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MOSCOW DYNAMICS SEMINARS OF THE NINETEEN SEVENTIES AND THE EARLY CAREER OF YASHA PESIN

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Introduction. This article has an eclectic nature. Its appearance at the beginning of this volume is justified by the personality of the main character. But this is not a scientific biography. My goal was to draw the picture of the milieu which surrounded Pesin and stimulated or obstructed his development as a mathematician, bringing the story to the moment he did his earliest work of great significance and describing reception of this work abroad and at home. Inevitably this slips into the genre of personal recollections, although I also record certain episodes communicated to me by other people, which I either was never aware of or have forgotten.

I believe that the crucial element of this milieu was the seminar which D. V. Anosov and I ran throughout most of the nineteen seventies. To put this into a broader perspective I start with a brief description of the mathematics seminar culture at the time and a more detailed discussion of two dynamics seminars: The Sinai–Alexeyev seminar at the Moscow State University and the one mentioned above, with a considerably more space devoted to the latter which from now on I will usually call simply "the seminar". After this exposition I turn to early stages of Pesin's career which were practicably inseparable from those of his life-long friend and early collaborator Misha Brin. Then I return to the seminar as a medium when mathematical development of both Brin and Pesin took place. After that my exposition becomes a bit more technical, at least partially: I try to describe the genesis, outline the contents and indicate the importance of two early bodies of work, one joint by Brin and Pesin and the other by Pesin alone. I do maintain another focus on the background and various circumstances of both favorable but primarily unfavorable nature. The last two subsections are again more narrative but they also contain some elements of analysis and illustrations of the impact of Pesin's work.

The 1960s and 1970s were the golden age for dynamical systems[1] in Moscow. The beginning and first flourishing of Yasha's mathematical career coincides with the second part of this period. He entered the School of Mechanics and Mathematics[2] of the Moscow State University (mech-mat MGU or simply "mech-mat", as we will call it from now on, following the abbreviated Russian usage) in the fall of 1965,

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[1] As well as for many other areas of mathematics.

[2] We translate Russian word "fakultet" as "school"; this translation is more accurate that often used "department". Fakultet is a large self-contained unit headed by a dean.

graduated in 1970, produced his first significant work in 1972-73, and the landmark work on the non-uniformly hyperbolic dynamics during 1974-77. This, together with the fact that I left the Soviet Union for good in February of 1978, makes the decade 1968-1977 a natural focus of this account.

A number of people have helped me significantly. Misha Brin, Boris Hasselblatt and Svetlana Katok read the paper carefully and critically and helped both with general comments and criticisms which led to improvements of the structure and presentation, and with corrections of numerous inaccuracies of various kinds. Brin's contribution stands out as particularly thorough with great attention both to many substantive points and minutest details. Valuable information about the seminar came first of all from D. V. Anosov, and also from V. Afraimovich, Brin, L. Bunimovich. R. Grigorchuk, M. Jakobson, A. Kochergin. Yasha Pesin also shared some of his recollections. Needless to say, I bear full responsibility for possible factual mistakes or inaccuracies as well as for all opinions and evaluations which some people familiar with various aspects of the subject matter may find objectionable or controversial.

1. **Moscow dynamics seminars.**

1.1. **The seminar culture.** Research seminars were at the heart of Moscow mathematical life of the period. As a rule the seminars were completely informal, the only fixed parameters being the seminar leaders, place of meetings and the days and starting time: almost always once a week and in the great majority of cases in the afternoon or evening. As is well known (and sometimes exaggerated), the end time was not firmly fixed although the standard duration was usually designated at two hours.

Membership of most seminars was fairly fluid. It usually included current graduate and upper division undergraduate students working with seminar leaders but at a typical major seminar those were usually in the minority. Former students, colleagues, students from adjacent fields made up the rest. It is important to remember that either directly after five years in the university or after additional three years in the graduate school, a very large number of mathematicians beginning their research careers took jobs in organizations not directly related with mathematical research or with teaching mathematics. Thus participation in the seminars was the vital link, the lifeline connecting those people with the world of mathematics. They had no obligations to come or speak, their mathematical work was usually not appreciated and often looked upon with suspicion at their places of employment, but they persevered and many of them kept producing first rate mathematical research for several decades. Yasha (Yakov Borisovich) Pesin is an outstanding example of a highly successful and productive mathematician who labored under these conditions from his graduation in 1970 till his emigration in 1989.

Some of the Moscow seminars became legendary, the famous Monday evening seminar run by I. M. Gelfand for nearly four decades holding the undisputed first place in this respect. The great algebraic geometry seminar associated with the names of I. R. Shafarevich and Y. I. Manin was the focus of activities in the area for several decades and had a decisive influence on the formation of several generations of outstanding mathematicians. In the seventies there were several other very popular and influential seminars, for example V. I. Arnold's seminar centered around singularity theory but with a very broad scope. The dynamics seminars which I will discuss had a somewhat lower profile but for two decades they were

arguably at the forefront of the world development in dynamics and had quite a considerable impact on both setting the agenda and pushing the frontiers of research in the field.

1.2. **The MGU and Steklov/CEMI dynamics seminars and leading personalities.** In the sixties the principal venue for dynamics (as we understand it now) in Moscow was the seminar at the MGU run by Ya. G. Sinai and V. M. Alexeyev. In the first half of the decade V. I. Arnol'd, then a rising superstar of the Moscow mathematics, worked in dynamical systems, primarily in the area of stability of motions, which became known as the KAM theory. He had an active dynamics seminar during the first half of the period which attracted some of the most promising students from several classes immediately preceding mine. It is quite surprising, in light of Arnol'd's later spectacular successes with students and other young mathematicians working in singularity theory and algebraic geometry, that not much came out of this earlier crop of Arnol'd's students. In any event, after Arnold's memorable visit to France in 1965-66 the principal focus of his interests and the direction of his seminar shifted from dynamics to singularity theory and algebraic geometry.

I started to attend the Sinai–Alexeyev seminar during 1963-64 academic year and continued regularly till 1970 and then intermittently till 1977. Participation in that seminar played a crucial role in my formation as a research mathematician. I hope to write some day detailed recollections of the dynamics in Moscow in the 1960s and this seminar in particular. I owe a lot to both seminar leaders.

Sinai was my adviser for both diploma (approximate equivalent of a M.S.) and the Candidate of Sciences (Ph.D.). He was by far the most dynamic and creative personality in the Moscow dynamics community at the time[3] and, with one notable exception, all leading Moscow dynamicists of my generation were his students.[4]

Alexeyev, several years older than Sinai, and much more low-key, became my close personal friend despite a dozen years of age difference. He was a model of a Russian intellectual in the best sense of the word. His mathematical achievements were formidable and laid the foundation for a fruitful synthesis of the classical celestial mechanics with modern (mostly hyperbolic) dynamics. Not being an open dissident, he maintained the highest standards of integrity in his public behavior and paid a price for that in the form of denied and delayed promotions, biting criticism from the Communist Party committee and difficulties in getting permissions for even very modest foreign trips.

Both Brin and Pesin attended the Sinai–Alexeyev seminar regularly during the 1967–1970 period when they were still undergraduates and both of them spoke at the seminar on a number of occasions during the seventies.

In the fall of 1969 D. V. Anosov and I started another seminar which met at the Steklov Institute or MIAN (Mathematical Institute of the USSR Academy of Sciences) till 1975 and later moved to CEMI (The Central Economics–Mathematics Institute of the USSR Academy of Sciences). I will sometimes call the Steklov Institute

[3]This should be qualified if one considers Arnol'd as a dynamicist. Arnol'd certainly possessed lots of flair and charisma on top of his formidable talents.

[4]B. M. Gurevich, V. I. Oseledets, M. E. Ratner, A. M. Stepin (whose other adviser was F. A. Berezin), G. A. Margulis (who already then was more than a dynamicist); the exception is M. V. Jakobson, a student of Alexeyev.

simply "Steklov" as an approximation of the Russian informal name "Steklovka". The story of this move is quite amusing and instructive and I will tell it later.[5]

During most of the seventies the two seminars had comparable influence; some of the topics overlapped, others were particular to one of the two. Pesin's work and development during this period is inseparable from the Steklov/CEMI seminar and, given the subject of these notes, it is natural to concentrate on its activities.

The core participants of the seminar were M. I. Brin, Pesin, A. V. Kochergin, two Anosov's students A. A. Blokhin and A. B. Krygin, A. A. Gura who worked with me, later E. A. Satayev. My wife Svetlana Katok, M. V. Jakobson and B. S. Pitskel, who was a student of Gurevich, and R. V. Plykin came most of the time; G. A. Margulis, E. I. Dinaburg, Yu. I. Kifer, R. I. Grigorchuk, and Alexeyev's students L. D. Pustyl'nikov and T. V. Lokot' showed up from time to time. More senior mathematicians, such as A. M. Stepin and V. I. Oseledets, came as speakers and occasionally as listeners. Several other young people participated regularly for extended periods of time.

While visitors from outside Moscow were not a very common occurrence those days, there was one notable exception. The school of qualitative theory of ODE in Nizhnii Novgorod (then called Gorkii) going back to the great radio-physicist A. A. Andronov and his brilliant collaborators from the 1930s and 1940s, was revitalized during the late 1960is and made important contributions to the development of the modern theory of dynamical systems, including hyperbolic dynamics, bifurcation theory and theory of flows on surfaces of higher genus. While E. A. Leontovich-Andronova was the nominal head of this group and the teacher of several brilliant young people, it was L. P. Shilnikov, a contemporary of Anosov, who became its real scientific leader. Although Shilnikov never spoke at the seminar, visits of Gorkii mathematicians, S. Aranson, V. Afraimovich, V. Z. Grines and L. Lerman, were quite frequent. Some of them spoke at the seminar, others came to listen and exchange ideas, and all of this contributed in a serious way to the agenda and vitality of the seminar. To avoid misunderstanding I need to mention that contacts between Moscow and Leningrad, the other major mathematical center in the Soviet Union, were regular and fruitful and included mutual visits by dynamicists but to the best of my recollection visitors from Leningrad did not show up at the seminar too often.

The mode of operation of the seminar was a mixture of presentation of the participants' own work, often not written yet even in a draft form, and presentation of works by other mathematicians, mostly foreigners, very often based on preprints which were quite rare and had a great value at the time. Anosov was one of the few people in Moscow to whom foreign colleagues sent their preprints due to his recognized status and to his few travels to the West. Later in the period I also started receiving some preprints.

The talks of the second kind were usually not so different from what people are accustomed to nowadays. Dissemination and good absorption of the "Western" (primarily the Smale school) approach to hyperbolic dynamics in Moscow was greatly helped by able presentation of the key work at the seminar. Anosov, being an outstanding topologist, played a great role in this process. Although the main early Moscow text on differentiable dynamics, the triple set of lecture notes [1], does not bear his name, my part of it featuring a systematic presentation of hyperbolic

[5]The 1969 starting date was given to me by Anosov. I firmly remember that the seminar worked at the Steklov at least from the Fall of 1970; possibly it met at the mech-mat MGU during the 1969-70 academic year. Brin's recollections agree with mine.

dynamics grew from both studying some of the Western work and from filling the Anosov blueprint [2].

Talks featuring original work were more various in form and often deviated from the standard model to a much greater degree. They also tended to be quite long, lasting for three and even four hours. In a somewhat simplified form here is the difference. Unlike a standard talk where both the speaker and the audience assume from the beginning that the results and arguments are correct and the speaker's goal is to convey this truth to the audience, here the audience at the beginning was usually not convinced of the truth of the arguments and the speaker's main task was to convince his listeners. On top of this, if the speaker does not have a carefully written draft, he may be swayed by the doubts of the audience or at least his conviction in the truthfulness of the arguments may be dented. Another possible scenario is that the audience does not get in tune with the thinking of the speaker and, while not directly challenging the arguments, somehow fails to follow the speaker. This of course happens at the talks of the standard contemporary format but usually the speaker goes forward anyway and the audience lets him do that, out of general politeness and respect for the truth which can be extracted from reading. At the Moscow seminars I am describing, the outcome was often different: the audience, unable or unwilling to follow the arguments, would basically block the speaker's efforts to move forward whether genuine doubts of the correctness of the arguments existed or not. While my more vivid recollections of the events of that kind are related to the Sinai–Alexeyev seminar, things like that happened at our seminar too. One episode of this kind is remembered by Grigorchuk and it happened at his first attempt to present his early work which later turned out to be quite influential.

1.3. **D. V. Anosov.** During the late sixties and the seventies Dmitry Viktorovich Anosov played a very special role in the dynamics community in Moscow and to a large extent in the whole of USSR. In particular, he was the most senior and best educated mathematician among the people involved with our seminar. He was also the only one with a serious "official" standing within the mathematical establishment. In fact, his position was more solid and secure than those of Sinai or Alexeyev even though both of them had regular jobs within the MGU.

Anosov came from a family of Russian "high intelligentsia", both of his parents were well recognized researches in chemistry. It was clear that the family adhered to very high moral standards, both personal and professional. The family lifestyle was distinguished by asceticism which I, with my Soviet version of something resembling an "upper–middle class" upbringing, found striking. I heard that Anosov's parents used to support poor students and other destitute people and I can personally attest that Anosov was extremely high–minded and generous in financial matters.

Although I was Anosov's junior partner, I was primarily responsible for setting the seminar agenda. My own work during the period proceeded apace which of course helped to inspire younger seminar participants. Anosov was very far from being a figurehead though. He was brilliant and quick, and possessed a very perceptive and critical mind. Everybody around him, including myself, greatly benefited from his comments, criticism, and help with pointing out and correcting errors. Nowhere was this more apparent than in Anosov's interaction with Brin and Pesin.

Anosov also provided invaluable help to younger participants as a link to mathematical establishment. Under normal circumstances for brilliant young mathematicians such as Brin and Pesin the quality of their work would have been sufficient

D. V. Anosov, 1977

to guarantee a smooth and steady career advance. But the circumstances were anything but normal. They worked under a double handicap.

First, there was a general albeit informal discrimination against Jews.[6] Discrimination primarily showed itself in denial of admission to various desirable career paths, promotions and other "privileges", such as travel abroad. Thus it was mild by historical standards but was nevertheless acutely felt by the overwhelmingly secular highly assimilated Soviet Jews who could find no consolation in the traditional forms of Jewish existence. There were great variations among professional fields and administrative structures in the level, comprehensiveness and deviousness of anti-Semitic policies, and administrators and other influential people had various degrees of leverage. Anosov used whatever influence he had on publication of papers and granting academic degrees to promote high professional standards, enforce criteria based on quality and originality of work and mitigate the effects of discriminatory policies.

The second handicap, which was mostly a consequence of the first, was that these young people where outside normal professional structures. Brin and Pesin did not enter the graduate program called "aspirantura" after graduation (for reasons explained below) which was considered the normal path toward the scientific degrees and an established status in the professional community. Neither did they work in institutions where mathematical research could be incorporated into their regular activities such as leading institutes of higher education[7], or some elite research institutes of the Academy of Sciences, such as CEMI. Thus they needed more moral encouragement as well as various kinds of support to carry out their work, have it published and obtain the Ph.D. I was able to provide lots of the former (sometimes

[6] Jewishness was understood in a purely ethnic sense: it did not matter much whether a person kept any element of Jewish observance or had any Jewish self-consciousness, or even would be considered a Jew by Jewish law, i.e., had a Jewish mother.

[7] Most of higher education establishments in the Soviet Union were specialized, often quite narrowly. "Institute" was the standard name for such places, e.g. "Moscow Institute of electronic machine building" which has one of the best mathematics programs and a number of distinguished mathematicians on its faculty.

my actions verged on irresponsibility) but it fell on Anosov to deal with the latter. I will mention certain specific episodes later.

1.4. **Seminar topics.** Here are several original topics which were developed and presented at the seminar. I tried to list earlier topics first but the order is definitely not strictly chronological since no records with exact dates have been preserved. Those marked with the asterisque will be mentioned later. Each topic normally encompassed a series of talks, often several series as new progress was achieved.

- Construction of ergodic diffeomorphisms on arbitrary manifolds. (Anosov, A.K.)
- * Partially hyperbolic dynamical systems, complete non-integrability and stable transitivity. (Brin and Pesin)
- Group extensions of Anosov systems and frame flows on manifolds of negative curvature. (Brin)
- Area preserving flows on surfaces of higher genus, fundamental class and smooth classification. (A.K.)
- Sharp estimates for the number of ergodic invariant measures for flows on surfaces. (Sataev)
- Billiards with focusing components and stochastic behavior. (Bunimovich)
- Existence of ergodic flows on surfaces. (Blokhin)
- Monotone equivalence of measure preserving transformation and flows. (A.K., Sataev)
- Examples of ergodic cylindrical cascades. (Krygin)
- Mixing in time change of arbitrary ergodic flows. (Kochergin)
- Mixing in special flows over interval exchanges and with flows on surfaces with degenerate saddles. (Kochergin)
- Absence of mixing in interval exchanges, special flows and polygonal billiards. (A.K.)
- Topological transitivity in polygonal billiards. (A.K.)
- * Lyapunov exponents and stable manifolds for general smooth dynamical systems. (Pesin)
- * Formula for entropy and π-partitions for general smooth dynamical systems. (Pesin)
- * Smooth dynamical systems with non-vanishing Lyapunov exponents: local ergodicity and Bernoulli property. (Pesin)
- * Geodesic flows on manifolds of non-positive curvature. (Pesin)
- Closed geodesics on Finsler manifolds. (Anosov)
- Bernoulli diffeomorphisms on arbitrary surfaces. (A.K.)
- Detailed analysis of the structure of Markov partitions and finiteness theorems. (Jakobson)
- Lebesgue spectrum in homogeneous systems on semisimple Lie groups. (Stepin)
- Systems with narrow Mather spectrum and non-wandering points of Anosov diffeomorphisms. (Brin)

Here are some topics of the series of expository talks.

- Transversality theory.
- Introduction to the foliation theory.
- Hirsch–Pugh–Shub partial hyperbolicity theory.
- Shub entropy conjecture.

- Robbin's proof of structural stability for systems with Axiom A and strong transversality.
- Franks–Newhouse classification of codimension one Anosov diffeomorphisms.
- Takens' examples of C^1 twist diffeomorphisms without invariant circles
- Feldman's examples of nonstandard measure preserving transformations with zero entropy
- Bowen's work on thermodynamical formalism via expansiveness and specification

Most of these presentations stimulated original work by seminar participants or at least attempts to think about related open problems.

1.5. **Move of the dynamics seminar from Steklov to CEMI.** Steklov at the time presented a mixture of the "Ivory tower" and reactionary obscurantism. I. M. Vinogradov, the long–time Institute director and, back in the twenties and thirties, a top number theorist, prided himself on not being in the Communist party and allowed himself certain acts of insubordination such as refusal to sign an academicians' letter against Sakharov. He protected I. R. Shafarevich, then at his dissident stage. On the other hand, Vinogradov's and his associates' anti-Semitism went beyond the general Soviet norms of the time. He was striving to make the institute and the upper echelons of the Soviet mathematics completely "Judenfrei" and, at least within the institute, came close to achieving this infamous objective.

Anosov, one of the up-and-coming young people at Steklov, nominally was just a senior researcher in the department of differential equations led by L. S. Pontrjagin, another formerly brilliant mathematician who became a notorious anti-Semite. A former student of Pontrjagin, considered for a while as his picked successor, Anosov refused to follow the hard line of his bosses, and, short of open rebellion, was sabotaging their agenda with considerable success.[8]

Admission to the building of the Steklov Institute was controlled as was customary in virtually all [9] research and educational establishments in the USSR. A (usually elderly) female attendant sat by the entrance and asked any visitor about his or her business at the institute. Until the mid-seventies the verbal reply "I am going to see such-and-such" (Anosov in our case) was sufficient. Presumably, if a large number of people were expected to come, the host should have mentioned to the attendant beforehand that there would be people coming to see him.

In 1975 a bizarre tragic incident happened. A deranged inventor got into the headquarters of the Academy, entered the office of a mid-level official and after an altercation fatally stabbed and beheaded him. A not unnatural bureaucratic reaction was to tighten admission rules for entering not only the headquarters but the academy institutes as well. In particular, at the Steklov institute an instruction was issued that all outside participants of the institute seminars should be listed in advance, those lists given to the attendant on duty, and identities checked at the entrance.

Up to this point this looks maybe as a bit of an overreaction but benign in its essence, driven by understandable security concerns. But then a twist comes. The lists had to be approved by department heads, in our case by Pontrjagin. As I understand it, by then relations between Pontrjagin and Anosov had become

[8] Anosov did become the department head but already under somewhat different circumstances; he holds this position at present.

[9] But not at the ultra-progressive CEMI, see below.

strained, to a large extent because of Anosov's support for mathematicians of "non-Aryan" origin. Pontrjagin "looked"[10] at the list featuring last names such as Brin, Bunimovich, Jakobson, Pesin,[11] and refused to approve it, thus virtually shutting the seminar down.

At the time I already felt fed up with petty and not-so-petty discrimination and was not in the mood to yield or adapt. So I suggested to move the seminar to the CEMI, located almost next door, where, due to a very different liberal culture, no pre-authorization of any kind was needed. Although it may have affected Anosov's prestige, he agreed and the seminar continued as before at the new location. According to Anosov, he did not frequently attend the seminar during its CEMI phase partly due to various domestic problems, but he kept active communications with Kochergin, Brin, Pesin, and me, as well as with some other seminar participants. The seminar functioned through the Spring of 1977. By the beginning of the Fall semester I already was "v podache", i.e. had applied for permission to emigrate, and the seminar was discontinued. Although I kept my job and many of my CEMI colleagues did not shun me, it would not have been wise for those who chose to stay to participate in any kind of public activity under my guidance.

A. B. Katok, 1976

The liberal and progressive character of the CEMI culture and its leadership has been acknowledged, and in the greater scheme of things the institute and some of its leaders played a significant role in the transformation of the Soviet Union in the late eighties which led to its final dissolution. I would like to add that during the earlier period discussed here, many among the institute's personnel, including a considerable number of distinguished scientists and administrators, left the Soviet Union. Application for emigration, even when granted, was treated by the state authorities as just short of treason. Nevertheless the institute administration mostly kept its cool and stayed away from repressive measures against both those who applied for emigration and against other Jews.

[10] He was blind.

[11] Well recognizable as last names of people of Jewish descent.

2. The rise of Yasha Pesin.

2.1. **At the university and right after.** The standard progress of a mathematics mech-mat student involved selection of a specialty within mathematics (such as probability theory, algebra, functional analysis, etc) and an adviser among the faculty specializing in the chosen field. The student was required to present "course papers" at the end of the third and fourth years at the university and "diploma paper" (equivalent of Masters thesis) at the end of fifth year. Students who expected to continue for three years of graduate studies in "aspirantura" leading to a Ph.D, it the great majority of cases would continue with the same adviser. In fact, in order to be considered for the graduate program, the Diploma adviser was supposed to provide a strong recommendation including his agreement to continue working with the student. On occasions, advisers were not regular or even part-time faculty; for example, I was the official undergraduate adviser for a number of students while working in CEMI and even one of two official Ph.D advisers for Kochergin.

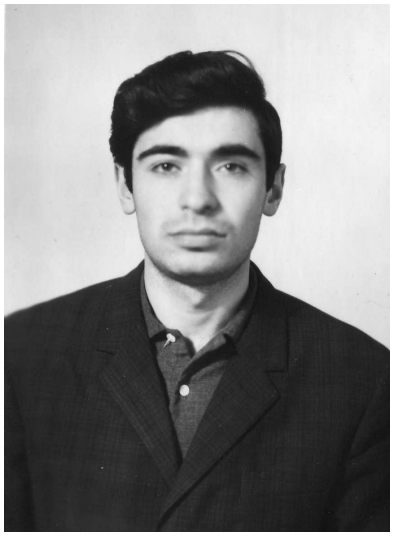
Naturally, among about 250 mathematics students from each mech-mat MGU class only a minority aimed to become research mathematicians but the system was geared towards those who did. And this minority, those who hoped to continue graduate studies and get a Ph.D in core mathematics was considerable, about one-fifth to one-fourth of the total. Sinai was quite popular as an adviser during that period and each year three or four talented research oriented undergraduate students chose him as their adviser. He probably felt that this was too much and on several occasions asked his former students to take over.

Among the class which entered mech-mat MGU in 1965 three students did their third year course papers with Sinai: Brin, Pesin and Kerim Volkovysskii. In the fall of 1968, more or less at the time of my Ph.D defense, Sinai asked me to take over as an adviser for the fourth year course papers of Brin and Pesin. This was the beginning of the life-long professional relationships as well as great personal friendship. Volkovysskii continued to work with Sinai and they published a short joint paper [30]. Hie subsequently published several papers in classical complex analysis.

From the very beginning Brin and Pesin, although assigned different projects, interacted very closely both in learning new mathematics and in their research. It is fitting that the first really major achievement for either of them was the joint work on partially hyperbolic systems announced in [7] and published in full as [8]. This work belongs to the subsequent period and will be discussed in a later section. Brin's diploma work was published as [5].

As I already mentioned, the natural path for a talented mech-mat MGU student after graduation was to enter the three-year graduate "aspirantura" program, usually at the MGU, less frequently at the Steklov institute.[12] During this period students, called "aspirants", were provided with a decent stipend and were not required to teach or take mathematical courses. The only formal requirement in mathematics was a sort of comprehensive exam. The principal goal of aspirantura was to prepare the Ph.D ("Candidate of Science") thesis. Only a small minority managed to submit the dissertation by the end of the third year but many more completed their dissertations within one or two years after finishing the program and taking their first regular jobs. Not everyone by far was able to sustain research

[12] It was common in the Soviet Union to continue graduate studies after the M.S. at the same place.



Ya. B. Pesin, 1970

productivity after taking jobs often not related to mathematical research or teaching. Still the aspirantura system was geared to provide a solid foundation and give a head start to a research career and it succeeded in a great number of cases. Mathematicians with Ph.D's usually obtained better positions, they had higher salaries, faster promotions and were thus better equipped to continue creative work even if they were not paid directly for doing research in mathematics.

Admission to the mech-mat MGU aspirantura, at least during most of 1960s, was primarily based on absolute standards, rather than on competitive rankings. I will not describe here how formal regulations, which aimed at a quite different outcome, were being circumvented. Let me just mention that credit for ensuring that virtually all highly qualified students were admitted and that talent and aptitude for research were the primary criteria, goes to the director of the mathematics graduate division of mech-mat the famed topologist P. S. Alexandrov and his deputy E. A. Morozova.

In 1969 this changed. About forty of the students recommended for aspirantura, virtually all of them Jewish, were given C's on arbitrarily conducted mandatory entrance exams in the "History of the Communist Party"[13] and the Ministry of Higher Education refused to release for them positions officially reserved for outside applicants who existed mostly on paper. Such a release was routinely done before. The action at the exam was the result of an intrigue by party hardliners conducted against the wishes of the university rector I. G. Petrovskii. It was a part of the offensive of the Party committee against the progressive part of the mathematical community during the general reactionary turn of the government domestic policy after the 1968 invasion of Czechoslovakia. However, specific anti-Semitic and anti-intelligentsia actions were not directly decreed by the highest party authorities and a lot depended on the interaction between the local administration and party authorities. In many academy institutes, such as CEMI, more or less progressive or simply tolerant administration controlled the situation and was able to avoid repressive measures and minimize the impact of the general policy changes.

[13]In previous years the exam was conducted basically fairly, and in fact mathematicians attached to examination committees watched over that.

However, at the MGU the party committee got the upper hand, especially with respect to mathematicians. The party hardliners managed to label mathematical community disloyal as a result of a very mild collective protest letter addressed to the Minister of Health and signed by ninety nine mathematicians, a good portion of them from the mech-mat, against forcible detention of one of their colleagues (A. S. Esenin-Volpin) in a psychiatric hospital.

The bottom line for our story is that in 1970 when Brin and Pesin graduated from the university they were not even recommended to the graduate program. Both of them would certainly have qualified by the standards of two years earlier but the rules of the game had changed. I do not know whether Sinai made a serious attempt on their behalf, but in any event the odds were pretty bad.

Brin got a junior position at the economics research institute of the infamous Gosplan, the supreme state planning authority. This was considered a good job. If Brin opted for a Ph.D in economics, a way for advancement and promotions would have been open to him.

Pesin took a job in the less prestigious industrial Institute of Optical and Physical measurements. Both made a decision to tough it out and continue to do mathematics in their spare time.

2.2. **Various jobs for mathematicians.** At this point it is appropriate to take a diversion and comment on the composition and stratification of the Soviet teaching and R & D establishment at the time and how this affected mathematicians.

Both the system of higher education and especially the system of "scientific-research institutes" were overblown. However, teaching establishments absorbed only a small portion of mech-mat graduates. On the one hand, teaching loads for junior faculty were very high (24 hours per week was not unusual) and pay the same, sometimes lower, than in research institutes. This made those jobs unattractive for young people with research aspirations. On the other hand, teaching institutes of higher education, especially those with extensive mathematics programs, had the tendency to employ their own graduates (with or without a home-made Ph.D) creating an inbreeding situation. And those institutions (with few notable exceptions) did not particularly welcome young people from mech-mat who generally had a broader mathematical culture and higher aspirations.

Scientific-research institutes came in many varieties and administratively belonged to many state agencies but outstanding among them were the institutes of the USSR Academy of Sciences. Although the academy, as I will call it for short, was involved in many programs and projects, fundamental research was acknowledged as its primary goal and, conversely, in non-academy institutes, any activity aimed at advancing fundamental science was looked upon with suspicion and required justification and/or concealment. In the institutes of the academy scientific degrees and other tokens of status such as publications in leading scientific journals (even in subjects not directly related to the institute specialization) carried more weight and prestige than in non-academy institutes.

Also the institutes of the academy tended to have flexible requirements for attendance which differed from one institute to another, typically mandating presence for most part of the day for between one and three days per week and in the case of CEMI went to the extreme liberalism of mandating only a token presence.

All these factors created hierarchy of jobs in the minds of many (if not most) mathematicians where pay was not important since the differences were slim anyway, and chances for advancement were often viewed with indifference if not suspicion. On the other hand, different "freedoms" were highly valued: freedom to do mathematics unmolested, freedom from boring everyday routine, freedom from the party and administration intrusion into professional and social life. There was a high correlation between the quality of a mathematician's job from this viewpoint and his (or much less frequently her) success in maintaining high quality research mathematical output for a long time.

Neither the Gosplan Institute nor the Institute for Optical and Physical measurements scored high on this scale, despite the fact that the former did offer opportunities for advancement in the area of economics.

2.3. **At the Steklov/CEMI seminar.** From its very beginning the dynamics seminar at Steklov was not in a usual situation, even by the standards of the time. It was not at all obvious that it would last for long, let alone be successful and influential.

Anosov had a secure and fairly influential position within the mathematical establishment but he did not teach on a regular basis and his access to students was limited.[14] I had my Ph.D and a job which gave me virtually total professional and practical freedom, and my CEMI superiors made it clear that my mathematical work was appreciated and considered a legitimate professional output. I could and did become interested in problems of mathematical economics but those activities never predominated and I was free to dedicate to mathematical research as much time and energy as I saw fit. I was also encouraged to teach both dynamical systems and mathematical economics at the mech-mat part-time. For a while this was allowed by the mech-mat authorities but in 1974 those activities were abruptly terminated in the next phase of the Party offensive. In retrospect it is clear that my fortunate circumstances engendered a not fully justified optimism which influenced Brin and Pesin whose circumstances were quite a bit less favorable. A more objective hard-headed view would probably have discouraged them and terminated their mathematical careers.

Indeed, compared to other seminar participants Brin and Pesin were in the most difficult circumstances.[15] Kochergin was a graduate student at the mech-mat. After graduation he got a teaching job in the MGU School of Economics not just teaching calculus but at the kafedra[16] of mathematical methods in economics headed by a luminary of progressive economic thought, the CEMI deputy director S. S. Shatalin. Jakobson already had his Ph.D and a fairly reasonable job in another institute of the Academy. Bunimovich was also a mech-mat graduate student (he was one of the very few who managed to get a "B" during the 1969 massacre); Blohin and Krygin were graduate students at Steklov. Younger participants were still undergraduates and the university schedule after the second year was quite relaxed.

[14] Anosov held a part-time teaching position at the mech-mat from 1968 till the summer of 1973.

[15] In the case of Pesin this was aggravated by geographical difficulties: He lived pretty far on the outskirts of Moscow and his job was located in another faraway corner of the city. Just coming to the seminar involved many hours of traveling by public transportation and he usually looked very tired.

[16] A subdivision of the school headed by a senior professor, sometimes translated as "chair", but in reality closer in its functions and structure to a department; each faculty member and graduate student was associated with a kafedra.

The plan for Brin and Pesin was to produce good work, publish several papers and to submit their Ph.D dissertations directly; such an option for obtaining the degree, called "soiscatel' ", literally "the seeker", was designed primarily for people who could fashion a thesis from research done on the job. In their case though it was like doing what people normally do in the aspirantura on top of a full time job. The first part of the plan (to produce good mathematics and publish it) succeeded brilliantly; the second (defend the theses) encountered a variety of obstacles which will be mentioned in due time.

2.4. **The work on partial hyperbolicity.** The joint work of Brin and Pesin on partially hyperbolic dynamical systems [7, 8] was the first major achievement for both of them. It had two sources. The obvious one was the landmark work of M. Hirsch, C. Pugh and M. Shub [11, 12] which established partial hyperbolicity as an important paradigm in dynamics. At the time only announcements had been published. A natural question appeared to what extent properties of uniformly hyperbolic systems such as topological transitivity, ergodicity or mixing which had been well understood by the time, could be extended to partially hyperbolic systems. The general idea was to find conditions under which the hyperbolic part somehow dominates the non-hyperbolic one. Obvious counterexamples such as direct products of a hyperbolic system with identity or other non-transitive systems should be excluded. A model was provided by the little known work of R. Sacksteder [29] who in turn had been influenced by the work of Anosov and Sinai [4] on ergodic theory of hyperbolic systems. Sacksteder's criterion for mixing assumed that stable and unstable distributions were smooth and their brackets generated the whole tangent bundle (the total non-integrability for the sum of two distributions). This condition is satisfied in a variety of homogeneous examples including one produced by L. Auslander, a partially hyperbolic automorphism of a three-dimensional nilmanifold, which stimulated Sacksteder.

The weakness of Sacksteder's approach was that even for small perturbations of algebraic systems, stable and unstable distributions lose smoothness. Even their sum is in general not smooth although there are important situations (geodesic and, more generally, contact flows) where it is. In 1971 I found a copy of Sacksteder's paper and we decided to look at the situation when the foliations are not smooth but possess a certain topological property, which was named local transitivity of the pair of stable and unstable foliations; in the smooth case this property follows from total non-integrability. Brin and Pesin showed that for volume preserving systems this property implies topological transitivity of the system and found the first case when this property persists for a natural class of systems. This work was important and turned out to be quite influential in two respects.

First, it provided basic technical tools for extension of ergodic theory from Anosov to partially hyperbolic systems. The key part of this is proof of absolute continuity of stable and unstable foliations for partially hyperbolic systems.

Second, it introduced various versions of the notion of joint transitivity of that pair of foliations. An immediate application was a proof of ergodicity and the much stronger K-property of frame flows for an open set of Riemannian metrics. This was the first result on ergodicity of partially hyperbolic systems beyond the algebraic examples (such as those considered by Sacksteder, or toral automorphisms) and trivial situations such as Cartesian products. Somewhat later, Brin, first alone and later with collaborators, gave virtually complete analysis of ergodic properties of frame flows. He also realized how to use transitivity of foliations to establish *stable*

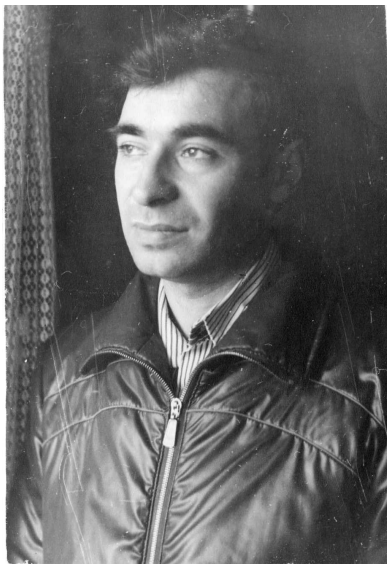
topological transitivity for partially hyperbolic systems. The natural next step, namely *stable ergodicity* had to wait for twenty years till the work of Pugh, Shub and their collaborators.

While Brin and Pesin produced their work under difficult and unfortunate conditions, they at least were fortunate with its publication. This comment requires another diversion. At the time virtually all work by Soviet mathematicians was published in Russian in the Soviet journals. Since leading journals were translated into English cover-to-cover this did not affect dissemination of their work. However, getting the work, especially a longer one, published in a respectable journal was a nontrivial matter and quality of results and their presentation were far from the only criteria. Politics, both general, including the anti-Semitic tendencies, and local, in the form of competition and sometimes even ugly fights between different schools and cliques, played a substantial role. There were three recognized first class journals which published long research papers: Izvestija, Mat. Sbornik and the semi-regular Proceedings of the Moscow Mathematical Society.[17] Control of those venues was divided between two organizations: Steklov (Izvestija and dominant influence in Sbornik) and the Moscow Mathematical Society (Proceedings MMS and junior partner in Sbornik). Although the Society nominally functioned under the aegis of mech-mat it was a remarkably independent and liberal organization with the arrangement (almost unheard-of in the Soviet Union) when the rank-and-file actually influenced both nomination and election of its officials. However, because of volume and regularity of their publication, the two journals controlled by Steklov predominated. Anosov was a member of the Izvestija editorial board and that was one of the places where he was able to promote the merit criteria and oppose the anti-Semitic and clannish policies of some other editors.[18] So for a change Brin and Pesin had no problem having their outstanding work appear in a premier journal.

2.5. **Beginnings of the Pesin theory.** At that time, during the 1973-74 academic year, it looked like Brin's work was proceeding apace leaning toward interface between dynamics and Riemannian geometry. Pesin at the same time had been involved in a couple of projects which resulted in his early publications [16, 17]. I would like to emphasize that at the time the work of these two brilliant young mathematicians was driven as much by certain external, "practical" if you wish, considerations as by the purely intellectual stimuli. This is only natural; they did not have the luxury, provided by aspirantura, to put those considerations on the back burner. Remember that both had full time jobs completely disjoint from mathematical research or teaching, and were not on any of the regular tracks leading to a Ph.D. In order to have a chance for acquiring the degree which would, among other things, make sustaining further work in mathematics more feasible, each of them had to produce a body of work well above the standard degree requirements. Under normal circumstances, say, if they were enrolled in the MGU aspirantura, the work on partially hyperbolic systems would have been more than sufficient for

[17] Uspehi had at least similar, arguably even higher status, but technically it published only surveys (apart from short communications) as is attested by its English name Russian Mathematical Surveys. Some of those surveys were in fact major research papers of broad scope. But those were essentially invited papers.

[18] The Editor-in-chief was Vinogradov who apparently did not intervene much into day-by-day functioning of the board which was effectively run by his deputy Shafarevich. However I remember Anosov telling me that he could publish a 50-page paper (or two) without Vinogradov's approval but not a 100-page one; this is how my monotone equivalence work got truncated and its second part never appeared in the turmoil of my emigration.



Ya. B. Pesin, mid-1970s

two first class Ph.D dissertations. After all, the paper [8], over forty pages long, quite a lot by the standards of the time, appeared in Izvestija, arguably the most prestigious Soviet mathematical journal at the time, a rough equivalent of Annals of Mathematics. In addition they produced other work on the subject. In a somewhat similar situation, but earlier and under more favorable circumstances, Stepin and I defended our dissertations on the same day in 1968, based on the cycle of work on approximations of dynamical systems by periodic transformations, the core of which was joint work and appeared in Uspehi.

So it was decided that in order to guarantee successful defense Brin and Pesin should present two totally separate bodies of work and this undoubtedly influenced the birth of Pesin theory. Before I outline the scope and history of that work let me say that Brin presented his thesis earlier. It contained a joint part on partially hyperbolic systems and his own work on frame flows and stable transitivity and was considered outstanding by all leading experts: Arnol'd who agreed to be an official referee, Anosov who was listed as an official adviser, Alexeyev and Sinai. Support also came from aging Kolmogorov, who at the time did not have direct interest in the subject but formed a high opinion of the work which was shown to him by Alexeyev. Nevertheless, an attempt to have the thesis defended at the mech-mat MGU[19] strongly supported by the people listed above, was derailed by the Communist party hacks helped by some corrupted and unprincipled non-party mathematicians. Maybe I will relate this story in full detail on another occasion. At the end Brin was able to submit and defend this thesis at the University of Kharkov.

Taking into account a great interest around the seminar in various aspects of hyperbolic dynamics[20] as well as successful work on partially hyperbolic systems

[19]Steklov as a place of defense was out of the question due to Vinogradov and Pontrjagin anti-Semitic stand.

[20]One of the fruits of this interest was a collection of surveys and translations of then recent Western papers [3].

it was natural for Pesin to turn his attention to what is now called non-uniformly hyperbolic situation. His first motivation was an extension of ergodic theory for geodesic flows on compact surfaces of genus greater than one from the case of metrics with negative curvature to the more general case of metrics without focal points which also includes metrics of nonpositive curvature. In this case, due to monotonicity of Jacobi vector fields, certain uniform separation of orbits takes place, and the point is to show that apart from infinitesimal flat strips this separation is exponential in some sense. Thinking about this problem Pesin hit on a right notion of nonuniform hyperbolicity which became the starting point of a great development. The centerpiece of the resulting program was derivation of the formula for the entropy of a general classical (smooth volume preserving) dynamical system but the outcome brought much more than the entropy formula. In [13] I told this story in the context of general development of the concept of entropy and its impact in dynamics but it is worth repeating in an abbreviated form.

Soon after Kolmogorov's 1958 discovery of entropy as an invariant of measure preserving transformations it was noticed that for classical dynamical systems, entropy with respect to invariant volume is closely related to the maximal infinitesimal exponential rate of expansion.

The easiest model is provided by an automorphism of an m-dimensional torus which is determined by an $m \times m$ matrix with integer entries and determinant of absolute value one. Entropy is equal to the sum of the logarithms of absolute values of those eigenvalues whose absolute values are greater than one, each counted with its multiplicity. This is exactly the exponential rate of expansion of the volume in the sum of root spaces corresponding to those eigenvalues and this is the maximal rate of expansion of volume in any multidimensional direction.

For a general uniformly hyperbolic volume-preserving (Anosov) system, the entropy is expressed similarly as the integral of the logarithm of the absolute value of the Jacobian restricted to the expanding (unstable) distribution.

A proper generalization of this to arbitrary volume preserving dynamical systems involves the notion of Lyapunov characteristic exponent. The Pesin entropy formula asserts that for a $C^{1+\epsilon}$ diffeomorphism of a compact manifold, entropy with respect to an absolutely continuous invariant probability measure μ is equal to the integral of the sum of all positive Lyapunov characteristic exponents with respect to μ.

But the entropy formula was only one of several and, arguably, even not the most important result of the Pesin work. First, Pesin characterized π partitions as measurable hulls of the partitions into global stable or unstable manifolds thus showing in particular that those measurable hulls coincide. The most important part of Pesin's work deals with the case when exponents do not vanish; such measures are often called hyperbolic measures. He proved that the system has at most countably many ergodic components with respect to an absolutely continuous hyperbolic measure and that on those components the system is Bernoulli, i.e isomorphic to a coin-tossing random process, up to a finite permutation. In particular, weak mixing, mixing, K, and Bernoulli properties are all equivalent. The classification for flows is similar; instead of a finite permutation one must allow for a constant time suspension, but the last statement holds. This established the paradigm of *nonuniform hyperbolicity* which is in principle capable of explaining coexistence of large but nowhere dense sets of invariant tori established by the KAM theory with "chaotic" or "stochastic" behavior on sets of positive measure. It also gave a precise picture

of what this latter type of behavior should look like. This picture is amply supported by a vast body of sophisticated numerical experimentation. Non-uniformly hyperbolic behavior has been established in a variety of situations, some artificial, some model-like, some quite natural and even robust.[21] Thus it is quite justifiable to speak not only of the "Pesin entropy formula" but also about the "Pesin theory". Pesin was the first to study in detail a natural case of partially hyperbolic behavior, geodesic flows on Riemannian manifolds of non-positive sectional curvature and more generally, without focal points [21].

In order to achieve all these remarkable results Pesin had to develop a formidable technical machinery which was partly adapted from earlier work on uniformly hyperbolic systems, partly built from scratch. This included three principal ingredients:

(i) Theory of Lyapunov characteristic exponents based on Oseledets Multiplicative Ergodic Theorem and classical Perron regularity theory,

(ii) theory of invariant stable and unstable manifolds based on development and improvement of classical methods going back to Hadamard and Perron and

(iii) absolute continuity of families of stable and unstable manifolds which required essential new ideas compared to earlier work of Anosov-Sinai for the uniformly hyperbolic case and of Brin-Pesin for the partially hyperbolic case.

2.6. **Pesin's work becomes known.** Pesin's work appeared in print over a period of less than two years in 1976–78 as a series of papers in leading Soviet mathematical journals [18, 19, 20, 21, 22]. These papers established the original contents and the core of what became commonly called "Pesin theory". They, especially the key Izvestija paper on the invariant manifolds theory [18] and the great Uspehi paper [19] with overview of the whole subject and original derivations of some of its central conclusions, quickly attracted attention of the dynamical systems community in the West. I should repeat it again that due to Anosov's principled stand two of Pesin's major papers appeared in Izvestija and two shorter ones in Zametki, another journal controlled by the Steklov Institute where Anosov was on the editorial board. The fifth paper published in Uspehi has the highest profile.[22] In it technical parts presented elsewhere where surveyed and key applications derived. This is the most widely known and most quoted paper of Pesin to this day and arguably one of the most influential and widely quoted papers in the core area of dynamics. Fast publication in high-profile journals which were quickly translated, helped to make Pesin's name familiar in the worldwide dynamics community.

The list of people who within a few years published expositions of various aspects of Pesin theory or made original contributions directly inspired by Pesin's work, reads as the honor roll of the dynamics community at the time: David Ruelle, Michael Shub, Charles Pugh, Ricardo Mañé, Michel Herman, Albert Fathi, Jean-Christof Yoccoz [26, 27, 28, 24, 25, 14, 15, 9, 10]. The term "Pesin Theory" spontaneously came into use as is illustrated by a quotation from Ruelle's paper [27]:

The multiplicative ergodic theorem and the construction almost everywhere of stable and unstable manifolds (Pesin theory) are extended to differentiable dynamical

[21] Still, establishing rigorously coexistence of KAM tori with non-uniformly hyperbolic behavior on a positive measure set remains a completely intractable problem.

[22] While not completely free of factional tensions, the Uspehi editorial board traditionally maintained both high standards and an enlightened broad view of mathematical developments. It is natural and characteristic of its attitudes that Pesin was invited to submit his key work there.

systems on Hilbert manifolds under some compactness assumptions. The results apply to partial differential equations of evolution and also to noninvertible maps of compact manifolds.

Of course, Pesin was not able to lecture about his work outside of the Soviet Union so the task fell on me when I found myself in 1978 first for six months in Western Europe and then in the United States. Brin emigrated in the following year and his stay at IHES certainly contributed to the awareness of the work. In particular, one of the greatest mathematicians of our time, M. Gromov, became interested in the subject and he and Brin published a joint paper [6].

2.7. **Reception of pioneering work of Brin and Pesin at home.** In a somewhat peculiar counterpoint, at home the reception, at least in the short run, was more muted. A direct sign of that is absence of works by other mathematicians following up or directly influenced by the work of Brin and Pesin. There are several factors which contributed to this. Of course due to their position neither Brin nor Pesin could have had students in Moscow at the time. Dynamical community was somewhat divided between the followers of Sinai and those of Anosov. Brin and Pesin naturally fell into the second category while students were at the university where Sinai was the dominating presence. One should mention though that later on Pesin had fruitful collaboration with Sinai. This collaboration produced an important joint paper [23] in the 1982 issue of Ergodic Theory and Dynamical Systems journal dedicated to the memory of Alexeyev who died in 1980. There was also a certain relative decline in the activity in the area in Moscow during the decade following Pesin's work compared to the extremely high level of the previous period. Emigration was one of the factors which contributed to that.

In the broader Moscow mathematical community at the time, the area of dynamical systems was still somewhat under-appreciated. This may sound strange since certainly the Moscow dynamics community was still one of the strongest and most vibrant in the world and its key members then were still in their thirties and early forties. It is nevertheless true that the worldwide infatuation with algebraic topology and abstract algebraic geometry based on heavy use of homological algebra hit Moscow with a certain delay and was still quite strong in the seventies. According to a very distinguished mathematician, now in his forties, who started his career at the very end of the period I am describing, in the mech-mat student's scheme of things, the top spot in terms of prestige was occupied by algebraic geometry, followed by group representations (with very strong algebraic tilt) and areas like dynamics and (quite to my surprise) singularity theory ranked lower.[23] I also believe that since Brin and Pesin were outsiders who could only spare a limited time to mingle with colleagues in the corridors of the mech-mat, theirs were not very familiar faces. Since lots of dissemination of mathematics, even when papers had been published, was verbal through direct personal contact and seminar talks, their mathematics was less widely known at the time than it deserves. I will not be surprised if some of my colleagues find this thesis controversial. Let me illustrate it by an interesting episode.

The Moscow Mathematical Society Prize for young mathematicians carried a considerable prestige, especially with the mathematical community at-large, as opposed to the official authorities. It was awarded by the elected Society Council which

[23] He mentioned specific names of the most desirable advisers which I naturally omit.

represented the cream-of-the-crop of the community in terms of research achievements and international reputation. The prize was awarded for a specific body of, work, jointly if the recognized work was joint, and was subject to the upper age restriction of thirty years for all nominees. The prize usually was given for really outstanding work which produced a strong and lasting impact and was also a good predictor of the winner's long-term success. Usually two prizes were awarded every year.

In 1975 Brin and Pesin were jointly nominated for the MMS prize for the work on partially and nonuniformly hyperbolic systems. Anosov and I were invited as non-voting participants to the council meeting. There were four nominations. One was undisputed: I. N. Bernstein for the work on analytic continuation of generalized functions. By then Bernstein had been recognized as one of the most accomplished mathematicians of his generation in the world and, besides, this was his last chance before reaching the age limit. The work of one of the other candidates was clearly less interesting than the rest. So the choice of the second winner was between Brin and Pesin and the remaining candidate, A. M. Gabrielov. The latter's work was on formal relations between analytic functions connected to questions posed by M. Artin, and while is was quite good, it is clear in retrospect that the choice in favor of Brin and Pesin should have been obvious.[24] Interestingly, not much was said at the meeting about superiority of Gabrielov's work. On the other hand, diverging views arose about the work of Brin and Pesin. Not all members of the Council had sufficiently good knowledge of the subject so it fell on experts or presumed experts to argue the case. Anosov presented a thoroughly reasoned opinion about originality, technical difficulty and impact of the work and I tried to help him. S. P. Novikov, an outstanding geometric topologist and at the time the only Fields Prize winner in the Soviet Union, emerged as the principal opponent. Novikov said that he had not been particularly impressed by the topological aspects of the work on partial hyperbolicity. When asked about his opinion of the "Pesin theory", i.e. the work on non-uniform hyperbolicity, he excused himself due to insufficient familiarity. This was sufficient to tilt the majority of the council toward Gabrielov. My point is that even such a well-educated, powerful and broad-minded mathematician as Novikov (as well as those for whom his arguments carried weight) did not appreciate the most advanced work in dynamical systems at the time.

Pesin was only able to defend his Ph.D in 1979. In his case no attempt was made to submit the work for defense at the mech-mat MGU. Several places outside of Moscow were considered and with great help from Anosov obstacles were overcome in Gorkii. Despite the great number of Jews among local dynamicists, usual problems were present there as everywhere else. Needless to say, the work included into the thesis was head and shoulders above the standard requirements and under more normal circumstances should have easily brought its author the higher Doctor of Science degree,[25] a structural equivalent of habilitation but much more demanding and selective than its European counterparts. Thus the seventies brought Yasha Pesin brilliant success in mathematics and first class international reputation, and a fairly modest improvement in his professional status at home.

[24] As a crude measure, one can look for the statistics of MSN citations for the relevant articles: over 160 for Brin and Pesin and 25 for Gabrielov, whose work also appeared in leading Soviet journals.

[25] In fact, the aged Leontovich-Andronova made this comment in an even sharper form during the defense.

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