisfies

$$(2.3) \quad h_n \circ S_{\alpha_n} = S_{\alpha_n} \circ h_n.$$

Equivalently,  $h_n$  has to commute with the action of the finite group  $C_{q_n}$ . To achieve that one maps a fundamental domain for this group to another fundamental domain

(e.g. to itself in the simplest case that already leads to highly non-trivial results) and then extends the diffeomorphism periodically on the rest of the space.

Usually at step  $n$ , the diffeomorphism  $h_n$  is constructed first, and  $\alpha_{n+1}$  is chosen afterwards close enough to  $\alpha_n$  to guarantee convergence of the construction. For example, it is easy to see that for the limit in (2.1) to exist in the  $C^\infty$  topology it is largely sufficient to ask that

$$(2.4) \quad |\alpha_{n+1} - \alpha_n| \leq \frac{1}{2^n q_n \|H_n\|_{C^n}}.$$

The power and fruitfulness of the method depend on the fact that the sequence of diffeomorphisms  $f_n$  is made to converge while the conjugacies  $H_n$  diverge often “wildly” albeit in a controlled (or prescribed) way. Dynamics of the circle actions and of their individual elements is simple and well-understood. In particular, no element of such an action is ergodic or topologically transitive, unless the circle action itself is transitive, i.e.  $M = S^1$ . To provide interesting asymptotic properties of the limit typically the successive conjugacies spread the orbits of the circle action  $\mathcal{S}$ , and hence also those of its restriction to the subgroup  $C_q$  for any sufficiently large  $q$  (that is of course will be much larger than  $q_n$ ) across the phase space  $M$  making them almost dense (Anosov’s original idea, when he invented this scheme; he took just one  $\mathcal{S}$  orbit at each step), or almost uniformly distributed (my first improvement; here one needs to control a majority of orbits simultaneously), or approximate another type of interesting asymptotic behavior. Due to the high speed of convergence this remains true for sufficiently long orbit segments of the limit diffeomorphism. To guarantee an appropriate speed of approximation extra conditions on convergence of approximations in addition to (2.4) may be required.

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DEPARTMENT OF MATHEMATICS, THE PENNSYLVANIA STATE UNIVERSITY, UNIVERSITY PARK, PENNSYLVANIA 16802

*E-mail address:* [axk29@psu.edu](mailto:axk29@psu.edu)

*URL:* <http://www.personal.psu.edu/axk29>