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## Title

An interview with Roald Sagdeev: his story of plasma physics in Russia, 1956-1988

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**Abstract**

This oral history interview presents Roald Z. Sagdeev's story of plasma physics in Russia. It chronicles the Russian school's achievements in basic, laboratory, fusion and space plasma physics. The interview begins with memories of Sagdeev's graduate student days in Moscow and then describes his work at the Kurchatov Institute of Atomic Energy (1956-1961), the Budker Institute of Nuclear Physics in Novosibirsk (1961-1971) and the Space Research Institute (IKI) (1973-1988). The interview examines the development of quasilinear theory, collisionless shocks, wave turbulence, instabilities, drift waves, chaos theory, the early stages of magnetic confinement theory and space plasma physics. Sagdeev and his school made seminal contributions in all of these areas, and all are central topics in plasma physics today. Sagdeev also speaks of his collaborations and friendships with notable scientists, such as M. N. Rosenbluth, M. A. Leontovich, L. A. Artisimovich, L. I. Rudakov, A. A. Galeev, V. E. Zakharov, as well as of the political and institutional challenges of this period. The conversation reflects Sagdeev's unique and significant influence in modern plasma theory, Russian space exploration and his support of international cooperation for the advancement of humanity.

## 1 Overview

In this brief overview, Roald Z. Sagdeev discusses aspects of the scientific environment in Russia during the early days of his career. He also speaks of the early history of nuclear fusion. This information complements material found in Sagdeev's 1994 memoir, *The Making of a Soviet Scientist* (MSS) [Sagdeev 1994]. Derived from an exploratory conversation between Roald Z. Sagdeev (R. Z. S.) and Uriel Frisch (U. F.), this section is intended as introductory material for the in-depth interview of Sagdeev by Patrick Diamond (P. D.).

U. F. "Roald, the purpose of the interview is not to write about 'the fascinating scientific life of Roald Sagdeev'. You were selected because you know a lot about the development of plasma research in the Soviet Union, and even when the people involved are now deceased, you frequently interacted with them". U. F. started by explaining why he finds this subject so exciting and how he got involved in it in the early 1960s as a student of the astrophysicist E. Schatzman (the author of the first open paper on the H-bomb in 1950). U. F. then asked R. Z. S. about the role of astrophysics in the birth of plasma research.

After U. F. told R. Z. S. briefly how, as a student of J. Yvon (around 1962) he was approached to join the French H-bomb project and declined, R. Z. S. told U. F. how such matters were handled in the Soviet Union in the early 1950s. Suitable students were not asked if they were willing, but were sent to the Soviet counterpart of the United States' Los Alamos National Lab, called Arzamas 16. There, students met with famous scientists whose names were classified and known only by rumor. These scientists included A. Sakharov and Y. Zeldovich. The students were told that they would receive research assignments under the supervision of a mentor. For R. Z. S., the mentor was D. Frank-Kamenetskii, the first person in this weapons lab to show an interest in astrophysical applications. R. Z. S. was assigned work on stellar opacity and radiation transfer. After a few months, the work was finished. It led to a Soviet "diploma thesis" and was published a few years later in *Soviet Astronomy*.

R. Z. S. noted that a central interest among the scientists of Arzamas 16 was radiation implosion. R. Z. S. did not like the idea of working on subjects too closely related to weapons design. For the most part, he managed to avoid it. He also stressed that an increasing number of students tried to avoid weapons research.

R. Z. S. returned to Moscow University, where he was accepted as a research student by L. Landau. (This was about 15 years after Landau got into trouble with Stalin and was rescued by P. Kapitsa. R. Z. S. pointed out that — even in the mid-1950s — some Marxist circles were attacking Landau and others for carrying out bourgeois science.) Acceptance into the Landau group required taking nine exams [see Chapter 5 of MSS]. Although R. Z. S. expected to become a student of Landau after graduation, a directive signed at the highest government level (perhaps by A. Kosygin) ordered a large number of students to join a newly created weapons lab (Chelyabinsk-70) in the Ural Mountains. This facility was intended to be the counterpart of the Lawrence Livermore Lab. R. Z. S. asked Landau to help him, and after a month or so, a deal was negotiated. R. Z. S. would work in Moscow at the Kurchatov Institute (central to all atomic program work) instead of in the Urals and would also be able to work with Landau. In particular, he would be able to attend the weekly Landau seminars.

R. Z. S. started working at the Kurchatov Institute and was assigned work on controlled fusion rather than on weapons. At Kurchatov, the leaders in controlled fusion were L. Artsimovich and M. Leontovich. Their associates included V. D. Shafranov, B. B. Kadomstev and S. I. Braginsky. R. Z. S. spent five years (1956-1961) at the Kurchatov Institute. See Chapters 7-9 of MSS for R. Z. S.'s description of his stay at Kurchatov and a good description of its controlled fusion research program.

U. F. "Who first did work on controlled fusion in the Soviet Union?"

R. Z. S. "The group of Artsimovich and Leontovich was the biggest one. The design being studied was the Z-pinch".

R. Z. S. explained that before the effort started at the Kurchatov Institute by Artsimovich and Leontovich, plasmas were only being studied as ionized gases, in connection, for example, with gas discharges and ionospheric research (e.g. the work of V. Ginzburg). R. Z. S. stressed that full plasma research (with consideration of instabilities, for example) was, at first, not present in the Soviet Union.

U. F.   "You have pronounced the word 'instability' — an important word for this interview project. When did the work on instabilities start in the Soviet Union?"

R. Z. S. explained that considerable efforts were made to understand the instabilities of the Z-pinch. Some of this was done with macroscopic modeling using the MHD equations or a two-fluid model. But, it also became necessary to investigate microscopic instability; R. Z. S. was involved in this. Various model equations and investigators were involved, including M. Kruskal at Princeton University.

As R. Z. S. spoke the word "nonlinear", U. F. interrupted to stress the great importance of this concept.

U. F. “Though we do not know when plasma research will lead to practical energy production, it has had a sizeable impact on the field of nonlinear physics”.

R. Z. S. concurred. R. Z. S. mentioned a talk he gave at P. D.’s seminar at UC San Diego in 2011. This was 50 years after the work of Kolmogorov and Arnold. That work, at first, was aimed at understanding the stability of the solar system — but, it was of interest to explore its implications for plasma physics. Here, R. Z. S. cited his 1961 work with A. A. Vedenov and E. P. Velikhov on the quasilinear theory of plasma instability [Vedenov et al. 1962].

R. Z. S. stressed the analogies with the Kolmogorov–Arnold work but also pointed out that Kolmogorov and Arnold were interested in stability, whereas Vedenov et al. were interested in heating through instability and stochastic diffusion (as a final result of the instability buildup).

U. F. stressed that even when the young R. Z. S. was assigned, in principle, to a weapons lab, he got involved in astrophysics (under Frank-Kamenetskii). R. Z. S. pointed out that a few years later, Zeldovich also got interested in astrophysics. Eventually, Zeldovich joined the Space Research Institute, then directed by R. Z. S. Zeldovich and R. Z. S. had many discussions. Zeldovich praised Frank-Kamenetskii for his early astrophysics work. Regarding astrophysics and weapons, U. F. pointed out the paper of E. Schatzman [Schatzman 1950], which may be viewed as an application of weapons to astrophysics, rather than the other way around.

Here begins an important story about G. Gamow and the prehistory of controlled fusion, most of which is told in Gamow's posthumous autobiography [Gamow 1970]. First, it is useful to quote a paragraph from MSS:

> The first nuclear bomb was made as a result of a *fission* [emphases added by U. F.] reaction. Theoretical physicists had known since the late twenties that the process that moved in the opposite direction, *fusion,* would produce an even greater amount of energy. But to materialize a fusion reaction would require that nature overcome the electric repulsion between nuclei. The most natural condition for overcoming electric repulsion exists within a high-temperature environment. The temperature has to be so high that at first it was unimaginable that fusion could be achieved in a man-made environment. Physicists thought the energy of the sun, and of the stars in general, could easily be explained on the basis of such thermonuclear reactions. (1994, p. 33)

In 1928, Gamow used the quantum tunnel effect to explain the alpha-decay. Then, Gamow and others explained how the same tunnel effect could be used to explain the release of energy through the fusion of light elements into heavier ones. In the late 1930s, a full quantitative theory for the Sun was developed by H. Bethe. But, a number of years before, the ideas summarized in R. Z. S.'s above paragraph were known to the experts in the field. Nearly identical to what R. Z. S. told U. F., Gamow's autobiography's states:

> Once, he [N. Bukharin, a high-ranking Soviet official, eventually executed by Stalin] attended my lecture at the Academy of Sciences (at that time still in Leningrad) on thermonuclear reactions and their role in the energy production in the sun and other stars. After this talk he suggested that I head a project for the development of controlled thermonuclear reactions (and that in 1932!). I would have at my disposal for a few minutes one night a week the entire electric power of the Moscow industrial district to send it through a very thick copper wire impregnated with small "bubbles" of lithium-hydrogen mixture. I decided to decline that proposal, and I am glad I did because it certainly would not have worked. (1970, p. 121)

R. Z. S. "It was the first time that the issue of artificial controlled fusion arose in the Soviet Union. This is remarkable, and Gamow spoke about it only a few months before passing away".

R. Z. S. "Artsimovich learned a lot of plasma physics from M. Steenbeck, brought to the Soviet Union at the end of the war, together with a large group of German scientists. Steenbeck worked on ionized gases before the war and published with A.V. Engel" [Engel and Steenbeck 1932]. According to R. Z. S., in the Soviet Union, Steenbeck was involved in electromagnetic isotopic separation. Steenbeck was eventually allowed to return to Germany, where he was involved in the discovery of the MHD alpha effect and also became President of the Academy of Sciences of the German Democratic Republic.

## 2 Early work

In this section, Sagdeev discusses his experience as a graduate student in Moscow and young researcher at what was later named the Kurchatov Institute of Atomic Energy. He discusses early work on magnetic confinement and instabilities in extended MHD (magnetohydrodynamic) models, derived from the Chew–Goldberger–Low system [Chew et al. 1956] for anisotropic pressure and weak collisionality. That model was developed in the United States during the late 1950s. In this section, ideas of “microinstability” (i.e. small-scale instabilities — with characteristic scale comparable to the ion Larmor radius — which do not cause macroscopic disruption but which do induce confinement degradation) make their first appearance. Here, we also meet Sagdeev’s longtime friend and colleague, Leonid Rudakov.

P. D. We are at the University of Maryland with Roald Sagdeev. Roald, could you start our interview by describing the scientific environment you encountered as a graduate student and young scientist in Moscow? How did you interact with various notable scientists of that time?

R. Z. S. At that period, in the mid-1950s, there were three principal centers of theoretical physics in Moscow. One was the Landau group at the Institute of Physical Problems, where Peter Kapitsa was Director. Another was a group under Igor Tamm at the Lebedev Physical Institute. A third group at a classified place was later given the official name of the Kurchatov Institute of Atomic Energy. Before that, it was called the Laboratory of Measuring Instruments. It was a huge, classified institution, where Igor Kurchatov was running the whole atomic problem in the Soviet Union. Inside the Kurchatov Institute, where I was assigned to work, there were several major departments. One, which had been recently created, was the controlled fusion group led by Lev Artsimovich. The Head of Theory was Mikhail [A.] Leontovich, who early in his life, was a student of Igor Tamm. There were other theorists working at the Kurchatov Institute, including Arkady [B.] Migdal, whose work focused mostly on quantum physics, solid state physics and superconductivity.

There was a lot of interaction among these three groups. Very bright, younger people were migrating from one group to another to stimulate interaction. One such person was Migdal, himself. He was a regular participant in the meetings of the Landau group. Landau seminars were held every Thursday. There was Vitaly Ginzburg, who was, at that time, still rather a young fellow. He was from the Lebedev Institute and considered a pupil of Igor Tamm, but he was very active in the Landau seminars, also.

Even at that time, the major Landau–Ginzburg theory — the phenomenological theory of superconductivity, based on something like a nonlinear Schrodinger equation — came out. So, I was somewhat disappointed that my assignment was to work at Kurchatov. What I wanted was to be part of the Landau group. So, I was compelled to go to Kurchatov, after avoiding an even more complicated and dangerous assignment of going to the newly created nuclear weapons laboratory in the Ural Mountains, Chelyabinsk-70.

P. D. Was this assignment at the level of postdoc or graduate student?

R. Z. S. After graduating with my bachelor's degree. Instead of becoming a graduate student of Landau, I was first assigned to go to the Urals as a junior scientist. That was the general environment. Eventually, it worked out much better than I thought at the beginning, because I was able to communicate with the Landau group very frequently — at least once a week. One day a week, I was at the seminar, talking to my colleagues.[1] My contemporaries, Lev Pitaevskii, and Sasha [Aleksandr] Vedenov were luckier. We were classmates at Moscow University and simultaneously passed the Landau minimum exams. The government did not prevent them from going to the Landau Group.

P. D. Of your many early contributions, two were the collisionless shock and the quasilinear theory. The latter may be thought of as mean field theory for

[1] Scientific life in Moscow during the time of the Soviet Union was a bit "delocalized," with scientists attached to, and working in, an institute, teaching at the Moscow Institute of Physics and Technology (MIPT, or PhysTech) or another academic institution, and regularly attending seminars at other labs or institutes.

Vlasov plasma instability dynamics. Both have, I think, a central element of understanding and pinpointing the origin and physics of irreversibility. This theme runs through a lot of your work. Could you tell us the scientific story of these two concepts? Were you thinking about them in relation to each other or to the ongoing work on chaos that you encountered from [Andrey] Kolmogorov's lectures in Moscow?

R. Z. S. When I joined the Kurchatov Institute staff as a junior collaborator in the lab of Leontovich, my first colleague was Leonid Rudakov. He came from the Institute of Engineering Physics in Moscow. The abbreviation was MIFI. The Institute was created to prepare the cadre for the atomic sector of Russian science and industry. The first assignment we got from Leontovich was to understand what happens inside the fusion machine plasma. The most advanced theory at that time was by Vitaly Shafranov, in the form of Kruskal–Shafranov criterion for pinch instability. Then, Leontovich wanted us to understand what happens at the edge of plasma. There must be some neutral gas coming to interact with the ions and electrons, and he said it would be interesting to have an analytical description of the interaction. This was because charge exchange has the highest cross-section at higher temperature; it doesn't decrease with energy so quickly as the other cross-sections. So, for a couple of months, Rudakov and I learned how to write kinetic equations for the interaction between hot plasma particles and charged particles. We were not very excited with that, because whenever we went to seminars inside Kurchatov, people would be talking about something different. From time to time, they would say, "There is a menace, which nobody can explain. It hangs over us, like a kind of Sword of Damocles". There was no explanation. Then, it was described as Bohm diffusion [Bohm 1949]. The diffusivity was claimed by Bohm in a very short article.

P. D. What year was this?

R. Z. S. We joined in early 1956.

P. D. So, 10 years before T3, people at Kurchatov were beginning to think about microturbulence?

R. Z. S. Yes. I think it was a few years after Bohm's article (about 1949) that some members — and probably experimentalists — described electric discharges in a magnetic field. We also followed what was happening in plasma physics, in general. We got a freshly published article by Chew, Goldberger and Low about collisionless magnetohydrodynamics [CGL theory]. Because everyone in our group, including Shafranov, had already established an area of focus, they were very busy. So, Rudakov and I were the first to accurately read this article. We thought: *Okay, now we have a new media. We have to describe the behavior of plasma, if it is determined by this model. The first thing we should do is to seek* *eigenmodes — low amplitude waves and fluctuations — using linear theory*. Very diligently, we linearized the equations. There were surprises when we looked at the linear waves. In certain cases, perpendicular and parallel pressures were not equal to each other. We were getting complex or imaginary roots, so that meant instability. Then, we said: *Okay, let's understand what is hidden here*.

Very quickly, we came to an explanation. One of them was a mirror instability. We called it *diamagnetic instability*; the name "mirror instability" came later, from the American literature. The physics was simple: Since plasma is diamagnetic, you can see how an excess of parallel pressure would expel magnetic field and cause more and more particles to fall into the potential well. The second explanation was what we called *centrifugal instability*; it was later called *firehose instability*. This helped us switch from what we thought was a very boring topic with neutral gas and gas-in-charge exchange to real plasma physics. We began thinking how this so-called diamagnetic or mirror instability, might play a role in mirror machines. We reported to Leontovich that there might be some complications — some instability — because plasma with a loss cone, by definition, would have perpendicular pressure greater than parallel pressure.

P. D. Was there an active program in mirror confinement in Russia at that time?

R. Z. S. Yes, but the famous breakthrough by [Mikhail] Ioffe had not occurred yet. But, the story with the mirror problem was the following: Nearly every type of magnetic configuration for a magnetic bottle on the Russian side was independently suggested or invented from what was happening in America. Tokamaks, as a toroidal machine, were suggested as an extension of the pinch by several people. In the beginning, it was suggested in a famous article by [Andrei] Sakharov and Tamm. The mirror machine was suggested by [Gersh Itskovich] Budker. This occurred a couple of years before we joined. Budker, himself, was not involved in further development of the mirror machine. He already had an idea about colliding beams and colliding-beam experiments. He was thinking about how to accelerate charged particles, like electrons, and keep them in storage rings. Another prominent guy at Kurchatov, Igor Golovin, was assigned to build some very early mirror machines. The early, small machines eventually developed into a fairly large machine called OGRA. This activity was taking place outside of the Artsimovich and Leontovich division.

There was a kind of a competition between what was happening in one division versus another. When Leontovich reported to Artsimovich the potential danger of mirror machines, Artsimovich was extremely excited. (I think it's probably in the character of competitors to enjoy seeing one's rival in trouble.) I was asked to give a talk at the big division seminar. Usually 100, maybe 200, people would come. There were experimentalists, theorists — everyone in fusion. I was asked to talk about mirror instability. Budker was personally invited by Artsimovich, who seemed to be anticipating the pleasure of Budker's reaction when he learned of his invention's design flaws. It was my first talk at this big gathering. It was the early spring of 1956, and this talk had an immediate impact on my life. After the seminar, Budker invited me to join his group!

P. D. A case of capitalism in the former USSR!

R. Z. S. Actually, I had already known Budker. During my last year as a student at Moscow University — in parallel with passing the Landau minimum exams — out of curiosity, I visited several places to evaluate other graduate school options. One visit was to see Budker, with a handful of other students. He was a fascinating guy. I liked him. But, at that time, I decided not to join Budker and to continue working with Leontovich. He [Leontovich] was an absolutely outstanding person. What amazes me, even now, was his approach to life and science. In that period, 1956-1957, Leontovich was in his early 50s, probably no more than 54 or 55 years old — and to my knowledge, he completely ceased doing his own creative science. All of his time was spent helping his pupils. He would move from one office to another, and ask, "What are you guys doing?" And, he would spend time to understand a situation, a particular problem, and he often gave valuable advice of a mathematical character. He was an especially great expert in mathematical physics. He even took care of his pupils, aside from scientific interests. He followed their housing situation, material life, etc. I have to confess, maybe a couple of times over that period, I had to borrow money from

Leontovich.

## 3 Collisionless shocks

In this section, Sagdeev describes the theory of the *collisionless shock*, which he invented. In contrast to familiar gas dynamic and MHD shocks, for which the scale is set by the competition between nonlinear steepening and diffusion, the scale of a collisionless shock is set by the balance of steepening and *dispersion*. Collisionless shocks form when the mean free path exceeds the spatial scale that defines the dispersion. This is typically the ion Larmor radius or the Debye length (for the ion acoustic case). Thus, the collisionless shocks in dispersion-dominated plasmas generate soliton-like structures. But, in plasmas, where the thermal energy of particles suppresses dispersive effects, the shock structure is controlled by microinstabilities and the resulting chaotic scattering of charged particles by electromagnetic fields. Collisionless shocks are ubiquitous in space and astrophysical plasmas. In this section, Sagdeev tells the story of collisionless shocks and discusses some of the key physics issues associated with them, such as entropy production.

P. D. Where did you go, scientifically, from there — from the CGL instabilities? Did the collisionless shock theory emerge from that?

R. Z. S. The collisionless shock in its first, simplest form — as a shock wave strictly perpendicular to a magnetic field — immediately emerged after that. After linear waves and firehose instabilities, the next topic to consider was the nonlinear stage, following the CGL model. We, of course, understood that the extension of CGL to parallel motion is based on a very simplified, adhoc hypothesis, and it wouldn't work, in general. What kind of hydrodynamics you can have in a collisionless case when you consider movements along the field lines?! So, I said: *Okay, let's consider the motion perpendicular to the field, and then, a specific heat ratio of* $\gamma = 2$. You can take into account magnetic pressure and plasma particle pressure. As a student of the Landau minimum, I immediately said: *Okay, let's see how the Riemann solution for simple waves could be applied*. There is a steepening. Eventually, you will have a discontinuity at the shock, and you do not have any viscosity. So, how do we reconcile this paradox?

My first simplified idea was: *Okay, so it would reach the Larmor radius, and then particles from downstream would enter into the upstream, at least on the distance of the Larmor radius*. The idea was whether the Larmor radius could be used as a kind of effective, collisionless mean free path and how one can create entropy. This was the first idea of overturning, and I needed something else to create entropy.

The next simple idea, which also came at that time, was that the phase of the gyration of the ions, in a non-uniform magnetic field, could gradually be randomized. That's phase mixing, in general, in space phase! As a student at Moscow University, I enjoyed the part of statistical physics specifically related to phase mixing. It was important as a foundation to statistical physics. I remember the lecturer of my class was not very remarkable in general, in science — but for some reason, he tried to bring students' attention to the idea of phase mixing — to Zermelo and such. I remember having a couple of conversations with him about how this phase mixing can be established and how it can bring randomization.

P. D. How, as part of your work on collisionless shocks, did the connection in plasma physics to the soliton occur? This is an important element of that story. How did that come to be?

R. Z. S. I can tell you what happened, originally. I immediately had the idea that there must be something like a shock. I went to talk to David Frank-Kamenetskii, who had just come back from Arzamas 16. We were under the Leontovich division. He was extremely excited, saying this might have great importance for astrophysics. The first publication was at a meeting of astrophysicists about solar activity. The idea was that, in solar flares, when shock waves generated by the flare propagate to a lower density plasma, there is a density accumulation. This shock would continue upward into the lower density, where collisions are already unimportant. Then, it would become a source of accelerated particles, simply because of the density-related accumulation. But, I wasn't satisfied with such a simple, qualitative explanation. There was no chance to simulate this using a computer. At that time, they were not available, at least in the Soviet Union.

Soon after, I started to think about finding something like a classical shock wave structure that could be considered as a steady state solution. If you make a jump from the Riemann solution, you will ask the question: How must a steady state structure emerge as a competition between nonlinear steepening and viscosity? Then, you can have a solution, finally — even if it's in simple fluid mechanics, including viscosity or heat conductivity. You will get a shock thickness of a few free mean path. So, I thought, *If I were to do the same thing in plasma, without classical viscosity, what would I get?* And finally, I was getting very strange solutions. I called them *individual pulses*, like a soliton. The word "soliton" was not yet invented. The paper on pulses was published before the 1958 Geneva conference. Also, it was clear that this particular type of solution would exist only up to a certain critical Mach number. Then, you will have overturning of the structure. It was like an isolated, mathematical exercise — to find this type of soliton-like solution.

P. D. To be clear, was this the basic theory of the ion-acoustic collisionless shock or still the magnetosonic shock?

R. Z. S. It was not yet ion-acoustic — it was magnetosonic. In 1956 or '57, I went to a major international congress of mechanics at Moscow University. A lot of people came. Before the conference started, I was walking through the lobby, and there was a huge exhibition of scientific literature on mechanics. I looked at a big, thick volume. It was the famous book on hydrodynamics by Lamb [1895]. As a theoretical physicist, I had never read it before, because I would stick strictly to Landau and Lifshitz and not study something outside of theoretical physics. So, I was leisurely moving from one page to another, until I reached an unusual picture, which reminded me of my individual pulses. I started to think: *What is it?* I then found another interesting analogy. In my calculations of the magnetosonic solitons, the letter I was using to describe the magnetic field was H, not B.

P. D. This was usual in the Russian school?

R. Z. S. Yes, in the Russian school, we used it. Here, it was lower case h. I said, "What is it?" Then, I discovered it was just elevation! This was an eye-opener. I said, "Why, there is a complete analogy — even critical Mach number!" I think that was the moment when I saw — that instead of competition and balance between nonlinear steepening and the collisional transport, like in the conventional structure of a shock wave — here, we have a competition between nonlinear steepening and dispersion at shorter wavelengths. For shallow water, you can immediately have a similar balance. This was an eye-opener! I went back home and said, "Can I find any other examples of that competition?" Ion-acoustic solitons were the first product. I even published a brief article on the hydrodynamic analogy for ion-acoustic waves. You can have the same type of discussion with shallow water or cross-field magnetoacoustic waves. Of course, from there, you can jump everywhere.

### Equations which look alike

The familiar shallow water wave system for water height $h$ and flow speed $v$

$$\frac{\partial h}{\partial t}+\frac{\partial}{\partial x}(vh)=0 \tag{1}$$

$$\frac{\partial v}{\partial t}+v\frac{\partial v}{\partial x}=-g\frac{\partial h}{\partial x} \tag{2}$$

can be modified by including dispersive surface wave corrections to obtain the Korteweg–DeVries equation

$$\frac{\partial u}{\partial t}+u\frac{\partial u}{\partial x}+\frac{c_0}{2k_0^2}\frac{\partial^3 u}{\partial x^3}=0 \tag{3}$$

Here, $u\ =3/2v$, $C_0=(gh_0)^{1/2}$ and $k_0=\ \sqrt{3}/h_0$. The KdV is a well-known example of a system which supports solitons, formed by the balance of its second and third terms.

Fast magnetosonic waves which propagate across a magnetic field $H$ are described by the system

$$\frac{\partial H}{\partial t}+\frac{\partial}{\partial x}(vH)=0 \tag{4}$$

$$\frac{\partial v}{\partial t}+v\frac{\partial v}{\partial x}=-\frac{H}{4\pi Mn}\frac{\partial H}{\partial x} \tag{5}$$

$$\frac{\partial n}{\partial t}+\frac{\partial}{\partial x}(vn)=0 \tag{6}$$

Here, Equation (4) is the induction equation with compressibility, Equation (5) is the fluid equation including magnetic pressure and Equation (6) is continuity. As $H/n=const$ (freezing-in law!), Equations (1, 2) and Equations (4, 5, 6) are identical, with $h\leftrightarrow H$, though they refer to very different physical systems. When dispersive effects, associated with the ion inertial layer ($c/\omega_{pi}$) dynamics, are added to Equations (4, 5, 6), a soliton equation results [Sagdeev 1966].

My next example was not an exactly perpendicular magnetosonic wave but an oblique one (at a certain angle). The dispersion relation, then, is completely different. You have a Whistler accelerator, and then have a different type of dispersion. The conclusion is straightforward. Instead of a compression soliton, you will get a rarefaction soliton, and so the structure will be different. So, that was how everything was evolving.

P. D. Was this where the idea of an effective nonlinear potential, what has come to be called Sagdeev potential, appeared?

R. Z. S. It was very simple to introduce it to magnetized plasma or to nonlinear ion-acoustic waves. Of course, if the Mach number is higher than critical, you will have reflection or overturning. And then, you come back to the original concept that magnetic fields control everything by intrusion of plasma ions from downstream-to-upstream, to the distance of about a Larmor radius. In the ion-acoustic case, there would be no force that would return the ions, so, in fact, they would then freely propagate upstream.

At that moment, a number of people learned how to numerically simulate this picture. For instance, at this mechanics conference in '57, I first met Harry Petschek, who came from America. My English was nonexistent at that time, but Petschek was speaking reasonable Russian because of his Czech origin. He was interested in collisionless shocks and had a completely different concept, which he was developing together with [Arthur Robert] Kantrowitz. So, finally, I had a lot of interaction with Kantrowitz and Petschek.

Their concept was very different. They did not consider the idea of overturning or competition of nonlinearity with dispersion. They wanted to introduce background noise of the oblique whistler waves, which could propagate faster than perpendicular magnetosonic waves. They made an interesting observation that with each passage through the shock front — from downstream-to-upstream or from upstream-to-downstream — the energy of these passing whistler waves would increase. If some hypothetical nonlinear effects would lead to these waves making many shock-crossings, it would create a mechanism for the dissipation of energy by multiple crossing. It was similar to the first-order Fermi process. But, the question was: How you can create sufficient amplitude for these waves from nothing – so it would react nonlinearly, with scattering back and forth? In the end, I think they abandoned this approach. In his last article about collisionless shocks, Petschek completely accepted the dispersion idea. But, his idea was an interesting one.

P. D. Do you have some further comments you wish to make on the subject of CGL and collisionless shocks?

R. Z. S. Despite the new results for plasma instabilities, it was clear that CGL is not absolutely appropriate, especially when you're talking about longitudinal movements along magnetic field lines. The next step in that same year of 1956 was to go beyond CGL. Together with Leonid Rudakov, we developed a framework of guiding center kinetics. The first thing we did was to redo the mirror and firehose instabilities. We got qualitatively similar results, but some quantitative ones were different. These affected the kinetic nature, especially for mirror instability. The results became very different in terms of the formulas. We put together a major paper for the second Geneva conference in '58. When I was delivering the talk about this paper, there was a parallel paper by an American group, presented by Marshall Rosenbluth.

P. D. This was your first meeting?

R. Z. S. Yes, this was our first meeting. Our first reaction was when Marshall was writing the same formulas for mirror instability and firehose that we had. We could only laugh in astonishment, as this "extraterrestrial" arrived with the same formulas. When we went to a dinner with Marshall, he said, "Why did you laugh when I was talking?" It was a very interesting reaction. Actually, this paper that I presented on behalf of Rudakov, Kadomtsev and a few other co-authors ended with a brief description of collisionless shocks. It referenced this soliton-like structure and overturning, and phase mixing in a non-uniform magnetic field. But, at that time, I didn't know Marshall also had a soliton-like solution. For some reason, it was not publicized much.

There were a few other groups that came with similar soliton-like solutions for magnetosonic solitons. There were a couple of British people involved. There was a group from Munich — Reimar Lüst (future President of the Max Planck Society) was among them. And, there was Harold Grad and his group from the Courant Institute. So, obviously, people were coming — but, I don't think they stressed this generic notion that there is nothing but competition between nonlinear steepening and dispersion. This is why they did not move quickly to other examples during this period.

P. D. Did any of these other groups address the entropy production issue? Given the availability of the soliton concept from mathematics, the really clever thing was the element of entropy production in the collisionless case.

R. Z. S. I vaguely remember that Grad and his team said something about phase mixing in their article. For me and my co-authors, it was rather simple, because phase mixing in a simple mechanical system was often discussed in earlier Russian-Soviet literature. [Nicolai] Krylov did some work in this area, even before [Nikolay] Bogolyubov. He wrote a book, which had everything known about phase mixing.

P. D. Was there any interaction with astrophysicists on this subject? You are aware of the whole later business on Violent Relaxation by [Donald] Lynden-Bell, which had some counterparts in the Russian school, as well.

R. Z. S. Lynden-Bell came much later. We already had the concept of the collisionless shock, but the first notion of it came from astrophysicists. Among my colleagues in astrophysics, there was one rather prominent plasma astrophysicist, Solomon Pikelner, at Moscow University. He came back from one international conference and told me that a guy named Tommy Gold suggested there must be collisionless shock waves to explain the sudden initiation of magnetic storms on Earth. Now, I understand what happened. Tommy Gold also published his article in *Nature* in 1956. It did not discuss the physics of collisionless shocks, but I would say that he advocated the need for such a shock. Otherwise, it would be difficult to explain the sudden onset of magnetic storms following solar flares.

P. D. In some sense, your work was purely theoretically driven, and it was not motivated by the need to address particular natural phenomena. Is this correct?

R. Z. S. Absolutely, it was driven just as a logical step in developing the magnetohydrodynamic approach.

P. D. So, one could say it was, in a sense, a prediction.

## 4 Chaos and quasilinear theory

Sagdeev now turns to his contributions to the related-but-distinct subjects of quasilinear theory and Hamiltonian chaos. Quasilinear theory refers to the mean field theory for the evolution of the plasma distribution as a consequence of a phase space instability, where growth results from Landau resonance. The success of quasilinear theory in predicting saturated states for some problems is remarkable, given its simplicity and its straightforward use of the linear Vlasov response to derive the mean field equation. A key aspect of quasilinear theory is its identification of the distinction between resonant and non-resonant particles, scattering and diffusion. The irreversibility intrinsic to resonant diffusion is rooted in the Hamiltonian chaos of particle motion in overlapping resonances. The theory of Hamiltonian chaos is the other, related topic of this section. Sagdeev describes the influence of A. N. Kolmogorov and the contributions of Boris V. Chirikov and George Zaslavsky. He also discusses the Hamiltonian chaos of wandering magnetic field lines and its implication for confinement, a topic he studied with his good friend, the late Marshall N. Rosenbluth. The theory of chaotic field lines continues to be of great interest today in the context of boundary control in hot, high performance confinement devices, and in the context of many astrophysical problems.

P. D. Let's move on to the subjects of Hamiltonian chaos and quasilinear theory. You were active in both, and these two topics are heavily coupled. Could you relate the scientific story of these two topics, and in particular comment on their interdependence? In a sense, the Hamiltonian chaos arose out of the work of Kolmogorov and Arnold, but the quasilinear theory that arose from conventional plasma physics was motivated by the need to understand the evolution of plasma turbulence and its effect on the mean field.

R. Z. S. In my case, the evolution of the work on linear theory was somewhat different. First of all, I was very interested in understanding the background physics of Landau damping. However, if you read the Landau paper, there is nothing about the physical meaning of this; it appears as a kind of mathematical artifact. So, there was very little in the Soviet literature following Landau. In some cases, strange things happened. For example, there was a very heavy volume on ionospheric plasmas, giving a very long mathematical expression for the imaginary part of epsilon for instability. Later, there even was a paper by Vitaly Ginzburg, which noted the imaginary part of epsilon for electromagnetic waves propagating through plasma, even in the absence of a magnetic field. There would be exponentially small Landau damping, which would look like exp[-$v^2/T$]. You had to substitute instead for $v$, the phase velocity of the electromagnetic waves in unmagnetized plasma, which is higher than the speed of light?!

In trying to understand this, it was clear that everything was coming from the resonance — that is, the particles moving with the phase velocity. No particles would be responsible for the imaginary part of epsilon in that type of straightforward expansion. People did not consider Landau damping and just took these expressions automatically. I tried to calculate the next order of nonlinearity. In this straightforward expansion, you will have a quadratic similarity in the denominator. In the next approximation, you will have a cubic similarity, which obviously makes little sense. Perturbation techniques wouldn't work. It was important to understand the basic physics.

One of my early pictures was quite simple. If you have a monochromatic wave, consider how an individual test particle would interact with it. You can even calculate if particles are moving slightly slower than the Langmuir plasma wave. They are pushed by the wave, so they are gaining energy. The wave is losing energy. If particles are moving faster, they will push the wave. So, they are delivering energy. The net result for the wave, whether or not it's losing energy, would be determined by the balance between these two energy exchanges. I brought a simple picture of the basics of Landau damping to Artsimovich. I said, "Look, given the actual value of Landau damping, you can get more or less accuracy from this type of consideration". It was very clear that it's the case when you cannot use perturbation techniques, but you can accurately approach it within a full, nonlinear framework. It was one line of evolution.

The second line of evolution was the following: As part of classified science related to radiation physics in weapons science, there was an interesting article by one of Landau's pupils, a Russian theorist by the name of [Alexander]

Kompaneets. This particular paper [Kompaneets 1957] was unclassified and published a few years earlier. It was about the evolution of photons, the light quanta, in media where they can be scattered by the electrons. This is like Thompson scattering, or more generally Compton scattering. Kompaneets converted this problem related to individual collisions of the quasi particles — the quanta — with electrons into a kind of diffusion equation.

P. D. Did this end up as a kind of diffusion, like you get in mean field theory for wave kinetics? A kind of scattering in $k$ and so forth?

R. Z. S. Yes, kind of.

P. D. Did this paper trigger your long love affair with quasi-particle pictures?

R. Z. S. No, it did not. It was very clear that the radiation transfer could be written in different forms, even as the Rosseland average for thermal conductivity. It was actually my bachelor's degree work with Frank-Kamenetskii on radiation transfer. But, what happened is that this article triggered the interest of several guys in the weapons lab. One of them, Romanov, was already a veteran of the weapons program. Another, Filipov, was my former classmate from Moscow. They generalized the theory. They followed Kompaneets into interaction between plasmons and electrons. It gave them a diffusion equation very similar to the

Kompaneets equation, which was a precursor to the quasilinear theory. They even gave a talk at Kurchatov about it, based on this type of approach.

By that time, [Evgeny] Velikhov, [Aleksandr] Vedenov and I were already deeply involved in a new look at something related to nonlinear Landau damping and quasilinear theory. The line of argument was the following: When you apply perturbation theory for a monochromatic wave, you can relate singularity in denominator and the integrability problem. What would happen if you have — not a monochromatic wave — but a broader wave packet? You are not accumulating powers anymore, because every added turn would give you different resonances. We thought it would eliminate difficulty in using a perturbation technique. If you follow this wave packet, you get a mean field technique immediately — the quasilinear equation.

I think my personal interests were somewhat different from Velikhov's and Vedenov's. How can this particular transition from a collisionless kinetic equation to a diffusion-type equation — which does not conserve energy — be explained? Two things played important roles. One was Kolmogorov's lectures in celestial mechanics about resonances. There was also a younger guy at Kurchatov, a collaborator of Budker, named [Boris] Chirikov. He talked about instabilities and the eventual loss of particles from inside the separatrix accelerators. In considering nonlinear resonances, Chirikov said that if you bundle up resonances, something bad would happen. It immediately helped me jump to the simple idea that if you have a non-monochromatic linear wave — a packet of several discrete waves — well-defined areas of the resonance of each wave are established. Quasilinear diffusion results from their overlap. In a monochromatic picture, each electron is controlled only by one wave in resonance. If you have neighboring waves with overlapping resonances, you have a collective motion of the electron from one resonance to another. I think it was this philosophy that was included in the paper reported at Salzburg [International Atomic Energy Agency conference, 1961] on controlled fusion and plasmas.

P. D. This was with Vedenov and Velikhov?

R. Z. S. Yes. The interesting thing about this paper [Vedenov et al. 1962] was that *Nuclear Fusion*, as a journal, was established under the auspices of the International Atomic Energy Agency and followed the rules of the United Nations. There were several official languages that were considered equal – English, French and Russian. Out of respect to Soviet participation in the United Nations, this paper was published in Russian in the conference proceedings. It is the least-cited paper of any I have ever published. It was never translated into English. This paper gave a detailed articulation of the idea of resonances overlapping for this particular case. In this paper, we extended the quasilinear approach to include the case of cyclotron waves, thus getting the effect of pitch angle diffusion (an important process in mirror-type configurations). Later, Charlie Kennel and Harry Petschek had to redo it in their seminal paper on particle precipitation from radiation belts. It's interesting that the major Kolmogorov and Moser article about KAM theory was also published in 1961. It was on a completely different, limiting case. Instead of considering the region of or condition for chaotic motion, they considered the area of stability. I think this made our paper with Vedenov and Velikhov the first article on the anti-KAM case.

P. D. You talked about the effects of the resonant surfaces, while Kolmogorov focused on the irrational surfaces?

R. Z. S. Yes, and Chirikov — who had very good, early ideas — talked mostly about practical applications for accelerators. For some reason, he never, at that time, came to the diffusion equation. Only later, when he wrote to suggest a standard map, did he start to talk about diffusion.

P. D. Was he concerned primarily with a single particle picture, not the collective?

R. Z. S. Yes.

P. D. So, if I understand the flow here: The driver was to understand the physics of Landau damping more deeply in the nonlinear regime, and the chaos theory was, shall we say, expropriated to once again resolve the issue of irreversibility?

R. Z. S. Yes, absolutely.

P. D. And then, the other key element – you know how we teach students in introductory courses the two forms of energy conservation, resonant particles and waves or particles and fields? This completed the quasi-particle picture, which came from Kompaneets. You have described how the three threads came together.

R. Z. S. In the history of Russian, of Landau's, involvement in nuclear weapons, those who knew the early story said Kompaneets was the closest associate of Landau in this classified format.

P. D. If we skip ahead a few years, where would you place the famous paper of Rosenbluth, Sagdeev, Taylor and Zaslavsky [Rosenbluth et al. 1966], which, in some sense, repeats or addresses many of these issues in the context of magnetic islands? But also, I might add that this article was published in English rather than Russian. Has that paper received the attention that was owed to the '61 paper?

R. Z. S. Obviously, the '66 paper was cited much more. The story of the paper is very interesting. I was preparing to go to Trieste. It was Marshall's idea to hold a joint workshop in 1965 and 1966. Before going to Trieste with George Zaslavsky, we had already had several years of joint discussions – on phase mixing, randomization, the transition to chaos, in general – with several publications. For example, one publication was related to the virtually classical problem of the transition from microcanonical ensemble to macrocanonical ensemble. Usually, in all the textbooks of that period, it says that you have to introduce one important assumption of random phases, for example. Before this magnetic island paper, we published an article in *JETP* [*Journal of Experimental and Theoretical Physics*] in the early '60s that analyzed the details of how you can go beyond the random phase approximation assumption. We thought, *Okay, let's start with fixed, not random phases and follow them as they evolve through different phases.* You use a number of eigenmodes with incommensurate periods or frequencies. Our argument was that you would eventually come, automatically, to the randomization of the phases. The message was that, most likely we would not need the random phase assumption in justifying statistical physics.

Another example we played with was classical, monochromatic wave Landau damping and how entropy is increased in this. You have a collisionless equation, starting with a Maxwellian distribution, and then the particle begins to bounce. The particles at the bottom of the potential well and closer to the separatrix have different periods. Eventually, you will have a modulated distribution function and the frequency of modulation — especially on the beams closer to the separatrix — would become greater and greater. The philosophy we developed is exactly like in statistical physics: How could we come to the notion of entropy? We could not describe entropy according to Liouville, where we follow the trajectory of every particle, and it eventually moves to the Maxwellian distribution. There was no instrument to help us to follow all the particles. So, if we are measuring the energy of particles with a certain finite energy window, at a certain moment in Landau damping, you would lose that. You would have H-theorem.

P. D. You have coarse graining.

R. Z. S. That was the line of the argument we were developing. And then, suddenly in '64, '65, we came to the notion that equations for magnetic field lines can be converted to Hamiltonian form. We said, "Look, this is exactly what we have for particles, for electrons in plasma". Immediately, the next step was: Could we get an analytical quasilinear solution for magnetic field lines? Yes! But, there was a problem. If we have a coil — a regular coil with known, small irregularities — can we get a diffusion equation when we know the exact magnetic field, with its irregularities? It bothered us, and we did not finish this paper. I was going to Trieste. We agreed that was a very important case and said, "We have to ask Marshall to join us".

I have to tell you, in my mind the authority of Marshall was highest in plasma physics. Maybe I told you already — the one thing I will feel sorry for, the rest of my life, is that I never told Marshall that he was also one of my teachers.

So, I came to Trieste, and we began talking with Marshall. I told him what we did with George, and now we were at a loss for what to do. Marshall started to think about our problem, and then, at certain moment, he asked if we would invite Bryan Taylor to join. So, we invited Bryan Taylor, also. Most of the discussions were, kind of, philosophical discussions. But, what Marshall did at that time was to write a huge number of pages — with calculations about individual resonances and so on. At the end of all this, he had almost repeated, in the form of an application to the magnetic field problem, what Kolmogorov and Moser did. This is how the article was published.

P. D. Did he, essentially, redo the KAM [Kolmogorov–Arnold–Moser] theorem?

R. Z. S. Yes. So then, we decided to publish this article.

P. D. You know, of course, that the question of the interaction of the external coil is essential to the fusion program now — for ELM [edge localized mode] control, for the so-called resonant magnetic perturbations and understanding the response of the plasma to it.

R. Z. S. So that you can artificially increase and decrease diffusion.

P. D. The surprise is, of course, that people thought it would artificially increase heat transport. Instead, it seems to artificially increase particle transport more than heat.

R. Z. S. By the time I returned from Trieste, Zaslavsky had married a younger lady, who was my grad student, Natasha Filonenko. She was from Odessa and was very capable of participating in calculations. A specific calculation about overlapping resonances was made for different stellerator-type configurations. It was published in *Nuclear Fusion* — fortunately, this time in English. In a way, I think it contributed to the creation of a negative attitude toward stellerators. The irony is that tokamaks have a much more trivial and straightforward configuration, compared to the brilliant idea of magnetic transform that Spitzer suggested for stellerators. Ironically, this more trivial configuration defeated it.

P. D. It is not clear which has defeated which yet, because the price of the simplicity is the current. It may be, in the end, too high a price to pay. And, by the way, this idea of the resonant magnetic perturbation effectively converts the tokamak to a quasi-stellerator. So, in a sense, you're back to the 3-D configuration.

R. Z. S. Additionally, the price you pay for the stellerator — which we soon learned — was the neoclassical superbanana diffusion.[2]

P. D. We will come to that. Let me ask you: Why did that line of work stop? You had these papers from work done in Trieste, and then, there was a quiet period. Then, in 1978, along came the Rechester–Rosenbluth paper [Rechester and Rosenbluth 1978]. This paper stopped talking about field lines, started talking about heat and also examined the interplay of collisions and stochasticity. What caused this shift?

R. Z. S. I think that when I came back from Trieste, I started to have some problems with Budker. For some reason — I don't understand why — he didn't like me taking these long leaves from his institute for Trieste. When I came back, he said that what I did was a mistake, and if I wanted to go my own way, he wanted me to take all of my science to a different institution. Actually, I had to leave Novosibirsk before because of some kind of tension with Budker. I came to Moscow, and I had to do some completely different work. George Zaslavsky joined me in late '70s.

P. D. I recently read the book, *The Birth of a Theorem*, by the French

[2] Here, *neoclassical* refers to collisional (i.e. "classical") transport in curved magnetic fields. "Bananas" refer to orbits of magnetically trapped particles, during which the particle drifts off the magnetic surface. Thus, the projection of the orbit in the poloidal plane of the torus resembles a banana, and the orbits are so named. "Superbananas" are very fat bananas that arise as a consequence of the structure of stellarator magnetic fields.

mathematician Cedric Villani, who won the Fields Medal not long ago. He proved a generalization of Landau damping to larger, finite amplitude. The question for you is: Many of these problems on chaos and nonlinear Vlasov plasma dynamics sit at the boundary of mathematics and physics. If you had the services of a few first-rate mathematicians at your disposal, what problems would you set them?

R. Z. S. I can tell you the problem I set already, in fact. The main, basic physics of Landau damping is that, eventually, depending on the amplitude of the wave, the distribution function of the plasma would settle into an ensemble of trapped particles — a plateau. If nothing else is involved, Landau damping would eventually saturate. If there are even a small number of collisions, you need only small angle scattering to partially restore the slope of the distribution function, and so, restore some of the damping. The first thing I did was publish in *Nuclear Fusion* — again with Velikhov and Vedenov — a simple case. We considered a finite-amplitude wave packet. From quasilinear theory, it would build a plateau, and you have no more damping. Then, let's bring small collisions into the calculation. The plateau would be slightly modified, and finally, you get Landau damping behaving like an effective nonlinear damping, $1/I^2$ (where $I$ is the amplitude. It was published. So then, I thought: *Okay, how it would work for monochromatic waves?* Which of the packets do you have to replace with trapping $\sqrt{\Delta I}$? Then, if you take it and substitute — instead of $1/I^2$, you will have $1/I^{3/2}$. Then, I thought it would be interesting if I could find someone with mathematical skills who could resolve it in a very strict framework — with the math for particles, trapped particles, passing particles, boundary layer, etc.

It was one of the first problems I gave to Vladimir Zakharov when he joined me in Novosibirsk. Before that, Vladimir was a student in Moscow. He was very interested in math, and he had an interesting professor — an advisor, who was quite prominent in mathematical circles by the name [Mark] Vishik and worked on singular differential equations, boundary layers, etc. Eventually, Zakharov and Karpman published a paper, deriving the amplitude [$A$] expanded in the power of 3/2 in the denominator [i.e. $A^{-3/2}$], separating trapped and transiting particles and resolving narrow, sharp boundary layers in velocity space. I think it still is one of the most important works on this subject. I don't know why the mathematical community ignored it. Vladimir, in that respect, did a great job. I think [V. I.] Karpman was a co-author, but the driving force was Vladimir. Before he did his next widely known work — the kinetic equation for ocean waves — he did this paper.

P. D. In a similar vein, many of the things that you worked on led to or anticipated some of the thinking of Tom Dupree — on a kind of renormalization and orbit scattering picture. However, you, yourself, never quite went there. Do you have a comment on that?

R. Z. S. Yes, I probably thought: *I should do it.* It's not so important in terms of practical applications, but it is important in terms of the justification of quasilinear theory. Your article in *Physics of Fluids B* was an example, where you actually defended quasilinear theory against Laval [Liang and Diamond 1993a, 1993b]. I was busy with other stuff. I feel sorry that Tom Dupree left plasma physics. He did a good job.

P. D. Of course, I'll be the first to second that. What's notable is that Dupree's work has had a big impact later on, through extensions by other people.

R. Z. S. I understand some people didn't like what he did, because there is a kind of general, negative attitude in theoretical physics against higher approximations in perturbation techniques. It goes back to renormalization. People thought: Why go beyond quasilinear with the next order of higher approximation when you use the quasilinear term – not for the background distribution function – but for the fluctuating function? I think Dupree did a good job.

P. D. But, in a sense, that criticism of him is unfair, because he gave a physical picture of the whole industry of the Direct Interaction Approximation in the Vlasov plasma context. Most would, with appropriate caveats, use the DIA methodology.

R. Z. S. I think there is another reason why I didn't pay attention to this development at that time. I wanted to move further with collisionless shocks. An important step in that field was to develop the quasi-particle idea in a different dimension, namely wave–wave interaction. The need for wave–wave interaction became very obvious. If you take — not a precisely perpendicular magnetosonic wave, but at an angle — then, you have a rarefaction soliton going into the downstream and moving ahead of the wave train in the upstream, a regular Whistler. I wondered: *If you don't have any collisions and any collisional damping, then this wave train would become infinite; it would break the whole concept of collisionless shock waves*. So, at that stage, I thought that I should find something to stop the infinite penetration of this upstream wave train. And so, the idea of this parametric decay instability came about. I started with the very simplified model of Korteweg–deVries in soliton wave theory. If you take Korteweg–deVries and consider it beyond one dimension, you will have a very simple model for decay instability of the cnoidal wave — a periodic wave because of the rising dispersion curve. I did it and never published the calculations, but I mentioned there might be such instability. When I came to Novosibirsk, I thought it would be interesting to look for other examples, such as the parametric decay instability of a Langmuir wave into another Langmuir wave and an ion acoustic. I brought Oraevskii to help me with the calculations, and so that kind of distracted me. And then, we started to work on the weak turbulence system, systematizing everything, treating quasilinear theory as a simple particle–wave interaction, and three-wave interaction. Vladimir Zakharov was thinking about four-wave interaction. The first papers on weak turbulence were being published, and Kadomtsev suggested that nonlinear Landau damping should also be included. Essentially, nonlinear resonant interaction of particles and waves concluded the final scheme of weak turbulence. A lot of time went into doing it. To some degree, it was important and interesting in terms of practical plasma behavior, but did not directly yield many important results for confinement. One reason was because the main object of interest — the waves governing heat transport — did not obey very well this simple nonlinear framework. The missing link was zonal flow.

P. D. I would argue that you are too hard on yourself. The work on Langmuir turbulence had a profound impact on theorists starting to think in terms of a zonal flow. That's a closely related problem.

R. Z. S. The only thing I did in relation to some application was to zonal flow. While at IKI, I had a different life — 99 percent of my time was dedicated to administration, science projects, getting funding, everything. Sometimes, I would squeeze in half an hour to talk to Alec Galeev, Vitali Shapiro, or George Zaslavsky. Shapiro came and said, "Look, there is an article by John Dawson, and in his numerical simulation of magnetized plasma, he discovered some kind of convective cells". Then, I had a simple idea: Why don't we try to use parametric decay instability of something existing, like a drift wave into another drift wave, and an entity that has a zero eigenfrequency (but has a $k$ number)? That's what we actually did in a simplified case.

P. D. This was the Sagdeev–Shapiro–Shevchenko paper in '78 on convective cells, correct?

R. Z. S. '78, yes. It was a pity that it was not followed further.

P. D. But, the key idea is sitting there. I can tell you: That paper was very useful in the early days of the zonal flow. The curious thing from a coherent interaction picture — about both that paper and a similar work by Hasegawa — was that both were concerned with, but neither actually pinpointed, the idea that the excitation of the zonal flow would lead to improved confinement. I speculate that was because the H-mode had not yet been discovered.[3]

R. Z. S. When I was very much involved in the Venus atmosphere project, we were sending two spacecraft with helium-pressurized balloons to study the super-rotation of Venus. There was an idea that microturbulence could generate this regular super-rotation. When I came to the U.S. and heard about H-mode, the first idea was straightforward: Why don't we use the same concept as in the super-rotation of Venus? Shapiro was at Maryland with Shevchenko. We asked

[3] Concerning the "high confinement," or H-mode, also see the paper by F. Wagner in this volume.

Jim Drake to join us. He had a numerical simulation of how convective cells could create this regular shear flow. The next simple idea we considered was to substitute drift waves for convective cells. We invented this instability — why didn't we do it?! Shapiro and Shevchenko were busy finding jobs in America. I was busy writing a book about political involvement with Susan [Eisenhower]. She and I had an NGO, a post-Soviet change project and so on. So, that was the story.

## 5 Strong Langmuir turbulence

In this section, Sagdeev discusses the work by his school on fundamental subjects on plasma turbulence, beyond the level of mean field/quasilinear theory. Here we encounter the theory of Langmuir turbulence, in which density cavities supported by plasma wave radiation-pressure undergo self-similar collapse to singularities. This process is related to self-focusing, and constitutes a paradigm of self-similar nonlinear evolution distinct from the classic Kolmogorov cascade. Here, we again meet Sagdeev's student Vladimir Zakharov, who went on to a brilliant career in his own right.

P. D. We're going to discuss work done by you, your colleagues and the Russian school in the realm of strong turbulence, particularly the evolution of Langmuir turbulence theory and the work of Vladimir Zakharov and his school.

R. Z. S. Going back to my tenure at Kurchatov in the late '50s, the major type of instability was the *current convective instability*. At that time, there was no attempt to consider anything that could be called strong turbulence to explain the nonlinear evolution. The evolution of the current convective instability did not fall into the framework of chaotic turbulence, but rather, a kind of organized destruction. The first notion of strong turbulence, I think, was used by Boris Kadomtsev when he wrote a paper — somewhat before the time of drift wave instabilities — on what was then called the *current convective mode*.

P. D. You refer to the Kadomtsev–Nedospasov work. Effectively, this is equivalent to rippling mode in the zoology of Furth, Killeen and Rosenbluth — but there were no magnetic surfaces, so you had no need to consider current.

R. Z. S. Yes. At the end of this story about current convective instability, Boris Kadomtsev used very simple pictures of mixing lengths to evaluate diffusion. There was nothing new, no breakthrough. Mixing length ideas were used, even long before Kolmogorov, by researchers studying fluid dynamics.

P. D. For example, Ludwig Prandtl.

R. Z. S. Prandtl's ideas were used by engineers, plumbers and so on. With drift instabilities, the immediate idea was to use the same mixing length ideas, and so, this is how gyro-Bohm scaling first came about. Then, my article with Marshall and Bruno at Trieste utilized mixing length ideas in a simple way. Real Bohm diffusion, in that picture of mixing lengths, appeared in my paper with Sam Moiseev in 1963.

P. D. Was this on the collisional drift wave?

R. Z. S. It was the collisional drift wave, because to get full-scale Bohm diffusion out of these simple arguments, you must have a growth rate comparable to a real part of the frequency and wavelengths comparable to the perpendicular size of the system.

P. D. Did the work on the current convective instability have an impact on the work on the collisional drift wave? In both, the critical flux gives rise to relaxation due to a collisional phase shift — a kind of collisional, frictional factor.

R. Z. S. By that time, no. The drift wave came a few years later — maybe four years. By that time, the current convective mode was already forgotten, put aside as something mostly relevant to discharge plasma physics, not to fusion. In fact, you are absolutely right; you can argue in this way, also.

Then, everyone was busy with weak turbulence, its quasilinear theory, wave–wave interaction and so on. We were looking for different examples in weak turbulence. Zakharov was very good. He understood that it would be very difficult to find clean examples in the plasma case. So, he went outside of plasma and looked to ocean surface waves. I supported him from the very beginning. The complication was that for ocean waves, there is no three-wave coupling — you have to go to four-wave coupling. My impression was that he wanted to go back to classical problems. The Phillips spectrum was already known, and Zakharov simply wanted to challenge it. For him, the mathematical difficulties of three- or four-wave coupling were a minor factor, because he was a powerhouse in the math. The funny thing is how he did it. He converted the math equations — the nonlinear equations for surface waves — into Hamiltonian form. Then, from the Hamiltonian form, he constructed the four-wave kinetic equations. And then, he looked at the collisional integral of four-wave and discovered that there is this power spectrum. In the first publication, he did not even give a very simple physical explanation of this power spectra.

Later, at a lecture I gave at Princeton, I pointed out that if you have a four-wave interaction, then the nonlinear transport in phase space is proportional to the cube of the amplitude, from which you recover Zakharov's $1/\omega^4$ spectrum. His was one of the best examples of using weak turbulence outside of plasma. It's still considered exemplary.

Less well-known here in the West, Zakharov found another example of weak turbulence outside of plasma in nonlinear spin-wave interaction in magnetic materials. That was a case of three-wave coupling in a field of spin waves. He published a paper with Victor L'vov [Zakharov and L'vov 1975].

In the meantime, in plasma, we were all very interested in the most basic model for nonlinear saturation in plasma instability. In the non-isothermal case with hot electrons, we knew that there is a basic nonlinear three-wave process coupling — the parametric decay of Langmuir waves into another plasma and an ion-acoustic phonon. Each time, you have longer and longer waves, so the relay process of that type would only carry energy into smaller wave vectors. We had a lot of discussions in Novosibirsk. Vladimir [Zakharov] became very worried about it. We called it "infrared catastrophe". By that time, Vedenov and Rudakov had already published a paper at Kurchatov about modulational instability of the Langmuir plasma waves.

P. D. When was that?

R. Z. S. I think they wrote it in something like '61 or '62. I had already left. I thought it was a very elegant paper. They used kinetic equations for plasmons and so on.

P. D. Is this where the analogy with Landau resonance and quasilinear theory for waves appeared?

R. Z. S. Yes. Essentially, the message from this paper was that there is another process affecting Langmuir wave spectra, which can create the opposite flux for shorter waves — but it was simply a linear theory of this modulation, nothing more. From this particular picture, somehow, Zakharov came to the idea of plasma wave collapse. He found that in a three-dimensional analysis, these cavities would continue developing and finally lead to the origin of singularity. As these cavities became shorter, small and narrow, the wavelengths of Langmuir waves would become shorter and shorter. It would stop quickly due to Landau damping, of course.

I can tell you how Zakharov came to the idea. There was an independent development in nonlinear laser physics. There was a fellow in Gorki [Russia] — a city, now called Nizhny Novgorod — by the name of [V. I.] Talanov. I don't remember whether I met him face-to-face. He wrote a number of articles describing this self-focusing of a beam, which is equivalent to collapse in nonlinear laser physics. First, Zakharov put these nonlinear effects from a laser media into a nicer mathematical language. From there, he made the jump to a self-similar solution of plasma-wave columns. This was very elegant and even difficult to understand, because it required a three-dimensional picture. In a single dimension, you can follow the creation of cavities. You see that, after a while, it stops and self-stabilizes. Sometimes, it could be converted into envelope solitons for plasma waves. In two dimensions, you can go further, but there is still a kind of balance that does not develop into singularity. So, you need three dimensions. Zakharov's first paper about plasma wave collapse ended with a kind of open question. People decided that this was a self-similarity solution. A three-dimensional equation would look like that. I cannot solve it, but I can hypothesize about it. It would lead to real singularity. In order to get final support, he needed computational input. He found a computational and numerical colleague at the Keldysh Institute of Applied Math in Moscow, Lev [Markovitch] Degtyaryov, who developed a simplified model using what we now call "the Zakharov equations". These are a kind of extension of the nonlinear Schröedinger equations.

P. D. And very similar to the nonlinear self-focusing equation, which is also a nonlinear Schrödinger equation?

R. Z. S. Also, I would say they were an extremely important precursor to what later happened with Bose–Einstein condensation. There is an equation called the Gross–Pitaevskii equation. Essentially, it came ten years later after the Zakharov model. For some reason, the people who were working in Bose–Einstein condensation completely missed that all the foundations for the nonlinear equations were already done.

P. D. It has the features of a dual cascade. You have energy collapsing to small scale and quanta accumulating in a single, large structure.

R. Z. S. Yes. And then, in the final stages before collapse, most of the energy would go to the tail of the electron distribution due to Landau damping. Essentially, it was a picture of individual collapses. It was very important to build a picture of turbulence based on multiple collapses, which established a balance between the source of instability — which would create Langmuir waves, whether it is laser pumping or electron beam injection — and tail heating. So, this what we tried to do at IKI when Shapiro and Shevchenko joined. We also invited Degtyaryov, the same colleague. It was very difficult to have a three-dimensional simulation at that time. To work around this, we saw that if you have strong pumping, one-dimensional, modulational instability develops in these cavities. This would continue for some time, up to the moment when the tail of the electrons would start to consume the energy. We wrote a number of papers about strong turbulence, establishing relationships between initial pumping, the final average amplitude of the waves and the tail of the electron distribution using this simplified numerical simulation. We later tried to use a two-dimensional Langmuir modulational picture, but the results were essentially the same. We wrote major articles for the 1982 Göteborg International Conference [Sweden].

There was one particular application of strong Langmuir turbulence that we were interested in, outside of the nonlinear laser–plasma interaction. It was related to the so-called *beam plasma discharge*. Shapiro and Shevchenko — before they joined me in Moscow — worked and graduated in Kharkov at the

Ukrainian Physical Technical Institute, which was a very prominent place in Kharkov. It was prominent after Landau spent some years there, before World War II, and after this, Fritz Houtermans was sent to Kharkov as a Director by the Nazis during the occupation of Ukraine. The advisor-boss of Shapiro and Shevchenko at the Institute, Yakov Feinberg, was an interesting fellow. He understood beam plasma instability physics on a simple, physical level very well. He was a very nice, smart, sweet man, with whom I had good relations, even after I managed to steal Shapiro and Shevchenko from him. He suggested, already in the late ‘60s, the phenomenon that he called beam plasma discharge.

Think of it like this: Imagine you have a neutral gas, and then you inject an electron beam into this neutral gas. So, the first process would be just ionization of the neutral atoms or molecules by incoming higher energy electrons. It would take a while, because the cross-section decreases with energy. Then, you start getting secondary plasma out of the ionization. This secondary plasma would immediately start getting energy at a higher rate due to beam plasma instability. This would create a much higher number of low energy electrons and is sufficient to quickly ionize the neutral gas. So then, you will have a kind of avalanche of ionization through a collective process of the beam plasma instability. Feinberg called it beam plasma discharge, and there were a number of articles that followed.

My interest in using strong Langmuir turbulence to describe this process had a very practical origin. The very first experiment I was part of when I joined

IKI involved launching an electron beam from a high-altitude rocket at the French Kerguelen Islands in the Indian Ocean. The electron beam followed the Earth's magnetic field line up to $L = 4$ and then crossed a distance of 100,000-150,000 kilometers. It had to re-enter over sub-Arctic Russia in the Arkhangelsk region. We used a French, high-altitude rocket called Eridan, and the electron gun was made in the Ukraine by a prominent engineer who was an expert in using electron beams for welding. Today [2015], the man is 95 years old! And he has been President of the National Academy of Sciences of Ukraine since 1962! His name is [Borys] Paton. He had great expertise in electron guns, so we contracted him. Then, on the receiving end, in the Arkhangelsk region, it was winter time. We had super-sensitive TV cameras watching the precipitation of these artificial electron beams. The weather was not cooperating with us. The TV pictures didn't come out very well, but we had special low-frequency radar that geophysicists used to study auroras. Then, the question was: How could this electron beam — precipitating in the upper atmosphere at an altitude of 100 kilometers — immediately ionize? This led to the use of the beam plasma discharge concept of strong Langmuir turbulence, which we had from following Zakharov's picture. There were several papers published. One of my grad students finally got a Ph.D. on doing all these calculations. His name is Evgeny Mishin. He works at Albuquerque at the Air Force Lab now, continuing to study nonlinear effects in ionospheric physics. We also tried to do some predictions for strong turbulence in laser–plasma interaction, but it was abandoned long ago.

P. D. Did you ever explore the idea of using some of these techniques for drift wave problems, with the zonal flows as the condensate?

R. Z. S. Never. Actually, at the end of one of the articles about parametric decay of a drift wave into another drift wave and convective cells, we said that we had to further consider nonlinear evolution. But, it was never done. I think the reason was because we switched to the Halley's Comet project. It became a nightmare for me.

P. D. So, you went out of fusion and into space.

R. Z. S. And also into politics, getting money for all these projects, you know — fighting with other competitors. It was a tough time.

P. D. Did you ever explore the statistical theory of multi-soliton turbulence?

R. Z. S. Not really. I think Moiseev was doing some statistical theory — not exactly with KdV, but with the Burgers equation. I never thought it had strong relevance. Actually, a couple of years ago, Vladimir Zakharov wrote an article about multi-soliton turbulence. It had interesting mathematical work in it. When

IKI celebrated my 80th birthday, Zakharov came to Moscow and gave a talk about this multi-soliton work.

P. D. Zakharov has produced a number of excellent students of his own, hasn't he? You might view them as your "academic grandchildren".

R. Z. S. Yes, he did. I remember students who were there during my time at Novosibirsk. Evgenii Kuznetsov was there. [Gregory] Falkovich came a little later. Kuznetsov, I remember very well. I think he is now probably the closest associate to Zakharov. Manakov was there. We thought he was an extremely promising guy — very bright, mathematically, but his life ended tragically. Oh, of course, [Alexander M.] Rubenchik (now at Livermore) — I knew him well. Rubenchik came from the same group.

We had another paper with Zakharov on strong turbulence and the story was the following: Can you describe chaos of strong acoustic sound waves in compressible fluid, in the same terms — as Kolmogorov described interacting vortices in incompressible fluid with his spectra? Essentially, what we did with Zakharov was a very simple model in the spirit of Kolmogorov. What would be the spectrum? We got $K^{-3/2}$. There was one particular problem, and we had a disagreement with Boris Kadomtsev, who said that it would not develop into Kolmogorov-type turbulence of acoustic waves, but rather degenerate into a gas of little shocklets.

P. D. Was this from his Kadomtsev–Petviashvili model?

R. Z. S. Yes, yes! And, in order to keep it within our picture, it was very important to prevent concentrating the focus of this nonlinear process to be along narrow, angular channels into shock waves. Finally, when I was already in Moscow, I brought Sam Moiseev into this problem. We suggested a kind of hybrid strong acoustic turbulence. In this, we had strong acoustic spectra and a very small contribution from vortices. Rolled-up vortices would be like entities with zero frequency — finite-k number — to provide small angle scattering for acoustic waves. Even a tiny contribution from such scattering would immediately smear out the narrow channels for shock wave formation. These vortices did not change the final spectral power-law but justified that we had angular isotropy. So, we published it later. I think it was rather straightforward and simple for this kind of strong turbulence. Zakharov tried to employ some of his students to do a numerical simulation of strong acoustic turbulence, but I think it was very important to have little vortex scatterers. And, it was another example of our life-long competition with Boris.

## 6 Fusion: drift wave turbulence and the T3 tokamak

This section deals with Sagdeev's work in magnetic fusion. It presents the key question of confinement, specifically Bohm (unfavorable) vs. gyro-Bohm (favorable) scaling of the energy confinement time. This issue is a consequence of the appearance of both the system size ($a$) and the gyro-radius ($\rho$) scales in the theory and of their disparity ($\rho/a \ll 1$). This is in marked contrast to the classic problem of turbulent transport in pipe flow, where (apart from the viscous sublayer at the wall), only the system size enters. Sagdeev discusses his work on drift waves, drift instabilities and drift wave turbulence, which drive confinement degradation. These processes fall outside the domain of magnetohydrodynamics and define the subject of "microturbulence". Sagdeev mentions Alec Galeev — at that time, another young star of his school. Sagdeev also recalls his discussions with western plasma physicists at Trieste and elsewhere. We also learn of the early Russian fusion program and the success of the T3 tokamak at the Kurchatov Institute.

P. D. Let's turn to the subject of magnetic confinement physics. Before we delve into the science, might you discuss some of the main players at the Kurchatov Institute, when you joined and later in your career?

R. Z. S. When I joined the Fusion Division of the Kurchatov Institute under Artsimovich and Leontovich, I met a number of people whose names are well known, through the history of fusion-related plasma. It was quite impressive. There was, of course, Shafranov. He was one of the youngest in theory group at that time. The most senior, except for Leontovich, was Braginsky. I remember him very well from that time. His major contribution was to develop a two-fluid, hydrodynamic plasma model, with all the transport coefficients calculated accurately. It was very impressive. It's interesting that, nowadays, when people need to use a two-fluid model of plasma, they typically use Braginsky equations rather than Harold Grad's equations, which are a little more complicated and less physically transparent than Braginsky's. Boris Kadomtsev joined the Kurchatov group somewhat later. He came after I joined, but in a senior, well-established position. He came after spending a few years working on classified, plasma-related problems. Most likely, he was also part of a group tangential to the weapons program, and he quickly became one of the leaders.

Another part of the group had interesting people. One of them was [B. A.] Trubnikov. He was a contemporary of Shafranov, about the same age. I remember there was a lot of pride in the Leontovich Theory Group. They were the first to pay attention to cyclotron radiation into higher harmonics, which was, potentially, a dangerous threat to energy balance. Trubnikov became very prominent in that group, because he was assigned by Leontovich to do all the calculations. The concept was straightforward: If you look at cyclotron radiation to the main frequency — that is, cyclotron frequency by individual electrons — it is enormous, but everyone understood that it would be optically trapped inside the plasma, which is opaque. For the first few years of controlled fusion development, people more or less ignored energy losses due to cyclotron radiation because of that. Then, Leontovich and his group thought that they should consider higher frequency multiples of cyclotron radiation — the mean free path of which would be of the order of the size of the plasma column. People who were working in the transport of photons knew that such effects usually take place in any kind of system, not specifically related to fusion or cyclotron radiation. So, Trubnikov was assigned to do the calculations.

By the time I joined, the work was already finished, and the Russians thought it would be a very important contribution when the program was declassified at the Second Geneva Conference [Second United Nations International Conference on the Peaceful Uses of Atomic Energy, 1958]. At the end of that meeting, the conclusion that the Russian bosses promoted was that, in terms of theoretical understandings of controlled fusion-related problems, we Russians were equal to the Americans — that is, with the Matterhorn Project — or at least not far behind. While Americans had an advantage in understanding magnetic transform and magnetic surfaces in toroidal systems, we compensated by better understanding the losses at multiples of the cyclotron frequency. The Kurchatov leadership considered the introduction of the magnetic transform idea of a stellarator almost like an offense. It was completely missed by the Soviet controlled fusion program of that time. One particularly painful incident happened when the Kurchatov team got preliminary abstracts of some American papers to be delivered at Geneva in '58, a few months in advance of the conference. When they reached Spitzer's major paper, which actually started with a simple example of magnetic transform in a Figure 8 geometry, Artsimovich's reaction was, "Oh yes, we know it”. In his general talk at Geneva, he said, “We understand Figure 8”. Leontovich was very upset and very angry. There was a kind of falling out between Artsimovich and Leontovich, because Leontovich was an extremely honest man, intellectually. He didn’t like Artsimovich’s attempt to take credit for something the group did not develop.

Who else was in that group? There was a lot of effort on elementary processes in plasma, line broadening, cross sections and so on — especially in plasmas not fully ionized. Vladimir Kogan was a leader of that group. Then, at approximately the same time I joined Kurchatov, there was another recruit, Alexey Morozov, who was already an older guy. Almost immediately after joining, he started to work on particle orbits for stellerators. His main interest, his “hobby”, was the invention of the plasma thruster. This type of thruster is called the Hall Thruster.

Artsimovich helped Morozov to build a little experimental lab. They started to design the very first thrusters. Since that time, it has become like a Kalashnikov assault weapon. Morozov’s Hall Thrusters are mass-produced. There is still a company in Russia that makes these. They are used for orbital corrections for station-keeping on Russian meteorological and telecommunication satellites. There is a recent story that Europeans are trying to build a constellation of hundreds of telecommunication satellites at an altitude slightly below 1,000 kilometers. The plan is for this to provide cheap Internet access for areas on the globe where it is not available. Using these, I think 800 to

900 satellites — all of them would be controlled by Morozov's design, which is mass-produced by Russians. That is a general description of Kurchatov at that time.

P. D. When did Rudakov join?

R. Z. S. Rudakov joined one month after I joined. We were contemporaries, and we were put in one room. When I departed the Kurchatov Institute for Novosibirsk in 1961, Rudakov left the Leontovich division. He moved to work with a completely different group in fusion led by [Yevgeny, or E. K.] Zavoisky. Zavoisky discovered electron paramagnetic resonance (often called electron spin resonance), and then was taken into the weapons program by Kurchatov. In the late '50s, he was brought by Kurchatov to work on controlled fusion. His new idea was to use high frequency heating, like acoustic resonance for magnetosonic waves in plasmas. Eventually, he turned toward what is called *turbulent heating*. Zavoisky brought Rudakov to this project. Rudakov became Chief Theorist of that division, until he left for a Troitsk affiliation to build a strong electron beam experiment in the late '60s.

My final interaction with Rudakov unfolded like this: Rudakov had a very bright, young student, who developed into a promising young plasma theorist. I invited this student to move to Novosibirsk and join my group at the Budker Institute. The name of this fellow was Dimitri Ryutov. After I left Novosibirsk, Ryutov was eventually made of head of plasma theory by Budker. He is now at Livermore, too.

P. D. Now, to physics! In the early 1960s, I believe, you, with Leonid Rudakov and others, developed the basic theory of what we now call the *drift wave* and the *ion temperature gradient,* or *negative compression ITG*, *mode*. This is amazing, because people in the late '50s were still focused on basic, ideal MHD, like the Energy Principle. The tokamak had not been built yet. What is the story behind these developments, and what drove you to work on these instabilities, which were far ahead of their time?

R. Z. S. I can tell you how it happened. The basic concept of MHD instabilities already was well understood. The Shafranov–Kruskal criterion had been used since the mid '50s.

P. D. Including resistive MHD?

R. Z. S. Resistive MHD came later, but ideal MHD and Energy Principle were already understood. In Russia, it was not formulated as elegantly as it was at Princeton — but still, the Russians were using an analog of the Energy Principle.

There were two things happening. First of all, Bohm diffusion.[4] I remember a number of conversations with Artsimovich, who said that everything we were doing, in the end, would be dependent on whether Bohm was right — or, at least, whether what he had in mind was relevant. The word *microinstability* was in existence in Russia, and then—

P. D. At what point was that?

R. Z. S. When I joined Kurchatov, the word was already being used at Kurchatov. Nobody was working on it in our group, but Kurchatov, who tried to supervise from the outside, and maybe some other people mentioned it. He seemed to have a mission. He wanted to do something else, not only bombs. He wanted to be "Father of Controlled Fusion".

P. D. Who coined the term microinstability? Was it Kurchatov?

R. Z. S. I don't know. It was probably Kurchatov. If MHD was a "macro-", it was a "micro" — that may have been his thinking. Kurchatov didn't see Artsimovich and Leontovich doing any specific job on that topic. He contacted Nikolay

[4] The scaling of the thermal diffusivity of a confined plasma is of paramount importance to magnetic fusion, as it determines the energy confinement time, which enters the Lawson criterion for ignition. Bohm diffusion — named for David Bohm, who first proposed it in a somewhat mysterious paper in 1949 — has the scaling $D_B \sim \rho\, c_s$, where $\rho$ is the (ion) gyroradius and $c_s$ is the speed of sound in plasma. It is pessimistic in that it predicts that no improvement in diffusion occurs in a larger device. The alternative scaling is gyro-Bohm diffusion, where $D_{GB} \sim (\rho/L) D_B$. Here, $L$ is, say, the radius of the confinement device; $\rho/L \ll 1$. $D_{GB}$ predicts "bigger is better", and thus is more optimistic.

Bogolyubov, the great Russian theoretical physicist, and asked him to do some work on this subject. You probably remember that Bogolyubov, years before that, published a paper about the drift approximation, the drift expansion in mechanics. But, Bogolyubov was also familiar with things like the foundation of statistical physics.

So, what was happening on microinstability already existed when I joined. There was a little office inside our department, where a pupil of Bogolyubov named [Yu] Tserkovnikov, sat. He did not report to Artsimovich and Leontovich; he was working under the guidance of Bogolyubov.

Essentially, what they were doing was to develop a complete kinetic consideration of plasma and magnetic fields by looking for some unusual instability root in the presence of nonuniformity across magnetic fields. Leontovich kept telling me they had some results (even papers coming), but they couldn't reproduce them and did not understand the final result. The moment we obtained guiding center kinetics with Rudakov — a useful tool — we thought, *Why not consider the same type of problems in a nonuniform plasma using a much simpler approach?* The first thing we considered was nonuniform plasma with zero wave number along magnetic field line. It's a purely perpendicular perturbation. We managed to reproduce one of the instabilities from Tserkovnikov's and Bogolyubov's very complicated calculations, which nobody else could repeat. It's very simple: If you have a distribution in a magnetic field, which is non-uniform, diamagnetic plasma would then have an instability, like an interchange. We published it.

P. D. That is really more like MHD instability, isn't it?

R. Z. S. It's similar to MHD, but in kinetic terms, it can go to shorter wavelengths. We published our results in an article, with not much excitement afterwards. It was rather straightforward, in terms of the physics. Then, one day in the late '50s, I think, we thought: *Since we have a simple tool, the guiding center theory — why don't we now bring parallel wave number (*$k_{\parallel}$*) into the picture?* Also, consider the separation of the two species.

P. D. You are referring to consideration of the two species — the fact that ions drift primarily across the field lines, while electrons move quickly along them?

R. Z. S. Essentially, the standard thing you get is the drift frequency due to the density gradient. But, if you have an ion temperature gradient (ITG), you can also get the ITG — almost a kind of hydrodynamic mode.

Then, we went to talk to Artsimovich and Leontovich. We were, on the one hand, trying to understand what Bogolyubov's guys were doing, and on the other hand, trying to see what kind of microphysics could be hiding behind Bohm's claim. So, the ITG mode was the first publication. I think it came at the end of

1959, roughly. Then came the simple zero Larmor radius density gradient drift mode and so on. We sent the paper in to *Doklady* [*Proceedings of the USSR Academy of Sciences*]. But, in writing this paper, there was a major mistake. I still feel a bit ashamed of that. Intuitively, it's understandable that this discovery was of great importance. We hurried to publish it, because, already in the new field of plasma physics, there were two centers of competition inside our division at Kurchatov — Kadomtsev, Rudakov and myself, and other younger guys. We were competing with them. At the same time, some of the "older" people like Shafranov and Braginsky were still involved in MHD-type research. Shafranov kept advancing theories of toroidal confinement, while Braginsky eventually met success with hydromagnetic dynamo theory. We wanted to publish our work very quickly. Leontovich would read each article and make a lot of comments. He was somewhat critical of the way we wrote the first draft. He said, "No, it is not ready for publication. You have to go back and rewrite it". For some reason, I became very angry. I slammed the door and left. This led to conflict with Leontovich. In the next few days, Artsimovich had to intervene to smooth things over with Leontovich. The paper was sent to *Doklady*. That was the first paper.

P. D. Did you do any kind of estimates of nonlinear saturation? Did you start to see whether you got Bohm scaling or not, even at the level of dimensional arguments?

R. Z. S. For the drift mode instability, you find Bohm scaling using a simple mixing length estimate. The growth rate is of the same order of magnitude as real part of frequency. It is very clear.

Then, I remember I went to an interesting, small conference on instability at the Culham Centre [for Fusion Energy, United Kingdom], in 1960, probably. Marshall was there. We started to discuss these instabilities with Marshall. Whenever we touched on electron half-residual contribution, Marshall said, “Why don’t we look at magnetic shear?” So, we did some of these first estimates with Marshall, thinking, *Okay* $k_{\parallel}$ *would rise as distance from the resonant surface multiplied by the shear parameter. At a sufficiently large distance, it would be easy to stabilize the mode by shear*. ITG remained outside this.

At that time, I was leaving Kurchatov. I moved to Novosibirsk and accepted Budker’s invitation. The first thing I did was to assemble my first group: Oraevskii and Alec Galeev, who joined when he was still a student. I said, “Let’s do a comprehensive analysis of all these drift instabilities”’ and that was the major task we undertook. At the Salzburg IAEA meeting, Marshall came up with finite Larmor radius stabilization of the interchange. From a methodological perspective, it came through Bessel functions. I said to my group, “Without doing all the calculations, I can now take our old dispersion relation with Rudakov and insert a Bessel function in the proper place, instead of a factor of unity”. We immediately got finite Larmor radius destabilization for different modes! Of course, before we published it, we had to do integration along the particle trajectory, so as to have a real derivation.

Alec Galeev, who was still a student, did a very professional job. He was much quicker than anyone else in my group in doing this type of calculation. Very soon we knew everything about the influence of shear on any kind of drift wave. The only remaining problem was to see whether we would obtain normal modes, so absolute instability can occur. After that was considered, I said to Leontovich, "I have a bright grad student, and I would like to send him to seminar at Kurchatov to talk about the impact of shear on drift modes". Alec went. It was the first time he was exposed to the broader community. Also, some similar work by [A. B.] Mikhailovskii was already happening at Kurchatov. I got a postcard from Leontovich. It was very interesting. He said, "You, Roald, behaved like Balda, the fox hero in Russian fairy tales, who is depicted in one of Pushkin's poems. When Balda had to compete with a clergyman in a race — instead of participating in the competition, himself — Balda entered his younger 'brother', the rabbit". Leontovich said, "You did the same. The final score is 5 against 3, in favor of the Novosibirsk group".

The major conclusion was that most of the modes should be normal. You can find normal modes related to the density gradient and so on. The collisional drift instability was not still there, so Bohm was out of picture. This is because, if you don't have a strong temperature gradient, you don't have a strong growth rate on order of the real part of the frequency. And then, somehow — accidentally — I was trying to play the game, too. We hoped that if you introduce a little friction of electrons with ions, and if you have a sufficiently long system, you will have a growth rate of the order of the real frequency. Then, immediately, you can construct Bohm diffusion from simple mixing arguments. Moreover, in Bohm's article, he mentions there was some critical magnetic field that would separate stable and unstable domains. You will also get it if you fix the mean free path. I asked Moiseev to check all of my calculations, and then we sent the paper for publication. I think the title of the paper had some version of an explanation for Bohm [Moiseev and Sagdeev 1963]. It was clear that if you have a finite length, or if you have magnetic shear, it wouldn't work. It was very easy to suppress this collisional drift instability. It was 1962 or 1963 by this time. Then, when I was in Trieste, we wrote a paper with Marshall and Bruno about ITG in a sheared magnetic field [Coppi et al. 1967]. Using mixing length arguments, we recovered gyro-Bohm scaling for ITG.

P. D. What's amazing is this was all driven by Bohm's rather speculative idea — there were no experiments. What happened when the T3 results came?

R. Z. S. The main conclusion Artsimovich talked about was that we proved Bohm was not working. If there is any Bohm, it was not in this particular tokamak.

It was very interesting. When there was a celebration of Artsimovich's 60th birthday — I was still in Novosibirsk, it was about 1968 or 1969 — we decided to prepare a present for him. With Vladimir Zakharov, we designed a special medal.

The laboratory workshop made it. On one side of the medal, we said, "For defeating Bohm diffusion". On opposite side, we said, "This medal should be put on a jacket on the same side as a medal for capturing Prague". It was just after the infamous [Soviet] invasion in 1968, and obviously a kind of politically sensitive joke.

But, for Artsimovich, it was very important that it was not Bohm. Moreover, in his analysis of experimental data, he concluded that the scaling follows neoclassical plateau-like trends for heat conduction.

P. D. How did you guys address the issue of the ITG back then? My understanding of the T3 results was that it indicated a significant electron anomaly that then pointed toward some kind of drift wave? Of course, back then, no one had a clue, whatsoever, as to the ion temperature profile. That only came in the early-to-mid '80s.

R. Z. S. At the time of the T3 results, we had already put aside all the drift wave anomalous transport topics. We were busy trying to find something related to neoclassical. I called Artsimovich and said, "I would like to come to give a talk". I went to Moscow and gave a talk about neoclassical. This is how he learned of it.

P. D. So, when did you begin to work on neoclassical theory?

R. Z. S. The origin is from the Trieste time (1966) of joint activity with Marshall Rosenbluth and his team. When we came to Trieste with Alec, in the very first conversation with Marshall, the idea was to have a swap. They would teach us about toroidal plasmas, and we would talk about nonlinear plasma theory. This is how Marshall arranged for the series of talks.

P. D. And those talks were, on your side, the origin of *Nonlinear Plasma Theory* [Sadgeev 1969a]?

R. Z. S. Yes. Tom O'Neil and David Book were assigned to take notes. In Trieste, David Book was very helpful, because his Russian was very good. As part of this arrangement, Marshall suggested that Herb Berk and Alec should study the particle distribution function in tokamak geometry in a nearly collisionless case. He wondered whether there are any analogs to loss cones. This article was published even before Marshall left. They had discovered, of course, bananas [banana diffusion], transiting particles and some kind of wiggles on the distribution function in the intermediate region between bananas and transiting particles.

Later, Alec and I were alone in Trieste for a couple of months. Every other member had left, and only the two of us stayed, trying to see what we should do. We were looking at this distribution, at the wiggles as a function of $v_{\parallel}$. I noticed that there were no obvious loss cones, but there were some places that might be sensitive to Coulomb collisions, especially small-angle scattering. The first thing was a straightforward estimate for banana diffusion with an enhanced collision rate. Immediately, without any theory, you get something substantially greater than simple old classical theory-based expectations. The second thing was: What happens if we increase collisions? So, a very simple, qualitative idea came before we did any theory. The idea was that there might be some similarity with nonlinear Landau damping. There must be some of kind of regime, where it is collisional — but the collision frequency would not enter, similar to Landau. Then, what if you consider particles circulating along magnetic fields that are nonuniform and anharmonic? It's like an interaction of magnetic fields with magnetic standing waves. I saw there must be a Landau damping-type effect. This is how the concept of plateau diffusion developed and very quickly. We knew the calculation would be quite cumbersome. The immediate idea was to utilize the Zakharov calculation for Landau damping collisional kinetics with boundary layers, which was very similar. Alec, very quickly, took it — even using the expansion in Laguerre polynomials, which Zakharov was using — and translated it to the neoclassical problem. So, before we left Trieste, we already had a backbone for the theory. That's how it happened.

The interesting thing about this is that it's still not stressed. In the very first paper, we proposed that — nominally, if you treat the problem kinetically — the neoclassical ion–particle diffusion is different from that for the electrons. So, the system is not automatically ambipolar! There should be an electric field established to balance charge. A number of people criticized this. Paul Rutherford at Princeton was the first to react to our paper. He said that he reproduced everything, but he did not agree with the absence of automatic ambipolarity and the need for an electrostatic field to restore it. Lev Kovrizhnikh at Lebedev Institute did the same. Then, we had to respond. We published a short article in *Doklady* with the title "A paradox in the diffusion of plasma in toroidal magnetic traps" [Sagdeev and Galeev 1969b]. This article explained that consideration of generalized momentum conservation, including free motion of the charged particle along curved field lines, would force some change in the velocity in a parallel direction. There can be greater perpendicular diffusion for ions than electrons. We explained this.

To my surprise, many years later, when I came to the U.S. — I was talking to people about this neoclassical theory. Tom Antonsen came to me and said, "Roald, I can give you very simple proof that it is automatically ambipolar". I was surprised. I said, "How did you do it? Where did you get it?" He produced notes of, I think, a kind of preprint of Marshall's lecture at a summer school. If you introduce a Maxwellian, unperturbed distribution and then linearize, it looks like ambipolar. So, I was surprised to find that Marshall wrote this.

When I met Marshall early in the '90s, I said, "Marshall, this is wrong. I can give you simple, physical arguments for how ions jump for a banana width in an ion–ion collision. Then, you have considered the collision of one banana and one transit particle — such a strong collision that the banana becomes a transit after that collision, and the previous transit also remains. They change position immediately". Marshall looked at this and said it was interesting. But then, he said, "Can you show where *my* mistake is?" I said, "It's very simple, because you imposed an initial Maxwellian distribution and did not let it change along the parallel dimension". In the end, I found a rather elegant way to get out of this interaction with Marshall. When Novakovskii and I were writing an article about neoclassical rotation [Novakovskii et al. 1997], we reiterated all of this. I invited Marshall to be a co-author, and it was published, finally. But, interestingly, a majority of people — and in the textbooks — think it's automatically ambipolar.

P. D. Let's continue on the subject of transport and neoclassical theory. Where did the name "neoclassical" come from?

R. Z. S. This is also very interesting. Do you remember Burt Fried from UCLA [University of California Los Angeles]? He, at one point — I think it was the early '70s — started a little magazine, a journal related to controlled fusion.

P. D. Yes, you mean *Comments in Plasma Physics and Controlled Fusion*.

R. Z. S. Yes. It contained very short, little papers. He said, "Why don't you publish a short review on transport?" During that conversation, I said, "You know, this is very different from classical transport". He said, "Yes. It must be called ‘neoclassical.’” It was an appropriate name.

P. D. Did you ever, after T3, take up microinstability theory again? It seems like that phase of your work began early and stopped fairly early.

R. Z. S. I was in a transition. I left Novosibirsk in 1970 and came to Moscow, searching for what to do next. For two years, I was at the Institute of High Temperatures, trying to assemble a small group. I brought Alec Galeev and then Shapiro from Kharkhov. There, I was doing something related to laser physics — nonlinear physics of laser plasma interaction. We even published a paper in *JETP* with Marshall and Alec Galeev. We calculated the parametric processes related to laser–plasma interaction, including growth rates of instability and so on. And then, suddenly, in 1973, I was brought to IKI as a director. Then, until maybe the late ‘70s, I was unable to do almost any research due to my administrative duties.

P. D. Marshall once told me a story about how you and he set up an apartment in Paris, for a time, and cranked out all the laser–plasma instability theory. How did that collaboration develop?

R. Z. S. This is how it happened. It was 1972. I was invited to give lectures at a summer school on space plasma and cosmic electrodynamics. It was a very interesting time for me. I spent more than a month giving, maybe, 15 or 20 lectures. Tommy Gold was lecturing in the same school, and we became good friends.

During that period, there was an international conference on ionized gases in Grenoble. I had a little paper, so I went to Grenoble from Brittany in northern France. There, I met Marshall, and we had an interesting interaction. I said, “Marshall, if you have the time, why don't you come to this summer school in this picturesque place on the sea?” He agreed, changed his itinerary, and we went. Everything would have been great, except Marshall got food poisoning from eating oysters. We frequently ate oysters — every day! Then, in a couple of days, came the end of my time at the summer school. The French organizer of the summer school said, “Why don’t you stay in Paris for the next week?” I was not at IKI yet and was kind of a free man. The French organizer gave me a key to his apartment in Paris. He, himself, was in Toulouse. Marshall and I settled in Paris for a few days. The refrigerator in the apartment was full of good champagne, and we drafted this laser paper and drank all of the champagne.

P. D. What happened to the Russian tokamak program? There was the great triumph of T3, but there were not many developments after that. The question I'm driving at is: Why isn’t “Alcator scaling” known as “Kurchatov scaling"?[5] If you look at the sequence of events: There was T3, and then there was what Princeton started. This included the ATC [Adiabatic Toroidal Compressor] and the PLT [Princeton Large Torus], which demonstrated that neutral beams worked. Then, really, the next big thing was the Alcator tokamak program. That group, of course, found Alcator scaling — the $1 / n$ scaling of the thermal diffusivity — which was very interesting. Then, they (as part of Alcator C) went on to do the pellet experiments. These were what caused the community to take the ITG mode seriously. Prior to that, the conventional wisdom was that "ions were neoclassical". What happened to the Kurchatov program after T3?

R. Z. S. I think what happened was that Artsimovich was still alive, and he accepted the next major step to build T10 — a much bigger machine. They started to design and work on it. When Artsimovich died in February of '73, there was no leader to take over and carry out the experiment. The experimentalists in his group — Razumova, Strelkov, Mukhovatov — they were key members of the team but did not have the chance to produce the needed leadership.

And while T10 was going on, they were also looking to replace the head of the fusion program at Kurchatov. I was still a free man, and I was approached by some colleagues from Kurchatov with the question of whether I'd be interested in coming back to succeed Artsimovich. I don't remember what my answer was, but

[5] The T3 program was notable as the first successful demonstration of confinement by tokamak. It was also notable as an early example of international collaboration on magnetic fusion, which occurred at the height of the Cold War. The measurements of electron temperature on T3 were obtained using a Thompson scattering system that was provided and operated by a team from the United Kingdom, led by the late Derek C. Robinson. Alcator scaling refers to the favorable scaling of energy confinement with density ($\tau_e \sim n$), discovered on the Alcator A tokamak.

they picked Boris Kadomtsev. I don't think he was very fond of the organization of construction — it's a very different life.

So, back to your question of what happened to T10. The project was poorly conceived from the beginning. The idea of a compact torus at high density had been on the table since the late '60s. Velikhov, who was in charge of the Kurchatov outlet in Troitsk, started to build this compact machine.

P. D. So, this was effectively a Russian Alcator — a high-field torus?

R. Z. S. It could have been — it never worked. They spent years on it, but nothing happened. I think one reason was that Velikhov did not specifically focus himself on one thing. He was spread out across programs, including a lot of military applications — for example, airborne lasers for missile defense. He got the Lenin prize for that, before he became an anti-Star Wars warrior. So, the compact torus died. Actually, it is interesting what Russians are saying now: "We will be ready to accommodate Ignitor". They said this because we have an infrastructure to pump the energy at Troitsk. So, I think there was no dynamic leader.

P. D. The death of Artsimovich slowed the progress tremendously. Was there any contact between Artsimovich and Bruno Coppi on the issue of the compact high-field approach?

R. Z. S. I don't know. Bruno might be able to tell you. When Artsimovich visited MIT, not long before he passed away, Bruno hosted him. So, maybe — they spent lot of time together. Bruno could have initiated this conversation.

P. D. Did your school of microinstability theory ever worry about fueling? Another interesting question, which is part of the story of drift waves and ITGs, is the question of the off-diagonal components of the transport matrix. The inward pinch is particularly important. I suppose Bruno Coppi should be credited for the first serious job on this in '78, but I'm wondering what the Russian school did on this subject.

R. Z. S. There was pinching of bananas, which finally was called *Galeev–Ware pinching*.

P. D. That was a neoclassical pinching — a weak effect. My question addresses the possibility of turbulent pinching — an up-density gradient component of the particle flux, driven by temperature gradients, etc.

R. Z. S. No. I was already out of fusion plasma at that time.

P. D. One other area you contributed to was the theory of convective cells and zonal flows, specifically the Sagdeev–Shapiro–Shevchenko paper in 1978. Really, was this was done as a response to the Dawson and Okuda simulations, in isolation from questions of confinement?

R. Z. S. Absolutely! The idea was that if they observed convective cells in plasma simulation, what could be the physical explanation of the origin of convective cells?

P. D. So, it was never connected to the bigger issue of what's going on in regulating transport?

R. Z. S. No, the origin of the paper was to explain the simulations. Then, we understood that if there is a parametric coupling, there must be something related to physical reality in plasmas. But, we stopped doing it. Then, we started this number of new space projects related to Halley's Comet and so on. Shapiro and Shevchenko were both strongly involved in these space physics projects. They were out of regular plasma physics and even getting involved with some kind of technical engineering inventions, in which Shapiro helped me a lot, at that time.

P. D. I think we have covered magnetic fusion.

## 7 Space plasma physics and the IKI years

This final section deals with Sagdeev's work on space physics and his leadership of IKI (Institute for Cosmic Research, or Space Research Institute of the Russian Academy of Sciences), which was the center of the Russian space program. Sagdeev discusses several applications of plasma physics to space problems. He discusses the Venusian atmospheric circulation (super-rotation) problem and applications of compressible turbulence to astrophysical problems. Sagdeev also recounts some of his experiences as the Director of IKI and speaks of some of the challenges he faced at that time.

P. D. We're going to turn now to the subject of space plasma physics and Roald's years with the IKI Institute for Cosmic Research. For this conversation, we're joined by Dr. Mischa [Mikhail A.] Malkov from UC San Diego, who worked with Roald at IKI for many years. Roald, can you describe the questions in space physics that captivated you when you began working at IKI? What kinds of interactions did you have with people such as Charlie Kennel and René Pellat?

R. Z. S. I came to work at IKI in the summer of 1973. By that time, there was a rather well-established experimental part of IKI dealing with near-Earth phenomenon, including radiation, plasmas and so on. First, I had to learn what they were doing and what contributions they were making, compared to American space physicists. There were several groups; they did not interact very well with one another. One of the strongest groups was led by Konstantin Gringauz. He had an important impact on early space plasma science. Gringauz was one of the first discoverers of solar wind plasma. Everything was based on simple detectors of ions using a concept called a *Faraday cup*. There was one experiment after another, because the early Soviet space program provided plenty of opportunity to fly such instruments — to low and high Earth orbits, to the moon and to early launches towards Mars and Venus. It was rather good at that time. The problem was that the sophistication of these instruments did not progress, substantially, in their ability to measure nuances of ions, energy spectra and angular spectra of the electrons. Obviously, there was a problem with theory at that time, so my first intervention was to bring on Alec Galeev as Chief Theorist. He converted to space plasma theory rather quickly and attracted new people to work at IKI. By the end of '70s, it became a rather substantial, theoretical group. Vitali [D.] Shapiro and Valentin Shevchenko joined. Lev Zelenyi was a young theorist at the time. He had already been recruited by IKI and became a close collaborator of Alec Galeev. Mischa, did you join around that time?

M. M. Later — after Lev, actually.

R. Z. S. When did you join IKI?

M. M. 1976.

R. Z. S. I can tell you the story of how Mischa joined IKI. An interesting collaboration was beginning with the Europeans. I was invited to go to Munich and Garching by Reimar Lüst of the Max Planck Society. He was originally Director of the Institute of Extraterrestrial Physics at Garching. So, I went, and there was a big exhibition of Soviet technology in Munich. My first duty was to attend and speak. The Soviet Ambassador to Germany, Valentin Falin, came, and I met him for the first time. He was a young diplomat, who was considered to be a representative of the Russian progressive wing. We've been good friends since that time. Years later, he, too, joined the Gorbachev team. At the Munchen meeting, he was accompanied with his advisor and assistant. Mischa, what was your father's position in his team?

M. M. He was, at first, I think, the First Secretary of the [Soviet] Embassy.

R. Z. S. So, I spent a couple of days with that team. Then, this assistant to the First Secretary of the Embassy said to me, "I have a son living in Moscow, who is about to graduate from the engineering physics institute. He is in theory and would be interested in finding out if he could work for IKI, if you would approve him". I said, "Okay, this is my phone number. Have your son call me". A few weeks later, I got a phone call from Mischa. He introduced himself. I said, “Please come and visit IKI to talk more”. Everyone liked him. Were you first introduced to Alec Galeev?

M. M. Not at first. I was brought to the department where people were doing mostly computer programming, which was, somewhat, my vocation. I was trained as a particle physicist. Computer programming was, of course, related but not exactly what I was planning to do.

R. Z. S. You know, I paid lot of attention to software as well as hardware. Some of my computer gurus went to Silicon Valley in America. The head of the software division, [D.A.] Usikov, is now in Silicon Valley. He was a co-author with me on a book [Sagdeev et al. 1990]. So, it became a rather good group.

M. M. It was good of you to pay attention to computing. Technically, I was in Galeev’s department with Vitali Shapiro. He was focused on pure theory, but I was trying to be involved in computing.

R. Z. S. In the meantime, what was happening with George Zaslavsky?

M. M. He came [to IKI] later.

R. Z. S. After I left Akademgorodok [Novosibirsk, Russia], George took a job at Krasnoyarsk, farther into Siberia. We sometimes communicated with him. Then, I asked, “Why don’t you come to Moscow to work at IKI?” Towards the end of ‘70s, I managed to bring him there. The problem was getting permission for him to live in Moscow. It was called the Moscow Propiska, and it was very difficult to get. Each time, it took a lot of effort to get an apartment. So, he moved. Thus, George became a member of the IKI theory team — and very valuable. Then, later, [Zaslavsky] recruited you to join his group.

M. M. No, I had been in the Galeev department until about 1985 or so. Later, you formed the Nonlinear Center, and I moved there.

R. Z. S. At IKI, at that time, we were interested in nonlinear stuff, in the case of collisionless shocks. I don’t think we made any new, relevant work in theory, but the idea was to mobilize and focus experimentation on measuring fine-scale structure in the bow shock or travelling solar wind shocks. We helped to launch a project called Intershock. We used an international framework, which allowed us to have a dedicated spacecraft for the shocks. The telemetry beat rate was very low at that time — not sufficient to probe the fine structure. The idea of Project Intershock was to have a look memory that would record everything with an excellent beat rate and keep the look memory on the spacecraft for a short time. If anything triggered special interest during this time interval, like a shock crossing, it could then slowly release this high-resolution telemetry by a communication link. It was built. Alec was gradually becoming more and more influential, even in the experimental program. Finally, I thought it was time to make him the head of all space-related plasma experiment and theory. So then, Shapiro was somewhat independent of Alec. We spent some time in trying to develop the Zakharov model of wave collapse in plasma to build a theory of strong turbulence for Langmuir waves. We were looking for laser experiments, and we had our own facility.

P. D. I remember there was a series of simulation papers on strong Langmuir turbulence during that time.

R. Z. S. Yes. Interestingly, the colleague we collaborated with on simulation from the Keldysh Institute, Degtyaryov, was the same guy who was working with Zakharov — but, we thought that even one-dimensional problems might shed some light on nonlinear things.

One of the works — that was a little outside of plasma — we did with Shapiro. It tried to explain the circulation of red spots on Jupiter. We tried to use a soliton-type of approximation to explain how the shear flow would keep fueling energy into the continuous rotation of a red spot.

P. D. Was this a kind of Rossby soliton?

R. Z. S. Yes. So, I called Sedov and said, “I’d like to give a talk on fluid dynamics in space science”. He said, “Okay, come to my seminar”. This was one of the few places I had never given a talk. Later, [V. I.] Petriashvili picked up on that and published his own papers. But, conceptually, it was the same. We never went back to this research. Then, there was a lot of excitement about space plasmas in relation to Halley’s Comet, bow shock and Alfvénic turbulence due to the interaction of solar wind with pick-up ions. There was even an idea to use accelerated particles in Alfvénic turbulence as a kind of small-scale model for cosmic ray acceleration. We published a number of articles with Shapiro using quasilinear theory. I didn’t have much interaction with Alec at that time, because he was busy with administration. But, one important piece of work we did with Alec was the following: You remember anomalous ionization suggested by Alfvén? If you have rotating a plasma, what’s important is if the rotation velocity reaches a certain critical value, then suddenly, the onset of anomalous ionization occurs. For a number of years, there was kind of challenge — what kind of mechanism or instability would trigger this? In such cases of energy in ions, there is no way to collisionally transfer it to electrons in a short time. Looking through all the instabilities, we turned to the lower hybrid instability as an appropriate mechanism. We even published a paper with Alec, and Formisano joined from Europe. I think that research was a very important milestone to use of the Rosenbluth–Post loss cone instability. It’s a very similar model. They did not go into the quasilinear development, so Alec and I first developed a quasilinear model. It is still a kind of clean-cut, analytical model. You have a growth of the spectra of the waves, loss cones of the field and the amplitude of spectrums, gradually decreasing to zero (unlike one-dimensional quasilinear theory or Langmuir waves). Then, there was a brief excursion. I was in Europe — I think, maybe, it was before IKI — and there was a growing interest in reconnection. Bruno Coppi, with Guy Laval and [René] Pellat, published a paper about the collisionless tearing mode due to a small fraction of electrons. I thought it would be very easy to saturate at extremely low amplitude.

P. D. Yes. You just have to knock out the electrons in the electron layer.

R. Z. S. Absolutely, just to change their distribution a little bit. I suggested it. I had already suggested an answer to [V. S.] Pokhotelov. He did some additional calculations. I was joined by Karl Schindler from Germany. We published a paper with Schindler, also. Zelenyi, who is now an expert on reconnection, said it was the last nail in the coffin of the Coppi–Laval–Pellat theory. It triggered Alec to start a completely new series of papers. His idea was that if there is a non-zero magnetic field — even in the neutral layer, with a small, perpendicular component to magnetize electrons — then growth would have to come from ions. And, it would lead to much stronger instability.

P. D. So, you have magnetized electrons, and basically unmagnetized ions.

R. Z. S. Yes. Essentially, it led to a series of papers, which were continued by Zelenyi. Pellat started to do the same thing. There was a major confrontation between Alec and René, because René found a stabilizing effect. It's a compressible mode! Part of the energy would be invested in increasing the thermal energy of plasma, which according to René, completely compensated for the energy accessible for instability. And then, while Alec was ill and not working, Zelenyi told me that his team replaced the Harris profile exact solution with a measured profile of the magnetic field. We saw that thermal energy increased, and we are able to fully compensate for available free energy. So, when sufficient energy was released, instability occurred. They think they were winners in the competition with René. Unfortunately, Alec and René were not able to comprehend what happened: Alec Galeev was severely ill, and Pellat died in 2003.

The only competitor for Zelenyi and the current IKI work on reconnection was Jim Drake. There was an interesting episode several years ago. Jim Drake received the Maxwell Prize for his work on reconnection, and Zelenyi, in the same year, was awarded the COSPAR Prize for reconnection. There was an American Geophysical Union meeting in San Francisco, and Zelenyi was delivering his review article about the state of reconnection. I happened to be in the audience. I asked, "Lev, how is it possible that, simultaneously, you and Jim each got prizes for completely different explanations of reconnection? How do you explain that?” Zelenyi gave a very interesting answer. He said for several years at IKI, he and Drake were competing, and he was angry that Drake never referred to his publications with Alec Galeev. Then, one day, they were sitting together and discovered that they were examining two completely different questions in reconnection. Alec, I remember, started this study of reconnection to find a trigger mechanism, which would start substorms in the geomagnetic tail. Finally, Zelenyi thinks that they now have a theory for that, based on Alec’s original idea and new data related to the profile of the magnetic field. At the same time, Jim Drake was not interested in the onset of substorms — he was interested in the subsequent acceleration of the mechanism for particles through multiple islands in the transition regions. So, they finally signed a treaty of non-intervention. I think that, perhaps, the issue of onset was much more important than acceleration. Pat, what do you think?

P. D. I would tend to agree. I think it’s more difficult. As the energy is released, the plasma will find a way to accelerate particles. This presents many mechanisms worth exploring.

Might you discuss how the IKI effort on plasma astrophysics developed during these years?

R. Z. S. In the IKI period, before [Yakov Borisovich] Zeldovich and [Rashid A.] Sunyaev came, I thought it could be very important to bring together space physics-related plasma physics with astrophysical plasma physics. I tried to push Alec toward this more astrophysical problem. There was an interesting opportunity for me to send him to Harvard University for a few months.

P. D. Was this when [Robert] Rosner and [Giuseppe S.] Vaiana were there?

R. Z. S. Yes. While he was there, Alec did calculations with them. He took the idea of lower hybrid instability, which would transfer energy from ions to electrons in solar physics, to explain the origin of x-rays from fast electrons. They published an article and then published another article on how a similar mechanism could be a source of x-ray emission in astrophysics. It was a very well-quoted paper [Galeev et al. 1979]. When Zeldovich came, there was a brief moment when Alec was interacting with Sunyaev. They published a couple of joint papers. During all of this, I needed someone to take charge of all space physics-related plasma work, including experiments, and I pushed Alec into an even bigger administrative job. I don't think Alec was very active, except for his work on reconnection. His last interesting work was related to interaction with comets. There is Alfvénic-type turbulence triggered by interactions—

P. D. You're referring to pick-up ion mechanisms?

R. Z. S. Yes. It started first with Shapiro, I think.

M. M. When Charlie Kennel came to IKI in about 1986, '87, he published on shocklets and the interaction of pick-up ions in solar wind. This was observed ahead of the bow shock and cometary shocks.

R. Z. S. When George Zaslavsky came to work for IKI, he made my activities there more interesting and refreshing. I saw George at least once a week. We were interacting a lot again, going back to study chaos-related physics. We were also much better equipped with computational tools than during the previous Novosibirsk period. George brought a few younger people to develop the use of computational tools for chaos theory. One problem was Landau damping in the presence of a weak, perpendicular magnetic field. We did a lot of work on that. George finally came up with a convenient map that would cover this transition. I was still using it when I was here at Maryland. The very first personal IBM computer that IKI got was given to George's group to work on this problem. Mischa, how did you get in touch with Zaslavsky?

M. M. It was on that same problem.

R. Z. S. Who invited you? George?

M. M. I think he invited me to work on the particular case when the magnetic field goes to zero. I remember a very interesting remark by George when I derived the formula on how the transition $B$ (magnetic field) → 0 occurs, leading to regular Landau damping. I told him that I couldn't match his expansions to get this formula. We resolved this difference and later published together.

R. Z. S. Then, we had the idea to write a book. The English translation included software listings for the most popular chaotic maps. We tried to apply it to the orbit of comets. When comets are very far, an encounter with Jupiter or Saturn could create chaos in such an elongated orbit. Halley's Comet was on the border between these two things.

What else was happening with George at that time? One particular problem related to this paradox of disappearing Landau damping. It can be formulated in a simple way: You have a longitudinal, one-dimensional Langmuir wave. You superimpose a perpendicular magnetic field — a very simple dynamic system — so, you have a transition to chaos inside the system. We considered this to be so much "simpler" than in the usual approach to chaos in plasma, which is needed for quasilinear diffusion. There, you have to introduce a wide spectrum of individual Langmuir waves, by hand. Here, you only have one Langmuir wave and a magnetic field, and you can get a transition to chaos.

P. D. Is this related to Charles Karney's work [Karney 1978]?

M. M. Yes, but Karney didn't get an accurate result for $B \rightarrow 0$.

R. Z. S. Some of these things were never published, despite the fact that George published another book when he was at Courant [Institute of Mathematical Sciences, NYU]. For example, I, myself, used some of these maps to examine the case of a non-zero magnetic field in linear theory. When you have an imaginary part only at the delta functions at multiple resonances, consider what the map would look like and the actual motion of the particle in this simple configuration. You will see that, with a rising amplitude of the wave, each delta function resonance is broadened to a finite width, due to these multiple resonances. You can even find the shape of this broadened line and so on. I gave a talk at a Los Alamos colloquium, showing these big pictures and giving some simple formulas. Dennis Papadopoulos recently said to me that they are doing straightforward, numerical simulation, and there is a kind of increased anomalous damping near multiples of the cyclotron frequency. I think someone should finish this. It would be interesting work, if they could finish what Karney did originally and what George worked on. Interestingly, as a result of this interaction, IKI — for a brief moment — almost became the center of chaos science in the Soviet Union during that time.

One day, I talked with George regarding this map with a magnetic field. It was exhibiting something that was very similar to Arnold's calculations of diffusion in a higher dimension system. We decided to invite [Vladimir I.] Arnold to IKI and show him all these things. He came and spent a full day with us. It was a very good interaction with him. I think, for the first time, he saw chaotic transition and diffusion on a computer screen.

At the end of the day, I said, "Dima" — his nickname was Dima — "Is there any way I can help you at the Academy?" He thought and said, "Oh yes, you can help me! I have a pupil in my group, who just got his Ph.D. — but he cannot find a job because of his last name". In math, anti-Semitism was very strong at that time. Arnold said, "Can you take him to IKI?" I said, "No problem". His pupil was hired. Maybe you remember his name: Anatoly Neishtadt. We co-authored some publications on chaos. Eventually, we all left the Soviet Union; Russians were already scattered around the globe. Neishtadt worked as a researcher at IKI until early 2000. He even helped Zelenyi with some work on chaotic trajectories of particles inside the neutral layer of the geotail. Finally, he got a position in the U.K. He's in the U.K. now. So, that was my brief interaction with Arnold. Many people think he was a difficult man — or that was his reputation. But, I think we had a very warm and friendly meeting with him. So, that was another part of the activities in IKI.

As part of IKI's basic plasma work, I also continued an applied project from a previous administration. It was to help the military understand certain surface ocean phenomena, such as those that might be relevant to detecting the presence of submarines.

P. D. I see. So, this leads us to [Valentin Semenovich] Etkin.

R. Z. S. Yes. I wanted to close this department, because I wanted IKI to become open for international cooperation. But, I was unable to because of Etkin, who was on the scientific staff of IKI, then. He wrote a letter to the government, notifying them that I planned to close research on important topics related to national security. There was intervention from the government — even the KGB came to see what happening — and so, I was unable to close the department. I went to Etkin and said, “Valentin, why did you do it? This is not an honest way to fight a war”. He said, “This was the only way to save my job”. He confessed that he wrote the letter. At the end of his life, we became good friends. Okay, yes, he defended his turf.

Then, I took Sam Moiseev. After Novosibirsk, he went to Kharkov in the Ukraine. I wrote him and said, “Sam, there is a piece of science related to nonlinear dynamics of the ocean that has some importance for the military. I don’t want to be under the control of such people”. So, I created a theory division that was parallel to Etkin’s. They were interacting, so Sam had to change his profession into ocean and atmospheric dynamics.

One outcome of this was the vortex dynamo. This was based on the idea that if you have an alpha-effect in terrestrial atmospheric turbulence, it might create organized vortices. These might be the origin of hurricanes. There were several papers, and apparently in this case, Sam was doing something close to what was developed independently by Uriel Frisch.

P. D. Here, do you mean this is a version of what Uriel calls AKA — the anisotropic kinetic alpha effect? This is a kind of Reynolds stress-based model for the generation of flow.

R. Z. S. Absolutely. The problem with Sam was that I tried to persuade him to launch a major computational effort in support of the analytical theory, and it didn't happen during my tenure. For reasons I don't understand, he was not active in that endeavor. So, that's essentially what is to be said about theory in IKI.

P. D. Did you or Moiseev make a connection of that work on AKA tokamak to the question of the generation of zonal flows?

R. Z. S. No. Sam was absolutely fascinated with trying to explain the origin of hurricanes using these ideas.

P. D. You didn't have joint seminars with the Kurchatov Institute or other fusion groups?

R. Z. S. No, we didn't have a joint seminar. I think I traveled to Kurchatov a few times over a long period, mostly to see Leontovich when he was still alive.

M. M. But, plasma physicists from Kurchatov came to IKI for nearly every seminar. They came regularly.

R. Z. S. My last notable encounter with Leontovich was at Kurchatov to celebrate his 75th birthday. The plasma theory group had become large by then. During the birthday celebration, a huge gang of younger people gathered in the cafeteria. We had a banquet, and each of us had to say something about Leontovich. I told how, when I was here at Kurchatov, a younger Leontovich was capable of a phenomenal, contortionist trick — that it was remarkable, and I always admired his ability to do it. I said that I wondered whether any of the young guys in this big room — hundreds of people were attending — could reproduce such a trick. I put a chair in the middle of the room. Nobody came to sit. You know what happened then?! Leontovich went and sat on this chair, and started to move his leg, putting it in a highly contorted position! It's an unbelievable story!

Of course, I interacted a lot with Leontovich on Academy affairs and during elections of new members. He clearly identified himself as being in a position contrary to the political regime. Some of the candidates at the elections were coming from the government, and he was not afraid to cast a ballot against them. It was very good. If you go to Sakharov's memoirs, there are some warm words about Leontovich — but, Sakharov also writes of his disappointment. When he wanted Leontovich to join him in a kind of official protest about human rights, Leontovich did not join him openly. I understand it was a difficult period.

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