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My Life and Science

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Abstract

In this article, I tried to compress the events of my long life and scientific career into a readable manuscript. The choice of scientific problems in development of which I was involved and people with whom I contacted naturally is not complete. I hope, however, that my selection more or less correctly reflects my teaching activity and my participation in the enormous progress of quantum mechanics, statistical physics, and condensed matter physics in the second part of the previous and in the beginning of the current century.

KHARKOV, UKRAINE: HIGH SCHOOL AND UNIVERSITY, 1935–1953

Most of my life—62 years—was spent in the former Soviet Union. My childhood fell on the years of Stalin's purges and the Second World War. I remember the nearby bombing, long train journeys throughout the Soviet Union, and rusks issued on food ration cards. My dear parents, Leonid Pokrovsky and Raisa Razumovsky, were strong supporters of my natural curiosity and thirst for knowledge. I am thankful to my high school teachers, Antonina Semyonovna Nikiforova, Nadezhda Afanasievna Granovskaya, and Maria Lukinichna Bezuglaya, who carefully directed my education and morality.

In 1948, having made a difficult choice between physics and music, I entered the Department of Physics and Mathematics of Kharkov University. At that time the Department possessed extremely qualified faculty. Among them were such famous scientists as physicists Ilya Lifshitz, Alexander Akhiezer, Moisei Kaganov, Grigory Lyubarsky (he was scientific advisor of my Master's Thesis) and mathematicians Naum Akhiezer, Alexei Pogorelov, Vladimir Marchenko, Alexander Povzner, Boris Levin, and Naum Landkoff.

My fellow students were Mark Azbel; my good and dear friends Vladimir Maleev (biophysicist), Vitali Pustovalov, Vladimir Bengus (solid state physicists), Evgeni Mazel (high-power semiconductor devices), Feliks Ulinich, Elena Milankina, Kima Cherkasova, Mark Minz, Stanislav Dukhin (theorists); and my future wife Svetlana Krylova (optics of liquid crystals).

Departmental life was not calm. 1948 marked the start of a major antisemitic campaign in the Soviet Union. The Department Chairman, Prof. Abram Milner, was found to be too liberal. He was replaced in 1951 by a dimwitted and malicious man who was a communist party member. The new Chairman instituted disciplinary measures against his most vocal critics. On returning from a summer trip to the mountains, I discovered that I had been expelled from the university. This action automatically annulled my waiver of required military service. Fortunately, the call to military service took place only every six months. This gave me time to go to Moscow for an appeal to the Ministry of Higher Education. In this difficult process I discovered that, during this time of Stalin's purges, many people retained their humanity and conscience. Because of their help, I was able on December 31, 1951, at 5 PM to visit with Minister Alexander Prokofiev, who restored my studentship at Kharkov University. Looking back on this now, it seems like a New Year's miracle. I will never forget how my fellow students defended me at the course meeting despite the incredible pressure from local party officials to approve my expulsion. Their resistance encouraged the headman of our course, Vladimir Maleev, to sign a letter in my defense, which was an important argument in my appeal.

NOVOSIBIRSK: FIRST STEPS IN SCIENCE, 1953–1966: RUMER, LANDAU, AND KHALATNIKOV

We graduated in May 1953 after Stalin's death. Svetlana and I were appointed to Novosibirsk for an obligatory three-year assignment in designated organizations. There I met Yuri Borisovich Rumer, a close friend of Lev Landau and former assistant of Max Born. He had been arrested with Landau in 1938. The brave and clever intercession by Pyotr Kapitsa led to the release of Landau after one year, but Rumer served out his ten-year jail term, where he worked for the Soviet Air Force industry. Then Rumer was sent for another ten years of exile in the small East Siberian town of Eniseisk. Due to the efforts of his numerous influential friends, he was appointed to a small Division of Technical Physics of the Soviet Academy of Science in Novosibirsk. I became his doctoral student ("aspirant") in 1955. My Ph.D. thesis was devoted to an extension of the Dolph theory of optimal linear arrays with equidistant emitters (1) to include distances between emitters less than half of the wavelength and arbitrary direction of the radiation maximum. To this end, I invented a new kind of polynomials that extended the well-known Chebyshev and Akhiezer polynomials (2). Professor Naum Akhiezer kindly taught me the Chebyshev methods. My dissertation was defended at Tomsk State University in 1957.

Meanwhile, I started to study topics that were much more interesting for me: the modern physical problems of Feynman–Schwinger–Tomonaga–Dyson quantum electrodynamics and Onsager's exact solution of the two-dimensional (2D) Ising model.

In 1955, Rumer's rights of citizenship were returned. He immediately started to collect a small theoretical group. Besides myself, it included young alumni of Tomsk University: physicists Sergei Savvinykh, Boris Zhelnov, and Ilya Gilinskii; mathematician Victor Toponogov; my fellow-students Mark Minz, Felix Ulinich, Alexander (Sasha) Dykhne, Sasha Kazantsev, Eduard Batyev, Alexander (Alik) Chaplik, Grisha Surdutovich, and Zhenya (Eugen) Baklanov; and students of Novosibirsk University Rita Vitlina, Matvei Entin, and Lev Magarill.

In 1957, Rumer was appointed Director of the Institute of Radiophysics in a newly organized Siberian Branch of the Academy of Sciences. To our great happiness, a lot of brilliant active scientists arrived at Novosibirsk, among them physicists Gersh Budker, Roald Sagdeev, Spartak Belyaev, Victor Galitski, Boris Chirikov, and Dmitri Shirkov.

At the beginning of 1957, Savvinykh, Ulinich, and I worked on the theory of waveguides, whose cross section varies slowly along the waveguide axis. We found that if the shape of the cross section does not change and its area changes slowly, then reflection does not appear to any order of perturbation theory. In trying to determine the reflection coefficient, I considered the simpler problem of reflection of a quantum particle by a potential barrier when its energy exceeds the barrier height. This is a purely quantum phenomenon.

One needs to solve the stationary one-dimensional (1D) Schrödinger equation (SE). It can be written as $\psi''(x) + k^2(x)\psi(x) = 0$, where $k^2(x) = 2m[E - V(x)]/\hbar^2$, E is the energy, and $V(x)$ is the potential. We assumed that the potential $V(x)$ is an analytic function of x that has no singularities on a real axis and goes to zero as $x \to \pm\infty$. Therefore, $k^2(x)$ also has no singularities but also no nodes on a real axis. In this approach, the potential energy can be comparable with the total energy, but it must vary slowly in space. This means that it changes slightly over the scale of a local wavelength $\lambda = 2\pi/k(x)$; i.e., $\lambda|V'(x)/V(x)| \ll 1$ for all x. I suggested a change of variables that transformed the initial problem of potential scattering to the problem of a free particle with a small perturbation. Instead of the coordinate x, I used the eikonal $s = \int^x k(x')\mathrm{d}x'$. Instead of the initial wave function, I introduced a new one, $\phi(s) = \sqrt{k(x)}\psi(x)$. In terms of the new variables, the SE acquires the form $\ddot{\phi} + \phi + \sigma\phi = 0$, where the dots mean differentiation with respect to s and $\sigma = \frac{3k'^2 - 2k''}{k^4}$ is the so-called Schwarz derivative. It is small magnitude—of the order of $(\lambda/a)^2$, where a is the characteristic linear size over which the potential changes. I conjectured that all terms of the perturbation series in this small parameter have the same order of magnitude. My coworkers verified my conjecture in one night. Up to universal numerical factors each term in the series is proportional to the exponentially small factor $\exp[i\Im s_0]$, where $s_0 = \int_{\Re x_0}^{x_0} k(x)\mathrm{d}x$ is the eikonal corresponding to the nearest turning point x_0 in the complex plane for which $k^2(x_0) = 0$ (3).

In September of 1957, Rumer introduced me to his friend Landau at the Kapitsa Institute of Physical Problems in Moscow. After some discussion, Landau approved our results. This was a big success for us: We calculated a simple and fundamentally important quantum-mechanical effect that had remained in obscurity some 33 years after the discovery of quantum mechanics. Such well-known and experienced physicists as Arkady Migdal and Leonard Schiff published erroneous works on this topic (4–6). I was invited to give a talk at the Landau seminar. After the seminar, many of its participants, among them legendary physicists Yakov Zel'dovich, Evgeni Lifshitz, Vitaly Ginzburg, and Igor Tamm, whom I knew by name and work but had never before met, congratulated me on my interesting first work.

Most importantly, Isaak Khalatnikov, then Landau's assistant, invited me to collaborate on semiclassical physics. We decided first to work on the same problem and try to find a more systematic approach that would give results without requiring the summation of an infinite series. From this common work began a friendship that continued until Isaak's recent death at the age of 101. Because of our collaboration, I visited the Kapitsa Institute for 1–2 months each year and had the privilege and great pleasure of communicating with Landau and his closest students and assistants between 1957 and 1962, the year of the ill-fated accident that ended the creative work of this great scientist and sincere and brilliant man. I am grateful for this precious experience.

On one occasion, Landau introduced me to the great mathematician Israel Gelfand, who invited me to his seminar at Moscow University. After a brief introduction, Gelfand stopped me and asked the participants to conjecture what should be the result or how to solve the problem of my talk. Nobody could say anything substantial except for Marat Evgrafov, who said that the proper approach must start with the solution of the SE near the complex turning point. This idea sounded very attractive to me. However, it took us more than two years to understand how to realize this program. At that time, Khalatnikov found some references to the method of the Dutch theorist A. Zwaan, who in 1929 found the spectrum of an adiabatically varying Hamiltonian by analytic continuation of the wave function in the complex time-plane. Inspired by the Zwaan method and Evgrafov's idea, we proposed to continue the solution of the initial SE for wave function $\psi(x)$ from $x = \infty + i\gamma$, where it is te^{ikx}, to the complex turning point x_0 in the upper half-plane along the so-called anti-Stokes line defined by the equation $\Im \int_{x_0}^{x} k(x')\mathrm{d}x' = 0$. Along this line, the two solutions $\phi_\pm = e^{\pm is}$ are easily distinguishable. It means that the chosen solution arrives in the vicinity of the turning point x_0 as the exponent ϕ_+, acquiring only a phase factor. Continuing the same solution along the arc of a circle around the turning point x_0 to below it, we reach the second anti-Stokes line going to $-\infty$. The radius of the circle must be large enough to ensure the applicability of Wentzel–Kramers–Brillouin approximation. The circle crosses one of the three Stokes lines that form angles at 60 deg with the anti-Stokes lines. After crossing the Stokes line, the asymptotic form of the solution changes from e^{is} to $e^{is} - ie^{-is}$. The second term is the reflected wave. It arrives at $-\infty + i\gamma'$ with an additional phase factor. The magnitude of the reflection amplitude can be found as $|r| = \Re \exp[i \int_{\Re x_0}^{x_0} k(x)\mathrm{d}x] = \exp[\frac{i}{2} \int_{x_0^*}^{x_0} k(x)\mathrm{d}x]$. In this way, we indeed obtained the results earlier obtained by summation of a series and found new results for the case of two close turning points or close pole and turning points that unavoidably appear if $V_{\mathrm{max}} \ll E$ (7). One of my most talented students, Alexander (Sasha) Dykhne, later Academician of the Soviet Academy of Sciences, extended this method to a particle in a periodic potential (8).

This work is now considered the starting point of a new branch of mathematical physics called asymptotics beyond all orders (9, 10). Phenomena described by this science include dendritic growth of crystals; viscous fingering in a layer of viscous liquid; equatorial Kelvin wave instability, important in meteorology and oceanography; propagation of high-energy elementary particles in external fields, particle channeling in crystals, etc. The main creators of this discipline are M. Berry, M. Kruskal, J. Boyd, H. Segur, J. Langer, B. Shraiman, M. Mineev, and P. Wiegmann.

After this work, the center of my interests shifted to the general theory of second-order phase transitions and critical phenomena. The motivation was a major discrepancy between Landau mean-field theory of the second-order phase transitions and the Onsager's exact solution of the 2D Ising model. Landau theory predicted that independently on dimensionality the order parameter in the ordered phase grows as $\sqrt{|T - T_\mathrm{c}|}$, that the correlation length on approaching the transition temperature T_c grows as $|T - T_\mathrm{c}|^{-1/2}$, and the specific heat has a finite discontinuity at the transition point, whereas in the Onsager's solution the same quantities behave as $|T - T_\mathrm{c}|^{1/8}$, $|T - T_\mathrm{c}|^{-1}$, and $-\ln |T - T_\mathrm{c}|$, respectively. Levanyuk (11) and Ginzburg (12) noted that the mean-field approximation is valid if fluctuations of the order parameter $\sqrt{\langle(\delta\eta)^2\rangle}$ are small compared to the average order parameter $\langle\eta\rangle$. However, near T_c fluctuations grow even in the framework of Landau theory. Therefore, this theory is valid only sufficiently far from the transition point, i.e., at $|\tau| \equiv \left|\frac{T-T_c}{T_c}\right| \gg \mathrm{Gi}$. The dimensionless parameter Gi characterizes a given physical system. Because Landau theory uses an expansion in powers of the parameter τ, this parameter must be small or $|\tau| \ll 1$. This implies that $\mathrm{Gi} \ll \tau \ll 1$. Not every system has this property. Specifically, the Ising model is characterized by only a single parameter: the interaction energy of the nearest spins. It is impossible to construct dimensionless parameter from only one dimensional parameter. Therefore for the Ising model, the Landau theory is invalid.

Even if the system has a region of validity for Landau theory where $1 \gg |\tau| \gg \mathrm{Gi}$, there is a range of temperatures $|\tau| \ll \mathrm{Gi}$ for which fluctuations are significant and Landau theory fails. What are the properties of an ordering system that fluctuates significantly? This problem became central for statistical physics and condensed matter physics in the 1960s and 1970s.

Alexander (Sasha) Patashinskii and I started to work on this problem in 1963. As a starting point, we used a construction proposed by Landau in the end of the 1950s: the partition function of strongly fluctuating order parameter should be obtained as a Feynman path integral in which the Landau mean-field free energy plays the role of the action. We transformed this statement into a field theory in three-dimensional (3D) Euclidean space (13). In the regime of large fluctuations, the order parameter field describing the fluctuations obeys a universal equation for correlation functions of second and fourth orders. We verified that at T_c this equation has the property of scaling invariance. This means that averages of the type $\langle\eta(x_1)\eta(x_2)\cdots\eta(x_{2n})\rangle$ under coordinate scaling $x_i \to \lambda x_i$ are multiplied by the factor $\lambda^{-2n\Delta_\eta}$, where Δ_η has the physical meaning of a scaling dimensionality (critical exponent) of the order parameter. The field $h(x)$, conjugate to $\eta(x)$ (the magnetic field if the order parameter is the magnetization) in D-dimensional space, contributes to free energy the term $F_h = -\int h(x)\eta(x)\mathrm{d}^D x$, which is invariant under the scaling transformation. Therefore, the scaling dimensionalities of the fields h and η obey the equation $\Delta_h + \Delta_\eta = D$. The analogous relation is correct for any pair of mutually conjugated fields, specifically for dimensionless temperature deviation τ and entropy fluctuation s. All these relations were also obtained from our equation for correlation function of the fourth order. However, the Dyson equation for the correlator of the second order contained the initial 4-vertex, i.e., a constant. The scaling invariance for such an equation is maintained only if $\Delta_h = D/4$ and $\Delta_\eta = 3D/4$. Such scaling dimensions contradict the Onsager exact solution for $D = 2$. For 3D field theory they lead to the Green's function in momentum representation $G(\mathbf{p}) = \langle\eta^*(\mathbf{p})\eta(\mathbf{p})\rangle \propto p^{-3/2}$. This result was erroneous. Four years later, Polyakov explained why the Dyson equation is invalid in the range of large fluctuations (14). He proved that the unitarity condition for the order field requires two complete 4-vertices in an equation for the Green's function. Therefore, the calculation of powers at each vertex in the Dyson equation gives no new relation for critical exponents.

In the meantime, we developed a physical picture of ordering in the regime of strong fluctuations (15). For simplicity, only the violation of Z_2 symmetry (Ising order parameter) was considered. We divided the total volume into a set of cubic cells (droplets) with the length of each cube side equal to the correlation length $r_c(\tau)$. Because the fluctuations strongly correlated within such a cell, we ascribed to the cell a single degree of freedom for the fluctuation. We introduced the cell "magnetic moment" $\mathfrak{M}$. Above the transition temperature, the moments of different cells are independent. The average square of fluctuations amplitude is associated with the square of that moment and with the correlation radius by the relation $\langle\eta^2\rangle = \frac{\mathfrak{M}^2}{r_c^{2D}}$. The average value of the order parameter below the transition temperature is $\langle\eta\rangle = \pm\frac{|\mathfrak{M}|}{r_c^D}$. Finally, the free energy per degree of freedom must be approximately equal to T_c. Thus, the free energy of unit volume associated with fluctuations is $\frac{F_f}{V} = \frac{T_c}{r_c^D}$. The specific heat of fluctuations per unit volume is $\frac{C_{V_f}}{V} \sim \frac{1}{r_c^D \tau^2}$. Other measurable values can be expressed in terms of the two basic values $\mathfrak{M}$ and r_c. This fact permits us to obtain a set of relations between the critical exponents α, β, γ, δ, ε, ζ, and ν defined by the power dependencies of different measurable values on τ and h. Namely, $C_V \propto \tau^{-\alpha}$, $\langle\eta\rangle \propto (-\tau)^{\beta}$ ($\tau < 0$), magnetic susceptibility in a weak field ($|h|\mathfrak{M} \ll T_c$) $\chi \propto \tau^{-\gamma}$, the order parameter in a strong magnetic field $\langle\eta\rangle \propto h^{1/\delta}$, the specific heat in strong magnetic field $C_V \propto |h|^{-\varepsilon}$, the correlation radius without magnetic field $r_c \propto \tau^{-\nu}$, and in a strong magnetic field $r_c \propto |h|^{-\zeta}$. The crossover from a weak to a strong field is determined by universal dimensionless scaling functions of the argument $\frac{h\mathfrak{M}}{T_c} \propto \frac{h}{\tau^{D\nu-\beta}}$, for example $\langle\eta\rangle = \tau^{\beta} f\left(\frac{h}{\tau^{D\nu-\beta}}\right)$.

We derived the droplet model more rigorously employing the expansion of free energy in the powers of $h(x)$. The coefficients of these expansions are multiargument correlators $\langle\delta\eta(x_1)\delta\eta(x_2)\cdots\delta\eta(x_n)\rangle$. They are homogeneous functions of their arguments of the power $n\Delta_\eta$. For finite deviation of the temperature from critical point, they are equal to the same correlators at $T = T_c$ multiplied by a universal scaling function of the ratios x_{ik}/r_c, where x_{ik} is the distances between the points x_i and x_k. Thus, our work established the scaling for higher-order correlators. When our work was published, the measurements of higher-order correlators were beyond experimental capabilities. But now optical methods permit the measurements of correlators of the third order and even higher. We also expressed the macroscopic observable values in terms of multipoint correlator series.

At that time, several works on universal scaling laws appeared almost simultaneously. Our work was first presented as a talk at a conference on critical phenomena in the summer of 1965 at the Joint Institute for Nuclear Research. The article was published in the Soviet journal *Zhurnal Eksperimental'noi i Teoreticheskoi Fiziki* in February 1966, and the English version was published in August of the same year. The first publication containing the scaling equation for an order parameter belongs to Ben Widom (December of 1965) (16). Leo Kadanoff's article was published in the journal *Physics* in June 1966 (17). In addition to scaling considerations similar to ours at $T = T_c$, Kadanoff's work contained the very important idea on renormalization by integrating out the short-range degrees of freedom. However, this idea was explicitly realized only in the later works of Ken Wilson. In the Nobel Committee Press Release for the 1982 Nobel Prize in Physics awarded to K.G. Wilson, our names were mentioned among seven scientists who made "important theoretical contributions" to the theory of phase transitions. Both our principal works (13 and 15) were cited in the Nobel Presentation by Kenneth Wilson. Leo Kadanoff in his textbook *Statistical Physics: Statics, Dynamics and Renormalization* cites our article (13) as the first formulation of the Scaling Law.

A very important role in the theory of phase transitions belongs to Michael Fisher. He proposed to consider the dimensionality of space as a continuous parameter. This idea had led Wilson and Fisher to consideration of phase transition in the space of dimensionality $4 - \epsilon$. I first met Michael in 1967 in Budapest during the conference on critical phenomena at the Physical Institute of the Hungarian Academy of Sciences. At a reception at the house of the director of the institute Laszlo Pal, who was also a big Communist Party boss, both Michael and I were so absorbed in scientific conversation that we broke the glass of a dessert table. This little incident marked the beginning of our friendship and great mutual respect.

In the year 1964, the Institute of Radiophysics was transformed into a division of the Institute of Semiconductor Physics. Yuri Borisovich Rumer was invited to the Institute of Nuclear physics by its director, Prof. G.I. Budker. Sasha Patashinskii also entered this institute. In 1966, I was invited to a newly organized Institute of Theoretical Physics in Chernogolovka. Alik Chaplik became the leader of our theoretical group at the Institute of Semiconductor Physics. He is now a full Academician of the Russian Academy of Sciences and still is the head of this laboratory.

This was the end of my long, 13-year Novosibirsk life. I remember it as my happiest time. I worked in a friendly and lively environment of friends and students who were almost the same age. Yuri Borisovich Rumer did his best to free us from the worries of everyday life. Despite the long tragedy in the middle of his most active years of life, he remained an extremely lively and passionate person. Communications with him were one of the most attractive features of our lives. He attentively followed the news of modern physics and molecular biology and discussed them with us. He eagerly introduced us to his old friends and acquaintances. We freely collaborated with people from other institutes and participated in conferences. People from Moscow and Leningrad visited our group; I remember Sasha Vedenov and Sima Eliashberg as our guests. It was also a time when our dear children Sergei and Olga were born and growing up, giving Svetlana and me much joy.

CHERNOGOLOVKA: LANDAU INSTITUTE, 1966–1992

The Landau Institute for Theoretical Physics was the creation of Khalatnikov, the only person who could overcome the barriers of Soviet bureaucracy. There he collected brilliant theorists and mathematicians and enabled them to fruitfully interact. I enjoyed communications with Anatoly Larkin, Gerasim Eliashberg, Lev Gor'kov, Alexei Abrikosov, Igor Dzyaloshinskii, Mark Azbel, Sergei Iordansky, Sasha Kazantsev, Emmanuil Rashba, Yeshua (Yuzik) Levinson, Vadim Berezinskii, Yuri Bychkov, Vladimir Gribov, Sasha Polyakov, Sasha Zamolodchikov, Sasha Migdal, Sasha Belavin, Volodya Fateev, Zhenya Bogomolny, Sergei Novikov, Yasha Sinai, Alexei Starobinsky, Vladimir Zakharov and later Arkady Migdal (senior). Among the first students were Yuri Ovchinnikov, Boris Lukyanchuk, Efim Kats, Slava Kamenski, David (Dima) Khmelnitskii, Gennadi Uimin, Vladimir Mineev, Grisha Volovik, Kostya Efetov, Sergei Brazovski and his wife Nina Kirova, Sasha Finkelstein, and Pavel Wiegmann. In Chernogolovka, my wife and I acquired our dear friends, mathematicians Vladimir Gurarii and Vladimir Matsaev. One of my teachers, the outstanding mathematician Aleksander Povzner also worked in Chernogolovka and became a close friend. I published many works with Sasha Kazantsev and my Ph.D. students Gennady Uimin, Volodya Mineev, Sasha Kashuba, Igor Lyuksyutov, Misha Feigel'man, Andrei Talapov, and Leonid Pryadko. I am proud that among the people who chose me as their scientific advisor was Alexei Kitaev, now professor of theoretical physics and computer science at Caltech, creator of the idea of topologically protected quantum information and winner of the 2008 MacArthur Fellows Program, the 2012 Breakthrough Prize founded by Yuri and Julia Milner for studies in fundamental physics, and the 2017 Buckley Prize. Khalatnikov organized Soviet–American, Soviet–French, and Soviet–German symposia that allowed us to make personal acquaintances with physicists from these countries. Khalat, as everyone called him, was not only an outstanding scientist but also a genius of organization. But the main feature that made him an ideal director of the institute was his unselfish admiration of young, talented people and his desire to help them.

Perhaps, the most precious gift I received from Khalat was my friendship with Walter Kohn that began during his visit (with his wife Mara) to Moscow in the year 1987 and continued till his death. Our families were friends, including our children. Several times we visited Walter and Mara in Santa Barbara and enjoyed his cooking and his wisdom. He visited us in College Station, the last time being at my seventy-fifth birthday jubilee.

For the Soviet–German Symposium, I was the responsible organizer from the Landau Institute; from the German side, the organizer was Walter Selke. We became friends and still maintain contact.

My next substantial contribution to the theory of phase transitions was made during the period from 1978 to 1982. It was the theory of the so-called Commensurate–Incommensurate (CI) phase transition at finite temperature, also known in the literature as Pokrovsky–Talapov phase transition (18). Its initial motivation came from experimental results obtained by Anton Naumovets and his group in Kiev, Ukraine. Naumovets & Fedorus (19) studied the structure of adsorbed submonolayers of Sr on the face (110) of W by slow electron diffraction. According to the Debye–Waller theory, the maximum of diffraction-spot intensity (Debye–Waller factor) has a plateau at low temperature, and exponentially decreases at temperatures exceeding the Debye temperature T_D. For atomic crystals, T_D is a few hundred kelvin. The Naumovets–Fedorus experiment confirmed the predicted behavior if the concentration of Sr is less than 50–60% of one filled monolayer. For higher concentrations, the maximum intensity has no plateau and no sharp decrease at $T \approx T_D$. The experimenters treated their result as evidence that at low concentration the absorbed Sr atoms are located in dips between atoms of the substrate (W) and, due to the van der Waals interaction, they form a regular lattice commensurate with the substrate lattice. However, at large density the Sr atoms overlap and destroy their commensurate crystal structure. They form a kind of flowing substance.

What is the mechanism for fluidity of the absorbed atoms? This problem was very topical at that time for physics of surfaces and interfaces (20). The class of submonolayers of atoms adsorbed on crystal surfaces included H_2, D_2 (21) and He, Ar, and Kr on graphite substrate (22). In the book (23), the reader can find many other examples of submonolayers adsorbed on different substrates.

Theoretical description of these systems was based on a 1D model proposed by Frank & van der Merwe (24–27). In this model, adsorbed atoms are described as a chain of particles connected by identical harmonic strings of unstressed length a with stiffness κ. The substrate is interpreted as a periodic potential $V(x)$ for the particles with period $b \neq a$. The competition of these two periodicities leads to the CI transition. The simplifying assumptions of a small difference of periods $|a - b|/a = |\delta| \ll 1$ and weak potential $V_{\max} - V_{\min} \ll \kappa a^2$ permit us to replace a chain of atoms by a continuous string in a periodic potential. The string either lies in one valley of the potential or transitions from one valley to another by a kink. If $\delta = 0$, the energy of a single kink is $\varepsilon_{\text{kink}}(0) \sim \sqrt{\kappa b^2 (V_{\max} - V_{\min})}$. This energy is positive. The kinks are energy unfavorable, and they do not exist in the ground state. This is the commensurate phase. For nonzero δ, the energy of the soliton is equal to $\varepsilon_{\text{kink}}(\delta) = \varepsilon_{\text{kink}}(0) - \delta\kappa b^2$. At a critical value $\delta = \delta_c = \varepsilon_{\text{kink}}(0)/(\kappa b^2)$, the kink energy becomes zero and then negative. The kinks then proliferate until their repulsion stops the proliferation.

The interaction between two solitons separated by a distance l is exponentially small, $U_{\text{int}}(l) \sim \varepsilon_{\text{kink}}(0) e^{-l/l_{\text{kink}}}$. The size of a kink is $l_{\text{kink}} \sim b/\sqrt{\delta} \gg b$. These relations imply that the period of soliton structure that appears for $\delta > \delta_c$ is $l \sim l_{\text{kink}} \ln \frac{\delta_c}{\delta - \delta_c}$ as long as $l \gg l_{\text{kink}}$. This periodic soliton structure is in an incommensurate phase. Its period depends continuously on the driving parameter δ. The predicted logarithmic dependence is very difficult to observe. To make the logarithm sufficiently large, it is necessary either to approach very nearly to the critical value or to employ a small concentration of adsorbed atoms, but then the signal from it is very weak. All measurements must be performed at low temperature because 1D order is destroyed by finite temperature (28).

The situation is different in a 2D crystal. There exists a simple situation in which the primitive cell of the absorbed atoms lattice is rectangular, and the effective periodic field acts only in one of the two directions. This is the case for alkali and alkali earth atoms absorbed on W. The commensurate lattices of the noble gases are usually triangular, and their elasticity is isotropic. The (111) face of Pt and Au induced an anisotropic periodic potential for adsorbed atoms. This geometry at zero and finite temperatures can be treated analytically (18).

One can imagine the continuous model of 2D commensurate crystals as a set of elastic strings with the rigidity κ located at the bottom lines of the 1D periodic potential. The soliton in this case is also a linear object: It is a local distortion of the periodic string arrangement so that one extra string or one missing string occurs in the soliton. At zero temperature, the soliton's structure appears as in the 1D case due to the difference of periods. There is no substantial difference between the analysis of the transition to incommensurate phase and its periodicity in one and two dimensions at $T = 0$.

An anisotropic lattice of adsorbed atoms and substrate at nonzero temperature was considered by Luther et al. (29). The basic part of this work was developed by Alan Luther and me during my two-month visit to NORDITA (Nordic Institute for Theoretical Physics) and Bohr's Institute in 1978, another miracle produced by Khalat in the KGB-controlled Academy of Sciences. A more sophisticated case with isotropic elasticity of adsorbed atoms and anisotropic substrate was studied by Pokrovsky & Talapov (18). In both cases, we considered the coordinate along the wells of potential to be the time. In this representation, the soliton line can be considered to be the world line of a quantum particle. Finite temperature induces the meandering of soliton lines. In the quantum picture, this meandering can be treated as random trajectories in a Feynman path integral. Thus, in this representation temperature plays the role of the Planck constant. Although the two soliton lines can approach each other on a distance of the order of the soliton width, they never intersect because the strong overlapping requires too much energy. Therefore, the meandering particles can be identified as fermions. This identification can be proved by Stanley Mandelstam's construction of two-component fermion operators from the Bose displacement field and the corresponding momentum.

In the ground state, fermions in one dimension doubly occupy all states within the momentum interval $(-p_{\mathrm{F}}, p_{\mathrm{F}})$. The number of such momentum states per unit length is $\frac{2p_{\mathrm{F}}}{2\pi\hbar}$. The density of fermions is double that, $n = \frac{2p_{\mathrm{F}}}{\pi\hbar}$. The total energy of the Fermi-line per unit length is $\frac{E_{\mathrm{f}}}{L} = \frac{p_{\mathrm{F}}^3}{3\pi m\hbar} = \frac{\pi^2\hbar^2 n^3}{12m}$. Including the energy arising from the difference between the fractional mismatch δ and its critical value δ_{c}, the total energy per unit length is $\frac{E}{L} = \frac{E_{\mathrm{f}}}{L} - (\delta - \delta_{\mathrm{c}})\varepsilon_{\mathrm{sol}} a n$, where $\varepsilon_{\mathrm{sol}}$ is the energy per unit length of the soliton line. Minimizing this energy with respect to n, we find $n = \frac{2}{\pi\hbar}\sqrt{(\delta - \delta_{\mathrm{c}}) m \varepsilon_{\mathrm{sol}} a}$. In terms of the string model and temperature, the ratio $\frac{\hbar^2}{m}$ must be identified with $\frac{T^2}{\kappa}$, which results in $n = \frac{2}{\pi T}\sqrt{\kappa a \varepsilon_{\mathrm{sol}}(\delta - \delta_{\mathrm{c}})}$. The physical reason for repulsion between soliton lines is the decrease of their bending entropy when they approach each other. Such a mechanism for entropy repulsion was first discussed by Gruber & Mullins (30) and Voronkov (31) in connection with the steps on the surface of crystals.

The CI transition is probably the simplest example of topological phase transition. Kinks and solitons are topological defects similar to vortices in the Berezinskii–Kosterlitz–Thouless phase transition. In contrast to this famous example, the CI transition changes the topological charge of the equilibrium state from zero to a value proportional to the linear size of the system.

Diverse applications of the theory, beyond the already discussed adsorption on perfect crystal faces, includes the roughening transition and the equilibrium shape of crystals between smooth faces with small Miller indices, as first theoretically predicted by Rottman & Wortis (32) and by Jayaprakash & Saam (33). The experiment was performed using a lead surface by Rottman et al. (34). It confirmed good agreement with our theory. A. Erbil et al. (35) performed experiments with Br_2 molecules intercalated into graphite. They found the CI transition at 342 K. The incommensurate linear lattice had density proportional to $(T - T_{\mathrm{c}})^{\eta}$ with $\eta = 0.5 \pm 0.02$. P. Martinoli et al. (36) measured critical current in a superconducting film with periodically corrugated thickness. The magnetic field perpendicular to the film created a triangular lattice of vortices whose length, and therefore whose energy, was periodically modulated. They discovered the CI transition and found the density of soliton lines to be in reasonable agreement with our theory.

Among other applications, it is worthwhile to mention the prediction made by Hanna et al. (37) that in a double-layer quantum Hall system a sufficiently strong magnetic field drives a CI transition in a crystal formed by soliton lines in two dimensions. Experimentally, the CI transition manifests itself with a singularity of the magnetic susceptibility at the transition point. Garst et al. (38) predicted a strong enhancement of Coulomb drag in a thin wire at quantum CI transition. Büchler et al. (39) proposed to search for the CI transition in a 1D chain of cooled atoms confined by an optic lattice detecting the commensurate phase by the static structure factor and by the appearance of a gap in the spectrum. They cited an important work by Japaridze & Nersesyan (40); these authors used the technique of fermion bosonization by Luther, Emery, and Peshel to study the magnetic state of 1D electrons with attraction.

Our work also gives a simple physical treatment of the Jordan–Wigner fermions that appear in the 2D Ising model: They are domain walls, which are lines in 2D space. If one of the coordinates is treated as time, these lines play the role of the fermion world lines. The difference with our problem is that, in the Ising model, the number of fermions is not conserved. A pair of them can annihilate in one point or be created at another point. The transition is the percolation of the domain walls.

I remember with gratitude Jack Villain's quick response to our article. He published his interpretation of our results in a short review article that attracted the attention of experimenters in the field. Our cooperation and friendship with Per Bak began with the discussion of this work.

In 1980, I was invited to the Workshop on Surface Phenomena at the University of Washington at Seattle. To my surprise, my attendance was allowed. The reason was the planning system of the Soviet Union. Due to the Afghan war, all Soviet official visits to the United States were canceled. Meanwhile, the Foreign Department of the Soviet Academy of Sciences had to fulfill the travel plan. Thus, I spent one month in Seattle and participated in the workshop, which was extremely interesting. I presented my work with Talapov, which was then recently published. I had rather fruitful discussions with theorists David Thouless, Michael Kosterlitz, Michael Schick, Eberhard Riedel, and Marcel den Nijs and with experimenters Greg Dash, Sam Fain, and Michael Chinn. I again worked with Per Bak. Seattle is an amazingly picturesque city. We traveled to meetings by boat across the lake. I still remember an unforgettable excursion to Mount Rainier. In its lower part it is forested and inhabited by a variety of birds and animals. I acquired new friends here. Michael Schick and Eberhard Riedel later were our guests in Chernogolovka and Moscow. David Thouless visited us in the United States.

The years of perestroika in the Soviet Union were critical for the Landau Institute. The most famous of its employees received tempting invitations from Western universities and laboratories. This coincided with great funding difficulties. With incredible effort, Khalatnikov managed to preserve the institute, but almost all the leading employees of the first generation and some young people left it; some of them maintained constant contact with the institute. In 1990, I and my wife had spent half a year in a small German city, Jülich, near Cologne. I worked in the Condensed Matter Division of the Institute of Nuclear Physics headed by Heiner Müller-Krumbhaar, a brilliant scientist and an extremely friendly and attentive person. His sharp wit and hospitality are the most pleasant memories of our life in Germany. Johannes Zittartz invited us to his home in Cologne together with our daughter and granddaughter. In Jülich, I first met personally Dr. Jacques Villain and my future coworker and friend Danilo Pescia, then a postdoctoral fellow. I visited Brookhaven in 1991 and worked with Per Bak. During this visit, I was invited to visit the University of California, Los Angeles, by Steve Kivelson, who was earlier my guest at the Landau Institute. We researched the way to detect the fractional charge in the quantum Hall effect.

AMERICAN YEARS, 1992–PRESENT

In 1992, I got an offer to the position of full professor at the Department of Physics (later Physics and Astronomy) of Texas A&M University (TAMU), where I remain an active faculty member.

The essential initial contact was made by Marko Jarić, an outstanding scientist and personality, whom I met through discussions of quasicrystals at the Trieste International Centre for Theoretical Physics. He unfortunately has died prematurely in his prime.

Though I accepted the offer from TAMU, I never lost my connection with the Landau Institute, remaining its Senior Scientist and a member of its Scientific Council. I visited the institute regularly at least once a year. All my publications of this period indicate two affiliations.

I have found at TAMU a friendly and scientifically exciting environment. I have worked a great deal with my colleague Wayne Saslow and my former student at the Landau Institute, Igor Lyuksyutov, who became later a professor at TAMU, and with experimentalists Don Naugle and Glenn Agnolet. I worked regularly with my Ph.D. students Artem Abanov (now professor at TAMU), Valery Kalatsky, Serkan Erdin, Amin Kayali, and Hongduo Wei. Later, I enjoyed discussions with Sasha Finkelstein, which were always pleasant and beneficial for me. I interacted with people from the Institute of Quantum Optics (Marlan Scully, Olga and Vitaly Kocharovsky, Alexei Belyanin and Tanya Erukhimova, and David Lee) and from divisions of elementary particles and string theory (Dick Arnovitt, Chris Pope, Peter MacIntire, and Bob Webb) and nuclear physics (Ralph Rapp, Saskia Mioduszewski, and Shalom Shlomo). I had also contacts and in some cases collaboration with my colleagues from other departments: mathematicians Peter Kuchment, Raicho Lazarov, Gregory Berkolaiko, and Alexei Poltoratski and chemists Fred Cotton, Kim Dunbar (with whom I had a common project on molecular magnets), and John Bevan.

The most important work I have conducted while at TAMU are theories of Landau–Zener (LZ) transition in noisy medium, ferromagnetic-superconducting hybrids, and Bose–Einstein condensation of magnons in ferromagnetic films. The nonadiabatic LZ transition at the crossing of two levels (Wigner-von Neuman) is one of very few general and fundamental results from time-dependent quantum mechanics. Traditionally it was applied in quantum chemistry (41) and in collision theory (42). In the 1990s the quantum hysteresis in molecular magnets found in experiments by Wernsdorfer & Sessoli (43) was a real triumph of the LZ theory. It plays a central role in the relaxation processes of many-body systems. It changes the states in a qubit and in this way transfers the information in quantum computers. Thus, the LZ theory remains a very active field now.

In 1932, Landau considered a two-level quantum system with Hamiltonian $H = at\sigma_z + b\sigma_x$, where a and b are constants, t is the time counted from the avoided crossing point, and σ_x, σ_y, and σ_z are Pauli matrices (44). He argued that most probably only two levels cross at the same time and that for a small interval of time any function of time can be replaced approximately by a linear dependence. Landau found that the diagonal matrix element of the scattering matrix for this problem is equal to $e^{-\pi\gamma^2}$, where $\gamma = \sqrt{b^2/\hbar a}$. Clarence Zener, somewhat later the same year, found the nondiagonal matrix elements (45). Independent of these two authors and from each other, the famous theorists Ettore Majorana (46) and Ernst Stückelberg (47) obtained the same results that same year and considered different aspects of this problem.

In real life it is impossible to avoid the noise. How does noise distort the LZ matrix? The characteristic time over which the transition proceeds is $\tau_{\mathrm{LZ}} = b/a$ if $\gamma^2 \lesssim 1$ and $\tau_{\mathrm{LZ}} = \frac{\hbar}{b}$ if $\gamma^2 \gtrsim 1$. The noise is called fast if its correlation time is much shorter than τ_{LZ} and slow in the opposite case. The effect of noise is described by the noise Hamiltonian $H_n = \sum_{i=x,y,z} \eta_i(t)\sigma_i$, where $\eta_i(t)$ are stochastic variables. We considered the Gaussian noise characterized by its pair correlation functions $\langle \eta_i(t_1)\eta_j(t_2)\rangle = g_{ij}\left(\frac{|t_1-t_2|}{\tau_n}\right)$, where τ_n is the noise correlation time. The noise is fast if $\tau_n \ll \tau_{\mathrm{LZ}}$. The functions $g_{ij}(\theta)$ were assumed to be of the same order of magnitude for $\theta \lesssim 1$ and to quickly decrease for $\theta > 1$. The first study of the fast diagonal noise, taking only $\eta_z \neq 0$, belongs to Y. Kayanuma (48). After that, Kayanuma considered a system with $a = 0$ and purely nondiagonal noise with only the x-component being completely responsible for transitions (49). Additionally, he assumed the correlation function to be $g_{xx}(\theta) = J^2 e^{-\theta}$.

In 2005–2006, I had a second bit of good fortune in my teaching: TAMU graduate student Nikolai Sinitsyn asked me to be his scientific adviser. Nikolai had studied physics at Minsk University (Belarus), and had been accepted as a student at TAMU, where he initially worked in string theory. However, he quickly figured out that this was not his field. I quickly recognized his outstanding talent, inventiveness, unusual erudition (for his young age), and his ability to rapidly convert ideas into calculations. It was a great pleasure to me to follow his rapid growth and to work with him. Nikolai and I extended the work by Kayanuma, lifting all its unnecessary limitations (50). We assumed that the total Hamiltonian contains the LZ part with arbitrary parameters a and b, that the noise has all three components, and its correlation functions $g_{ij}(\theta)$ are arbitrary. The major assumption that we retained was that the noise is fast and weak $|g_{ij}| \ll b^2$. With all these extensions, we were still able not only to find the formal solution of the problem but to present a simple physical picture for it. Below I briefly describe our approach.

The reason why the problem was solvable is the large difference in timescale for the regular LZ process and for transitions due to noise. As previously noted, the time interval for the regular LZ process is b/a. Because the noise is fast, the energy that it gives or takes from the two-level system at the moment t must be equal to $a|t|$. However, its frequency spectrum is limited by the value $1/\tau_n$. Therefore, noise-induced transitions stop after the time interval $t_n = \frac{\hbar}{a\tau_n} \gg \frac{\hbar}{b}$. For any experimentally measurable value of γ^2, the time interval $\frac{\hbar}{b}$ is not less than τ_{LZ}. Because the regular LZ process timescale is much smaller than the fast noise timescale, one can employ the complete solution of the regular LZ problem, i.e., the density matrix $\rho(t)$ as $t \to +\infty$ versus $\rho(-\infty)$ as the initial condition for the noise-induced transition. In a simplified situation when $\eta_y = \eta_z = 0$, the probability for no transition is equal to $P_{1\to 1} = \frac{1}{2}[1 + e^{-2\pi g_{xx}(0)/\hbar a}(2e^{-2\pi\gamma^2} - 1)]$. This result becomes the LZ formula if $g_{xx} = 0$. However, if $g_{xx}(0) \gg \hbar a$, implying $\gamma^2 \gg 1$, then the noise results in an equipartition of the two levels and complete loss of memory. At each moment of the relatively long interval of noise-induced transitions, the Born approximation for these transitions is valid. Among all of the frequency components of noise, only the component with $\omega = at/\hbar$ is in resonance, and this gives the dominant contribution to the transition probability. Thus, by collecting the statistics on transitions due to noise, we can probe the noise spectrum.

The equipartition that takes place for relatively strong transverse noise is specific for classical noise. It disappears if the noise is quantum, i.e., produced by phonons in a thermal equilibrium state and returns to the equipartition only at high temperature (51).

Later, Nikolai developed the theory of multilevel LZ transitions, finding the complete set of exactly solvable multilevel LZ models. A substantial contribution to this work was made by another very talented student, Chen Sun, who is now a full Professor at Hunan University (China).

In collaboration with Igor Lyuksyutov; my students Serkan Erdin, Amin Kayali, and Hongduo Wei; and the experimental team of Don Naugle, we studied the new concept of ferromagnet-superconductor hybrids (52). It was well known that the superconductivity with s-pairing and ferromagnetism are incompatible in a homogeneous system. However, in inhomogeneous systems the strong ferromagnetic-superconducting interaction due to the magnetic field generated by both components can be employed to create materials with new and easily controlled properties for science and technology. There are two different approaches to such hybrids. In the first, on which we concentrated, the idea is to avoid the proximity effects that suppress one of the components. This goal can be attained by separating the ferromagnetic (F) and superconducting (S) components by a thin insulating film. The alternative approach is to use the proximity effect to change the Cooper pairs in the F-layer and in this way to achieve a large change in the transport coefficients and magnetic response. The first approach was developed in several experimental and theoretical groups including ours. At Argonne National Laboratory, theory was developed by Valery Vinokur with whom we actively collaborated, with experiments by the group of George Crabtree. The Belgian theoretical group (Milorad Milošević, François Peeters, and Sergey Yampolskii) worked closely with experimenters from the University of Leuven headed by Victor Moshchalkov. Experiments were also performed by the groups of Ivan Schuller (University of California, San Diego), Art Hebard (University of Florida), John Ketterson (Northwestern University), and Piero Martinoli (University of Neuchatel, Switzerland) with theoretical support by Hans Beck. I cooperated with this group in the 1980s. The alternative direction that employed the proximity effect was developed theoretically by Konstantin Efetov, Anatoly Volkov, Sebastian Bergeret (Germany), Lev Bulaevsky (Los Alamos), Alexander Buzdin (France), and Zoran Radović (Serbia). Experiments were provided by Valery Ryazanov (Chernogolovka, Russia), Alexander Golubov (Trent, Netherlands), Jan Aarts (Leiden, Netherlands) and others.

Our approach was based on an idea from Igor Lyuksyutov that the appearance of a vortex in an S-layer can decrease the energy of an FS-bilayer even if the F-layer does not create external magnetic field. Indeed, a perfect F-layer with magnetization perpendicular to the boundaries can be considered to be a magnetic analog of an electric capacitor. Therefore, it produces no external magnetic field. A vortex in the S-layer creates a dipolar-like field with quantized flux $\Phi_0 = \frac{hc}{2e}$. If the F-layer is near enough, this field interacts with its magnetization and decreases its energy by the value $-M\Phi_0 d_F$, where d_F is the thickness of the F-layer. The total energy of excitation is equal to $E_{exc} = \varepsilon_v - M\Phi_0 d_F$, where the energy of a Pearl vortex $\varepsilon_v = \frac{\Phi_0^2 d_s}{16\pi^2\lambda^2} \ln \frac{L}{\xi}$, L is the width of domain in the F-layer or the linear size of the magnetic dot, ξ is the coherence length, λ is the London penetration depth for the magnetic field and d_s is the thickness of the S-layer. These formulae show that the excitation energy becomes negative if $\frac{16\pi^2 M\lambda^2}{\Phi_0 \ln \frac{L}{\xi}} > \frac{d_s}{d_F}$. The penetration depth goes to infinity at the superconducting phase transition. Therefore, the excitation energy is negative near this transition point. Thus, there is an instability with respect to proliferation of vortices over a finite temperature interval. Vortex repulsion limits this process. The result is a periodic lattice of vortices in the S-layer. This lattice can be commensurate or incommensurate with the lattice of dots or the periodic domain structure of the F-layer. The transition to the commensurate state strongly enhances the critical fields and currents in the S-layer. My experience in the study of periodic commensurate–incommensurate structures was very useful in predicting of rather complicated properties of the FS-hybrid structures.

The Bose–Einstein condensation (BEC) of magnons in yttrium iron garnet (YIG) film under steady AC pumping was discovered by the experimental team of Münster University led by Sergei Demokritov in 2006 (53). (I first met Sergei in Jülich in 1990, beginning our mutual scientific interest and personal friendship.) Although the number of magnons is not conserved, the magnon lifetime (decay) τ_1 is much longer than its relaxation time τ_r. Therefore, during relaxation, the number of magnons is conserved and they relax to a state with nonzero chemical potential μ. The latter depends on the pumping power and grows as the pumping power grows. When it reaches the minimum magnon energy, it cannot grow any more. If the pumping continues to grow, the new arriving particles go to the state of lowest energy, i.e., to the condensate.

An important peculiarity of F films is that their magnon spectrum has two minima at wave vectors $\pm\mathbf{Q}$ parallel or antiparallel to the spontaneous magnetization $\mathbf{M}_0$ and external magnetic field $\mathbf{H}$, both in the plane of the film. The field $\mathbf{H}$ creates a Zeeman gap in the spectrum of magnons that is the same for both minima. Therefore, the condensate wave function is a superposition of contributions from the two minima. In 2012, the same team found an interference pattern in the condensate density for YIG, with the wave vector of the pattern given by $2\mathbf{Q}$ as it could have been anticipated. We (the student Fuxiang Li, University of Cologne professor Thomas Nattermann, Wayne Saslow, and I) started to work on this problem also in 2012. The main problem was understanding how the magnons of condensate are distributed between the two minima. If they do not interact, then their total energy does not depend on their distribution between the minima. This degeneracy is lifted by their interaction. We calculated the interaction amplitudes for condensate and found that the interaction induces strong violation of the reflection symmetry, and the resulting condensate density in one of minima is much larger than in another (54). Furthermore, the interaction energy that occurs is negative. The homogeneous state is unstable. Because the interaction is weak, the inhomogeneity of the ground state that results from this instability is insignificant.

In 2019, we provided theoretical support for experiments by the Demokritov group (55). They created an additional inhomogeneous magnetic field that served as a potential well for the condensate magnons. The experiment convincingly showed that magnons in this trap repel each other instead of the attraction we and several others predicted. In 2020, I collaborated on another experiment by the same group (56), where they applied a very short pulse of magnetic field that shifted the initial condensate from the minima to four points on the dispersion curve having the same energy. Thus, instead of two fixed condensates they created four moving condensate clouds. Two of them moved opposite to their wave vectors, and the other two moved parallel to their wave vectors, with a velocity significantly slower than that for the back-moving clouds. The intensity measurements for these clouds displayed some asymmetry with respect to reflection $\mathbf{Q} \longleftrightarrow -\mathbf{Q}$, but it was much smaller than predicted by our theory.

What could be the reason for these discrepancies between theory and experiment? A thorough analysis of the assumptions of our theory reveals one weak point: We assumed that the relaxation within a minimum and between minima occurs at the same rate. However, the processes giving interminima relaxation are much weaker. They are provided by the interaction of the condensate magnons with low energy and momenta and thermal magnons with about 100 times larger energy and momenta. These processes are even slower than the decay processes responsible for the finite magnon lifetime. Therefore, in the stationary state, an equilibrium between minima is not established. This state should be found solving the Boltzmann kinetic equation as for other kinetic processes. The existence of coherence between condensates in the two minima shows that these processes are substantial and may provide some asymmetry in the state. However, an unavoidable little asymmetry of the experimental device makes one of these states energy preferred. Although work on this problem is still in progress, we believe that we now understand the most important features of the system and will be able to obtain quantitative agreement with the experiments.

My biographic sketch would be not complete without mentioning two long-lived collaborations with people who became my close friends. First was my collaboration with Thomas Nattermann; it was about 30 years ago when we first worked together during visits to each other. The most cited of our works relate to the theory of hysteresis, but I prefer our works of 2008–2010 (57) on the deep levels of interacting particles in a disordered environment. Without interaction of particles, this problem was solved independently by Ilya Lifshitz (58), by Johannes Zittartz and James Langer (59), and by Bertrand Halperin and Melvin Lax (60) in the years 1966–67 and is called Lifschitz tails. It is rather interesting that the problem with interaction was solved more than 40 years later, though its solution did not require advanced mathematics but simple physical arguments and a thorough analysis of several possible regimes. Depending on parameters (number of particles, strength and correlation length of random potential, and strength of interaction, and for the harmonic trap its oscillator length) it displayed a very rich set of different regimes including nonergodic, one cloud, fragmented (cloud of smaller clouds) and harmonic with the size of the cloud about oscillator length. I also like our work on topological defects in chiral magnets (61).

My second long collaboration is with Danilo Pescia, experimenter and head of the Lab at ETH Zürich. We worked on versatile magnetic systems (mostly magnetic films with different types of structure) and their dynamics, and also on instrumental problems. My close collaborators Igor Lyuksyutov, Alexander Kashuba, and Artem Abanov frequently visited the Pescia laboratory and worked with his team. In turn, Danilo visited us in College Station.

Considering what has happened in my life, I see that I was a fortunate person. I was very happy to marry Svetlana, who made my scientific career possible, who fostered the growth of our children and grandchildren, greeted our common friends, and fed my students. I had great teachers and students. I learned a great deal from them. I had amazing friends and colleagues. I am also lucky to have lived for 90 years and still be in a functioning state. My efforts were awarded with the Landau Prize of the Academy of Sciences of the Soviet Union in 1984 (together with Patashinskii) and the Landau Gold Medal in 2018, as well as the Onsager Prize of the American Physical Society in 2005. These awards are especially dear to me because of the names of people to whom they are dedicated and by my acquaintance and friendship with the remarkable personality and scientist, and the founder of the Onsager Prize, Russell Donnelly. I am grateful to the University of Cologne for nominating me for the Humboldt Prize, which I received in 2001. It gave me the opportunity to spend more time working with Thomas Nattermann and his team and to travel in Germany.

Science and specifically condensed matter science made enormous progress during my life. Every 5–7 years were marked by a discovery of Nobel level: the Ginzburg–Landau theory, the Mössbauer effect, the Landau theory of Fermi liquid, the Bardeen–Cooper–Schrieffer theory of superconductivity and its experimental verification, Abrikosov's theory of two kinds of superconductors, the Josephson effect, theory and experiment on the critical phenomena and second-order phase transitions, the quantum Hall effect, superfluidity of ^3He, quasicrystals, high-temperature superconductivity, and topological insulators. I must also mention impressive progress in experimental devices such as semiconducting transistors and heterojunctions, lasers, the scanning electron microscope, SQUIDs (superconducting quantum interference devices), MRI (magnetic resonance imaging), and ARPES (angle-resolved photoemission spectroscopy). But unfortunately I do not see less growth of hate among humans that threatens to destroy all these achievements. There was enough hatred in ancient times, but now it is much better equipped technically. As unpleasant as it may seem, it must be admitted that it was science that provided hate with technology. Nevertheless, I do not see any way for me to oppose this hate other than showing to young students the beauty of science and helping them to possess the command of its methods.

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