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ORIGINAL PAPER

Autobiography

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Abstract
On the occasion of my 85th birthday, I share insights about my life and science. This autobiography has been written in an interesting period of my life. Two years ago I formulated a new paradigm in a particular scientific discipline: spin exchange. This work has helped me to see that the paradigm formulation is an effective practical tool for increasing the effectiveness of scientific research. Today I am full of plans to use this tool to continue scientific research.

1 Bird's-Eye View

I was born and grew up in a small village with just 29 houses in the southern foothills of the Urals. There was six of us—four brothers (I was the youngest) and two sisters. I believe my family contributed greatly to who I was able to become. I always had many responsibilities, much of it was hard physical labor, and sometimes it would be something simple like scaring off the sparrows to avoid them pecking the young green wheat grains in our garden. When I was at high school, my summer holidays consisted of helping the family prepare hay and firewood for the winter while also working on a collective farm. My parents taught me the importance of responsibility from a young age.

Our village did not have a school so as a general rule, kids would go to the one in the large neighboring Tatar village. However, it happened that I went to elementary school in a small nearby Russian village instead. I then finished secondary school in the regional center, in the city of Belebey. Every Sunday, I would come home (this was a 12 km walk) and then returned to Belebey with enough food for the upcoming week. Almost always I would be put up in the house of a family in which there were children my age from the same school.

Studying was a great pleasure for me, and I always looked forward to the first day of school every year.

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I consider myself lucky to have been taught by the teachers I had, especially in high school. My favorite teachers were Tatyana Konstantinovna Maksimova who taught history, Vera Ivanovna Yusupova who taught mathematics, and Ivan Yakovlevich Frizen who taught us German. They taught me to distinguish between the understandable and incomprehensible, to ask myself questions and look for answers. I enjoyed school.

After finishing high school in 1954, I decided to enroll into the mathematics department at Kazan University, influenced, as it would seem, by my math teacher. As I was applying, a friend of mine told me that boys usually enroll into the physics department. That is how I ended up in physics. As always, you do not know how life will unfold. After all, a year or two ago, we could not even imagine that we would be forced to work in self-isolation conditions as a result of COVID-19.

I kept in touch with my high school through all these years. I support the schooling system in general and enthusiastically accept invitations to speak with school pupils. That was the case in Novosibirsk and continues in Kazan. Communication with school pupils inspires me. After all, this is a meeting with the future.

Even though I ended up in physics quite by accident, I never regretted it. I believe that in any discipline, there are infinitely many interesting things. Countless hours spent with my comrades in the reading room of Kazan University make up some of the best memories for me.

My development as a physicist took place in graduate school at the Institute of High Molecular Compounds (IHMC) of the USSR Academy of Sciences in Leningrad (1959–1962).

For a year (in period 1962–1963), I had to work as a teacher at the Karaganda Polytechnic Institute in Kazakh Soviet Republic.

Over the course of 25 years of work at the Institute of Chemical Kinetics and Combustion of the Siberian Branch of the Russian Academy of Sciences (ICKC SB RAS) in Novosibirsk Akademgorodok, I made my way from Junior to Chief Researcher. Based on the results obtained in Akademgorodok in 1967–1972, I received my second doctorate (habilitation) in 1973. The defense of the dissertation took place at the Council for the Defense of Dissertations at the Kazan University under the chairmanship of S.A. Altshuler.

Key scientific achievements during my time in Novosibirsk were:

1. A foundational contribution to the theory of magnetic and spin effects in radical chemical reactions (for this, in 1986, I received the country's highest honor at the time: the Lenin Prize together with Yu.N. Molin, R.Z. Sagdeev from Novosibirsk, and A.L. Buchachenko and E.L. Frankevich from Moscow).
2. A fundamental contribution to the theory of pulse EPR spectroscopy.
3. Founding contributions to the theory of spin exchange and the spin probe method.
4. Revision of the widely spread theory of paramagnetic relaxation of spins in liquids, in particular, due to the dipole–dipole interaction.

5. Kinetic equations for the one-particle spin density matrix of paramagnetic particles in dilute solutions, taking into account the change in the state of electron spins during bimolecular collisions (e.g., taking into account the spin exchange, recombination of free radicals, triplet–triplet annihilation) and taking into account the contribution of the dipole–dipole interaction of particles.

In the late 1980s, Perestroika began in our country. Employees at the institutes of the USSR Academy of Sciences were now given the right to recommend candidates for the position of directors. In this whirlwind of events, I was elected Director of the Kazan Physical-Technical Institute of RAS (KPhTI). KPhTI is named after E.K. Zavoisky, a pioneer in the study of the phenomenon of electron paramagnetic resonance (EPR). My scientific results obtained in Akademgorodok were directly related to EPR. This is partly why I was a good fit as Director of KPhTI RAS.

In 1988, I set a goal that managed to 'infect' not only the staff of the institute, but also many people in Tatarstan, in Russia and abroad—to make KPhTI one of the best centers of EPR spectroscopy in the world.

After 27 years of work as the director of KPhTI RAS, in my report to the general meeting of the institute's employees, I summed up my work and the work of KPhTI in the following way: "Imagine a person who has worked tirelessly all day and by the evening, is satisfied with the work he has done. This is the feeling I experienced."

The Zavoisky Institute today is an internationally recognized EPR center. On the occasion of my 80th birthday, the former President of the AMPERE society and the world magnetic resonance society (ISMAR) Hans Wolfgang Spiess (Max Planck Institute, Mainz, Germany) had this to say in Zeitschrift fuer Physikalische Chemie: "Your enthusiasm for the benefit of your research and the global scientific community is unprecedented. You made the Kazan Institute of Physics and Technology named after E.K. Zavoisky a Centre for EPR Spectroscopy, recognized in all countries".

I have quite a lot of evidence of the high appraisal my work has received by the world scientific community. Of course, it is inspiring. In addition, today the support and appreciation of my work from my colleagues in the scientific community, including the Kazan Physical-Technical Institute, the Academy of Sciences of the Republic of Tatarstan, and the Russian Academy of Sciences, are very dear to me. With deep gratitude, I recall the constant support of the leadership of the Republic of Tatarstan for my initiatives in physics and science in general. I am very grateful to the City Council of Deputies and the administration of the city of Kazan for electing me an Honorary Citizen of the city of Kazan.

Here are some key scientific results I was able to achieve after moving to Kazan:

1. Creation of a new paradigm of spin exchange and its manifestation in the EPR spectra of dilute solutions.

2. Calculations of the effective radius of spin exchange between charged paramagnetic particles in their bimolecular collisions in solutions (together with graduate student A.E. Mambetov).
3. Theoretical prediction of the formation of spin excitons and spin polaritons in dilute solutions of paramagnetic particles.
4. The theory of pulse double electron–electron resonance for paramagnetic centers with overlapping EPR spectra (together with graduate students R.B. Zaripov and I.T. Khairutdinov).
5. Protocol for EPR tomography of conducting and lossy—dielectric samples (together with K.L Aminov and graduate student M.P. Tseitlin).
6. Suggestion of a new mechanism for the hyperpolarization of electron spins of triplet excitons, induced by the spin selection rules for triplet–triplet annihilation and its experimental confirmation (together with C. Corvaja., L. Franco, V.K. Voronkova).
7. Peculiar manifestations of entangled states of electron spins in EPR spectroscopy of separated charges, which are formed in the singlet state at the primary stage of the conversion of solar energy into chemical energy in plants:
   - Quantum beats of line intensity in the EPR spectrum (together with D. Stehlik);
   - Anomalous phase of the electron spin echo signal (together with students Yu.E. Kandrashkin and A.K. Salikhov);
   - Line of two-quantum transition in the optically detected EPR spectrum even in the absence of spin–spin interaction between separated charges (together with H. Hayashi and Y. Sakaguchi).
8. Molecular model of the cryoprotective effect of trehalose in photosystem I (jointly with A.Yu. Semenov, M.D. Mamedov, and A.A. Sukhanov).
9. Original protocol of quantum teleportation on electron spins using spin selection rules for an elementary chemical act as a logic operation.
10. Protocol for the implementation of the quantum logic operation controlled-not (CNOT) using electron spins as qubits (together with graduate student M.Yu. Volkov).
11. Interpretation of the deceleration of spin decoherence in multi-pulse experiments as a manifestation of the quantum Zeno effect (jointly with E.L. Vavilova, R.B. Zaripov, V. Kataev, and B. Büchner).
12. Theoretical prediction and experimental confirmation of the red shift of the modulation frequency of echo signals, induced by dipole–dipole interaction in a pair of localized spins in the reaction center of photosynthetic systems, due to random modulation of the dipole–dipole interaction by the process of spin–lattice relaxation (together with A.A. Sukhanov).

I consider the organizational achievements over the 27 years of my work as director of KPhTI RAS to be the following:

1. Initiations of new scientific directions at KPhTI RAS:

- Pulse EPR spectroscopy. In this area of science, KPhTI RAS became one of the best centers in the world.
- Tunnel and atomic force microscopy.
- Femtosecond spectroscopy.
- Quantum computing.
- Medical low-field magnetic resonance tomography.

2. Creation of a specialized Academic Council for awarding the degrees of Doctor and Candidate of Physical and Mathematical Sciences (1988).
3. Creation of the international journal “Applied Magnetic Resonance” you are reading now. It is currently on its 53rd volume (1990).
4. Establishment of the international Zavoisky Award for outstanding contributions to the development and application of EPR spectroscopy (1991).
5. Carrying out, for the first time on the territory of Russia, the Ampere Congress in 1994.
6. The traditional annual international conference ‘Modern Development of Magnetic Resonance’ (1991).
7. Creation of the annals of KPhTI RAS: YEARBOOK-KPhTI (annually since 2001)

Looking back at the Kazan period in my life, I am delighted when I think about my participation in the creation, formation and long-term work at the Academy of Sciences of the Republic of Tatarstan. By the decision of the first President of Tatarstan Mintimer Sharipovich Shaimiev, a commission was created to prepare the charter of the Academy of Sciences of Tatarstan. For many months, at the meetings of this commission, we regularly and vigorously discussed the principles of the organization and activities of the proposed Academy of Sciences. After 30 years, we can confidently say that the Academy of Sciences of Tatarstan has found its place. It played an outstanding role in the preservation of scientific traditions in Tatarstan, in its successful development during the very difficult years of Perestroika. For more than 25 years, I was the Vice-President of the Academy of Sciences of Tatarstan, and now I continue to work as a member of the Presidium. I took an active part in the creation of the Encyclopedia of Tatarstan and the Atlas of Tatarstan. Thanks to the Academy of Sciences of Tatarstan, I met and made friends with many scientists of the Republic.

1.1 Briefly about the family

I met my future wife Zoya at the House of graduate students of the USSR Academy of Sciences in Leningrad in 1960. She was seconded from the Novosibirsk Polytechnic Institute. Later we got married. After graduate school, I was assigned to work in the city of Karaganda in Kazakhstan. We soon moved from Karaganda to Novosibirsk and found work in Akademgorodok.

We lived with my wife Zoya Vasilievna for 54 years and raised two children. Now, six more grandchildren, a great-grandson and four great-granddaughters are growing up. I have close relationships with my family, which is very supportive. It is a real delight to watch kids grow and I am looking forward to spending many more years of joy with them.

2 Some Details and Circumstances of My Work

2.1 Study at Kazan University (1954–1959)

The first 2 years of study were devoted to general education: higher mathematics and general physics. When the time came to choose a narrow specialization, I did not hesitate to choose the department of theoretical physics and became a student of Semyon Aleksandrovich Altshuler, a colleague of the pioneer of EPR spectroscopy, Evgeny Konstantinovich Zavoisky. At his request, I translated a classic article by H.A. Bethe for the department, in which he studied the states of a linear spin 1/2 chain with antiferromagnetic exchange interaction. In my fourth year, my term paper related to the classic work of R. Kubo and K. Tomita in linear response theory. Both these articles made a great impression on me. After completing the 5th year, I would have liked to continue my studies as a postgraduate student of S.A. Altshuler, and certainly specialize in EPR spectroscopy.

But in the fall of 1958, Semyon Aleksandrovich invited me into his office and said that the academic council of the faculty had decided to open a new topical specialty in polymer physics and that A.I. Maklakov who was leading this work, personally chose me along with a few other students to join him.

I had the choice to remain a student at the Department of Theoretical Physics or completely change my area of research, go to the Department of Molecular Physics, quickly settle my assignments and go to Leningrad to the Institute of Macromolecular Compounds of the USSR Academy of Sciences (IHMC) to Mikhail Vladimirovich Volkenstein to complete my diploma work. At the same time, it was said that after graduating from the university, KSU would send me to IHMC to complete and write my Ph.D. thesis if M.V. Volkenstein will find it appropriate. I successfully completed my diploma work, where I quantitatively developed the idea of M.V. Volkenstein on the absorption of ultrasound in concentrated polymer solutions.

2.2 Graduate Studies at the IHMC of the USSR Academy of Sciences in Leningrad (1959–1962)

As a result of my efforts, I became a graduate student (aspirant) of M.V. Vol’kenshtein. Yuliy Yakovlevich Gotlib was my daily mentor. I went through a wonderful school of scientific creativity in the laboratory of M.V. Volkenstein. I was tasked with creating a theory of dielectric losses in polymer glasses. At the institute in the laboratory of G.P. Mikhailov, experimental research in this area was conducted. It was necessary to theoretically describe the experimental results obtained by them. Therefore, I immediately became a regular member at scientific seminars also in the laboratory of dielectric losses.

In polymer molecules, orientational relaxation of the electric dipole moments of chain monomers occurs not independently of each other, but in a cooperative manner. M.V. Volkenstein proposed kinetic equations that describe cooperative configurational transformations of polymer chains. I had to find the distribution of characteristic relaxation times. I was able to show that the kinetic equations formulated by M.V. Volkenstein, can be reduced to such a form when one can use the van Hove theorem, well known in solid-state physics, for the phonon frequency distribution in an n-dimensional crystal. I found the distribution function of the relaxation times of the conformational transformations of polymer molecules.

Experiments on dielectric losses in polymer glasses showed that there are three characteristic frequency ranges, the so-called α, β, γ losses. α-Losses are attributed to losses due to the segmental motion of polymer molecules, i.e., cooperative, coordinated movement of polymer chains. It can be imagined that a segment containing about 20 monomers can experience conformational transitions quasi-independently. γ-Losses are associated with relatively fast rotations around the C–C bonds of the final monomers of the chain. β-Losses are associated with relaxation losses in the region of intermediate frequencies between the frequencies of α- and γ-losses.

It can be expected that the cooperative motions of polymer chains can be characterized by a very wide set of characteristic times and there can be a wide distribution function of relaxation times. But this broad distribution function, as experiments on dielectric losses showed, turned out to be a two-hump; there are two characteristic regions of relaxation times.

To explain the distribution of relaxation times with two maxima, I proposed the following idea. I assumed that the segmental movement of polymer chains must, of course, be correlated. But it is easy to imagine that this alignment would not be as perfect as the movement of the gears in a Swiss watch. There will be random mistakes. If we imagine a curve that describes the motion of polymer molecules in the multidimensional space of atomic coordinates, which in statistical mechanics is called configuration space, then for a strictly cooperative motion of the polymer, this curve will change rather monotonically with time. These changes will give α-relaxation. If the cooperation is not error-free, then this curve will be 'smeared' into a certain band. The characteristic times of the processes that create these 'erroneous' movements will give β-relaxation. This idea is supported by the experimental fact that the α and β relaxation regions merge into one with increasing temperature.

My circumstances developed in such a way that later I did not engage in further development of my idea of the characteristic manifestation of the 'errors' of the cooperative movement of polymer chains. I think this idea may be relevant to a number of other problems as well. As far as I know, this idea was further developed by Yu.Ya. Gotlib in the IHMC.

2.3 Work at the Institute of Chemical Kinetics and Combustion (ICKC) SB RAS in Akademgorodok (Novosibirsk) and at Novosibirsk State University (NSU) (1963–1988)

I relocated from Karaganda to Akademgorodok in Novosibirsk with the desire to work at the Institute of Chemical Kinetics and Combustion. This institute was recommended to me by Oleg Borisovich Ptitsyn, a leading scientist in the laboratory of M.V. Volkenstein. Oleg Borisovich gave me a letter addressed to V.V. Voevodsky. After reading this letter, V.V. Voevodsky immediately hired me.

In Novosibirsk Akademgorodok, I became part of a wonderful team of young scientists who were graduated in the Moscow Institute of Physics and Technology and were invited by V.V. Voevodsky to Siberia. There was a very creative atmosphere. I consider the years of work in the ICKC as a gift of fate. As a theoretical physicist, I happened to take part and make a significant contribution to the formation and development of several scientific disciplines: pulse spectroscopy of electron paramagnetic resonance, spin chemistry, and spin exchange in solutions of paramagnetic particles.

For me, joining the ICKC in Akademgorodok (Novosibirsk) meant a complete change in the field of scientific activity in comparison with the IHMC in Leningrad. Therefore, it took me 2–3 years before I was fully accustomed to the problems that were being solved in the ICKC.

V.V. Voevodsky (VV, as we would call him when he was not around), when hiring, set me the task of studying the theory of heterogeneous catalysis on the surface of magnetic materials. In particular, he was interested in the question of whether the phase transition paramagnet-antiferromagnet or paramagnet-ferromagnet can somehow affect the catalytic activity.

Reflecting on this task, I remembered Bethe's article on elementary excitations in a chain of spins, which I already mentioned, and thought that, unlike the paramagnetic state, bound states of two magnons can form in ferromagnets and antiferromagnets: called bimagnons. I theoretically found the bimagnons and published the results. I continued to ponder whether bimagnons could act as catalytically active centers on the surface of a magnetically ordered catalyst. However, the circumstances were such that soon this topic of catalysis ceased to be actively developed at our institute.

In the 1960s, there was great interest in studying the effect of radiation on substances. This topic was also in the focus of scientific interests of V.V.'s laboratory. Therefore, I became interested in this problem. I studied the theory of elementary chemical acts and chemical kinetics. I began to give a special corresponding course of lectures at the Department of Chemical Physics of the Faculty of Natural Sciences at Novosibirsk State University. NSU published my first small textbook for students 'Introduction to the theory of chemical reactions'. Later I also gave a 2-year general course in physics to students of the Faculty of Natural Sciences. This allowed me to significantly expand the horizons of my scientific interests and knowledge.

For radiation chemistry, it is important to understand what the primary chemical act consists of, how the primary effect manifests itself under the action of radiation. As I found out, in the radiation chemistry of organic solids, it was generally accepted that radiation creates molecules in an electronically excited state and that these excited molecules dissociate by breaking the weakest chemical bond.

Reflecting on this, I thought that there may be another fundamental principle: in the primary act, the covalent bond is broken, on which the excitation is most likely localized. Of course, one can expect that it is the weakest bond that can be the most probable site of localization of excitation. But this is optional.

I presented my idea at the National Conference at the ICKC. It was met without any enthusiasm. During a break, Nikolai Yakovlevich Buben (Institute of Chemical Physics, USSR Academy of Sciences, Moscow) approached me and encouraged me. He said that they encountered a situation in an experiment that supported my idea.

> That was my first memorable experience when a new theoretical discovery was not immediately accepted by the scientific community. Looking back, I now understand that any new idea requires persistence to break through and become accepted. Therefore, today I aim to pay more attention to new ideas (whether mine or my colleagues) to ensure as a community we make the best use of new scientific development.
> By the way, establishing the 'Applied Magnetic Resonance' magazine is one of my ways to contribute to better communication about new ideas and experiments in this discipline.

At the same time, I got acquainted with another interesting discipline: solid-state chemistry. The laboratory of Vladimir Vyacheslavovich Boldyrev dealt with this problem at the ICKC. One day, on the way home from the institute, he told me about an interesting effect, which he called the scissor effect: it turned out that in the series of inorganic crystals they studied, the thermal and radiation resistances change in the opposite way. Immediately, I got the idea that this can be explained by different manifestations of free volume in the thermal and radiation stability of crystals. V.V. Boldyrev recently told me that this idea is still alive and finds application. Throughout the years, several times, I have turned to the problems of solid-state chemistry.

The area of my interests changed a lot when Anatoly Grigorievich Semenov and Vsevolod Evgenievich Khmelinsky at the institute created a very good pulse EPR spectrometer (1966).

V.V. instructed the laboratory of Yuri Dmitrievich Tsvetkov to develop an EPR experiment protocol to determine the local concentration of free radicals under irradiation of organic materials.

The first experiments were made by Arnold Moiseevich Raitsimring and Valery Fedorovich Yudanov. During the experiments, V.F. Yudanov obtained an unexpected result. According to the theory developed in the work of E.L. Hahn, W.B. Mims and L.G. Rowan, one would expect that the decay of the signal of the so-called primary electron spin echo should oscillate in time. These oscillations should have their origin in the dipole–dipole interaction of an unpaired electron with magnetic nuclei. In addition, for impurity paramagnetic centers in single crystals W. Mims observed this modulation.

However, in the experiments of V.F. Yudanov, the expected oscillations of the primary electron spin echo signal did not appear in the case of radicals that are formed and stabilized in irradiated frozen malonic acid.

Upon learning about this result, I became very interested. In addition, I understood the reason. It turned out that in our spectrometer the attainable powers of microwave pulses forming an echo signal could not provide nonselective excitation of electron spins, as was assumed in the theory of E. Hahn and his colleagues. In the experiments of V. Yudanov, the microwave pulses used for excitation of spins were rather weak, the power of the microwave field in the indicated experiments was insufficient for noticeable excitation of forbidden transitions causing the necessary spin flip of protons.

Soon, modulation effects in the decay of the signal of the primary spin echo were observed for other radicals, which we were able to explain taking into account the excitation pattern of spins by microwave pulses that form the echo signal. This was the first time the selective spin excitation effect was demonstrated in pulse EPR spectroscopy. Since this successful experience of cooperation, for more than two decades I have been in the role of a 'resident theorist' in the laboratory of Yu.D. Tsvetkov.

We had a coffee club. Every day we got together twice a day, drank tea and coffee and discussed current affairs in science. In the late 1960s and early 1970s, pulse EPR methods were intensively developed only in two laboratories: Bell Telephone and at ICKC SB RAS. Therefore, everything was new. It often happened like this: someone reports a new observation in an experiment, and in the coming days, I would bring the results of my calculations to the coffee club and we would discuss it all, and vice versa—I would have an idea; some idea of a possible experiment and soon one of the experimenters would bring the results of the experiment and we will again, discuss it all and move forward in understanding the problem.

From the very beginning, it became clear that in solids, the most important contribution to the relaxation of spin coherence (phase relaxation) of electrons is made by the spin–spin dipole–dipole interaction between the free radicals under study.

The theory of paramagnetic relaxation of electron spins due to their dipole–dipole interaction in solids is a very serious challenge for the theory. For two reasons:

- First, this interaction is long-range. It decreases with increasing distance r between the two spins according to the $1/r^3$ law. Therefore, the total contribution of the interaction of the selected spin with all other spins has a logarithmic divergence in r.
- Second, we are dealing with dilute paramagnetic centers. In this case, very large fluctuations of the local environment of a given spin by other spins appear.

Both these circumstances make it difficult to distinguish a certain characteristic time of paramagnetic relaxation of spin coherence due to the dipole–dipole interaction.

Together with Alexander Georgievich Maryasov, Alexander Dmitrievich Milov, A.M. Raitsimring and Yu.D. Tsvetkov, we carried out theoretical and experimental studies of the manifestation of the dipole–dipole interaction of electron spins in the kinetics of the decay of the spin echo signals. The results obtained have created a good basis for the further development and application of pulse EPR spectroscopy for studying the spatial distribution of organic free radicals in solids during photolysis and radiolysis.

These studies formed the basis for the method of measuring the distance between particles in spin pairs and clusters, which is widely used today.

I would also like to note that I revised the accepted theory of paramagnetic relaxation due to dipole–dipole interaction in nonviscous liquids. In textbooks, including the famous book by A. Abraham, in the derivation of kinetic equations, the transfer of quantum coherence to a given spin from the interaction partners is neglected. This means, for example, that the dipole–dipole interaction cannot cause the collapse of the inhomogeneously broadened EPR spectrum into one homogeneously broadened line. I fixed this error and got new kinetic equations. It turned out that the dipole–dipole interaction between paramagnetic particles in nonviscous liquids leads to the transfer of quantum coherence to a given spin from other spins, and this transfer occurs with a change in the coherence phase equal to π! This leads to interesting consequences in the form of EPR spectra of spin probes in nonviscous solutions, as was shown later in our joint work with graduate student Ravil Bulatovich Galeev (KPhTI).

I was lucky to be in the right place at the right time.

Sometime at the beginning of 1972, Yuri Nikolaevich Molin invited me to his office and said that a student of Tatyana Victorovna Leshina, M.A. Kamkha, in the laboratory of Renad Zinnurovich Sagdeev observed a change in the ratio of the products of a radical reaction when passing from the Earth's magnetic field to the magnetic field of the NMR spectrometer. Yu.N. Molin asked for a theoretical estimate of the possible magnetic effect in the studied reaction. For about 2 h, I was solving this problem on a blackboard. I calculated the effect of a magnetic field on the recombination of two identical radicals, each of which had a hyperfine interaction with one proton. In addition, I showed that in this model system, a magnetic effect on the product yield of up to 10% is possible.

The experimental data and my calculations were presented at an international conference in Tallinn in a report by T. V. Leshina. After Leshina's speech, Gerhard Closs said that this could not be the case and referred to Robert Kaptein, who was also present in the audience. Robert told the audience that his graduate student Hollander did studies on this subject.

I had no calculations with me, so I recalculated everything. In the morning before breakfast, I met with Hollander and showed the calculations, and gave them to him to study. Closing the conference, G. Closs said that our message on the magnetic effect will become the most important event of the conference if the result is repeated. Several months later, new evidence of the magnetic effect in radical reactions appeared.

> That was probably the first time when I got seriously noticed on the international arena by fellow scientists. Since then, I have earned a reputation of 'solution provider'. In the following years, leading EPR laboratories invited

me to work with them in order to explain experimental results they could not interpret with the existing level of understanding.

1968–1984 were the years when almost every publication I was involved in contained a new or revised theoretical explanation, as a result of new experimental data, which otherwise might have been left aside or kept as an unexplained mystery.

Together with graduate student Pyotr Aleksandrovich Purtov, we analyzed in detail the magnetic effect for a pair of radicals, in which only one of the radicals had a hyperfine interaction with one proton. It turned out that in this case, the magnetic effect is not expected when comparing the Earth's magnetic field and a field that is much larger. True, the magnetic effect for the probability of recombination of a pair of radicals passes through an extremum in an external field comparable to the field of the hyperfine interaction. Apparently, Hollander calculated the probability of recombination of a pair of radicals with one proton in a zero field and in a very strong field. In this case, the probability of pair recombination should be practically the same.

This is how I became a participant in the creation and development of spin chemistry.

A significant part of spin chemistry is the nonequilibrium polarization of the spins of electrons and nuclei in the course of chemical and physical spin-dependent processes. From my results on chemical spin polarization, I want to highlight two examples.

In 1975, at the end of the working day, my graduate student F.S. Sarvarov reminded me that we urgently need to present the title and abstract of our report to the All-Soviet Union conference in Kiev. I somehow completely lost sight of this matter. In a complete frenzy, I began to brainstorm out loud what kind of report we could present. Then, I got the idea of how it would be possible to forcefully calculate the chemical hyperpolarization of spins.

Usually, one calculates the change in the wave function or density matrix of the system under consideration, in which the formation of nonequilibrium spin polarization occurs. After that, the average value of the operator of the experimentally observed value is calculated.

I suggested calculating a quantity directly measured by experiment. For this, it is convenient to use the Heisenberg equation. This approach is especially convenient for calculating spin hyperpolarization in reactions involving short-lived radical pairs. Suppose we need to calculate the nonequilibrium polarization of spin of a given nucleus in the recombination product of the geminal radical pair. This polarization is proportional to the mean value of the operator Q_z, which is the projection of the longitudinal I_z component of the nuclear spin into the subspace of the singlet state of the electrons of the radical pair.

After that, it is necessary to write down the Heisenberg equation for the operator Q_z and its derivatives. At each stage, it is necessary to calculate the average value of the derivative for a given initial spin state of the radical pair until a nonzero value is obtained. This approach has proven to be rather effective.

In 1982, to describe the experimental data obtained in the laboratory of R.Z. Sagdeev, I numerically calculated the polarization of nuclei in the products of radical recombination and changed the parameters to fit the calculation to the experimental data. Suddenly, for one set of parameters, the integral polarization of the nuclear spin changed sign. By this time, the rules for the sign of the integral polarization of nuclear spins were recognized, which are named after R. Kaptein, G. Closs, L.J. Oosterhoff (KCO). According to these rules, the sign of the integral polarization should not have changed when changing the parameters in my numerical calculations. I double-checked my calculations carefully. It all happened again.

I understood the nature of the KCO rules' violation that I had discovered. It is associated with the mutual influence of different nuclei on their chemical polarization. When I presented my work at an international conference at the House of Scientists in Akademgorodok, Robert Kaptein was rather skeptical about my report. However, just a few months later, it was Robert who was the first to experimentally confirm my theoretically predicted violation of the KCO rules in the effect of chemical polarization of nuclei (CIDNP).

Later on, this became part of the new paradigm of CIDNP.

My participation in the development of research on spin exchange began with a conversation with Yu.N. Molin (1969). He told me that his student G.I. Skubnevskaya did an experimental observation that has no explanation. Experience has shown a strong dependence of the exchange broadening of the EPR spectrum of a radical on the rate of paramagnetic relaxation of a complex of paramagnetic ions.

I suggested why this might be. He said that he thought the same, but wanted to have a formula for quantitative comparison with an experiment. Yu.N. Molin also said that he had approached several theorists before me, and they said that it would not be easy to get the corresponding formula. I brought him the formula a day or two later, and we wrote an article about it. Therefore, I got acquainted with a new field of science for myself—spin exchange.

The next step was interaction with Kirill Ilyich Zamaraev. In his group at the Institute of Chemical Physics of the Academy of Sciences of the USSR in Moscow, the rate constant of spin exchange between the radical and the complexes of divalent manganese ions was measured in an experiment. In addition, they got an order of magnitude different value depending on which components of the EPR spectrum, for the radical or the complex, they used to determine the spin exchange rate constant. This contradicted the model adopted at that time, in which spin exchange was treated as a common chemical reaction.

For more than a year, Kirill Zamaraev could not get an explanation for his observation, although he turned to several theorists in Moscow and Akademgorodok. His graduate student A. T. Nikitaev, being on a business trip in 1970 from Moscow to Akademgorodok, told me about their problem. During the discussion, I proposed kinetic equations that could provide a solution to the problem. He went on to tell me that there are other, more complicated equations in the widely available literature for this case. We went to the library and he found the necessary Linden Bell equations. Looking at them, I saw that they can be converted to the form I wrote. A few days later I sent the solution to the problem to K.I. Zamaraev in Moscow, and we wrote a joint article (1971).

Later on, I kept returning to this problem from time to time. When writing a book on spin exchange in 1976 together with K.I. Zamaraev and Yu.N. Molin, in the theoretical part, in the framework of perturbation theory I showed that the slow transfer of spin coherence leads to the formation of collective modes of motion of quantum coherence. These collective modes give spectral lines of a mixed shape, i.e., they represent the sum of absorption and dispersion, and the contribution of dispersion is different for different modes (different spectral lines).

Later I showed that due to repeated collisions of particles, there is a shift in the resonance frequency of paramagnetic particles associated with the interference of the exchange interaction of the colliding pair of particles and the hyperfine interaction of electrons with magnetic nuclei (1985). Several more studies have been performed. In addition, all this ended with the fact that in 2019, I formulated a new spin exchange paradigm (see below). When it started 50 years ago, I did not think at all that it would become a long and exciting story for me.

2.4 Work at the Kazan Physical-Technical Institute (KPhTI) RAS and Kazan University (Since 1988/Present Time)

2.4.1 Scientific and Organizational Activities

Moving to Kazan in early 1988 was, of course, a big change in the life of my family. In our country, it is not typical to change your place of residence. Nobody and nothing drove me from Akademgorodok. Everything was fine for me there.

As it turned out, I had a long road ahead since I still had to win the trust and support of my new colleagues at the Zavoisky institute in Kazan. I had definite goals to turn KPhTI into one of the leaders of world science in the field of electron paramagnetic resonance (EPR). However, the 5-year term I was elected as Director for, simply was not enough for such a task.

It was necessary to quickly achieve a tangible result to get support at the Institute.

My first step was to create an all-institute seminar. I tried to make the most of my experience of seminars at the Institute of Chemical Kinetics and Combustion of the SB RAS. It is surprising, but there was no such seminar at the Zavoisky Institute! Moreover, in response to my proposal to create it, I received a number of requests from KPhTI colleagues, in which considerations of the uselessness of such a seminar were stated. Now, more than 30 years later, it is safe to say that the scientific seminar of the Zavoisky institute made a great contribution to the creation of a demanding and business-like creative atmosphere at the institute, it has become a place for study, advanced training of researchers, and a tool for enhancing scientific culture at the institute.

KPhTI has never had a dissertation scientific council with the right to confer the degrees of Candidate and Doctor of Sciences. It turned out that KPhTI's repeated requests to create a council to the Higher Attestation Commission in Moscow did not receive support. I considered the presence of such a council to be fundamentally important for positioning KPhTI as a leader in the field of EPR. I dreamed that scientists from our country from different cities would come to KPhTI to defend their dissertations. With all the necessary documents, I went to an appointment with the Chairman of the Higher Attestation Commission and explained the situation. He supported our appeal. Soon our dissertation council was opened. This gave me good support from colleagues at the institute.

At the time of my joining the KPhTI, there was only one intern. I considered it absolutely necessary to create a Kazan state university chair at KPhTI, which would work on the principles of the Novosibirsk university chairs on the basis of the SB RAS institutes in Akademgorodok. I estimated that to achieve adequate momentum for KPhTI, 4–5 young people would need to be annually admitted as graduate students or interns. The then-leadership of KSU supported my initiative and with KPhTI as their base, the Chair of Chemical Physics of KSU was created (1989). Most graduates of the department go on to work at KPhTI and other scientific organizations. Speaking about the Chair of KSU based at KPhTI, I recall examples of support from my colleagues. Peter Atkins made a great gift. He flew to Kazan and gave several lectures to the students of the chair. As expected, the students were delighted. I note that I was only able to pay for his hotel accommodation in Kazan. A few years later, Dietmar Stehlik and Klaus Moebius helped organize a 2-week trip for all students of the chair to Berlin, to the Free University. It was, of course, wonderful.

Literally a month later, when I started working as a director of the Zavoisky Institute, the All-Soviet Union conference on Magnetic Resonance was held in Kazan (June, 1988). All the preparatory work had been done even before I arrived in Kazan. In general, I was practically not involved in the work of this conference, which was organized by KSU and KPhTI. I was invited to the closing of the conference.

At the closing of the conference, the Chairperson read out a brief summary of the conference, which, on behalf of all conference participants, was then transmitted to the Scientific Council on Radio Spectroscopy in the Physics and Astronomy Division of the USSR Academy of Sciences. I suggested adding one sentence: that the conference participants should consider it necessary to create a national journal of the Academy of Sciences of the USSR on magnetic resonance and its applications. They began to deny my proposal quite violently. They referred to the fact that similar proposals were made more than once at previous conferences and did not lead to any result. I did not know this background, since before I had rarely participated in scientific conferences outside Akademgorodok. I got up and once again asked to include my proposal in the decision of the conference. I did not know exactly why I needed it. But I assumed that this could somehow help in realizing my long-time dream to create a magazine on magnetic resonance in the homeland of the discovery of paramagnetic resonance.

My proposal was included in the decision by a vote. Soon I got the decision of the USSR Ministry of Communications to create an international journal 'Applied Magnetic Resonance' (1990). The money for the creation of the magazine, for the purchase of paper, and the distribution of the magazine around the world were allocated by our partner Springer Publishing House in Vienna. Of course, many memories are associated with the creation at that moment of the only scientific journal in the USSR in English. In the context of this autobiography, these details are not important. It is important that this journal has occupied and continues to occupy a significant place in the history of the Kazan Physical-Technical Institute. It also helped my colleagues at the institute to believe in my dream to turn the Zavoisky Institute into one of the leading centers of world science in the field of electron paramagnetic resonance.

While working on the Applied Magnetic Resonance magazine, I acquired at the Zavoisky Institute wonderful associates, Laila Vasilievna Mosina and Sergei Mikhailovich Akhmin.

The achievement of my strategic goals was facilitated by the presentation of the annual international Zavoisky Award for an outstanding contribution to the development and application of EPR methods (1991). The idea of creating this award came to me at the General Meeting of the Academy of Sciences of the USSR, at which members of the meeting voted against creating a prize (or medal) named after Zavoisky. I contacted all the world's magnetic resonance societies and received their support for the creation of the Zavoisky Award. This proposal was also supported by the Presidium of the Russian Academy of Sciences. This award was created 30 years ago, when in our country, basically, the economy was destroyed. For 20 years, the Award money was paid by Springer Publishing House, with which we cooperate on the magazine. About 10 years ago, by the Decree of the President of the Republic of Tatarstan, the Government of Tatarstan also became the co-founder of the award, and now the Award is paid by the Government of the Republic of Tatarstan and the Springer Publishing House. The awarding ceremony and related events are a great celebration of science in the Republic of Tatarstan and its capital—Kazan.

In the midst of Perestroika in 1994, the Ampere Congress was held in Kazan for the first time in Russia. More than 160 foreign colleagues from all over the world participated in the meeting. Huge obstacles had to be overcome.

First, the President of the Ampere Society was very skeptical about the organization of the jubilee congress in a country, which was going through a turbulent period. Fortunately, I was familiar at that moment with the Honorary President of the Ampere Society Karl Hausser. In addition, at the meeting of the Bureau of the Ampere Committee in Zurich, at which I defended the proposal to hold the congress in Kazan, he supported the candidacy of Kazan with his usual energy and temperament. Our colleague from Poland Jan Stankovsky joined him. As a result, the Ampere Committee decided in favor of Kazan.

Second, it is hard to believe today that many of our international colleagues categorically refused to participate, as they were afraid of a transfer in Moscow. They agreed to arrive if there was a direct charter flight from Frankfurt to Kazan. Immediately before the start of the congress, there were serious unforeseen difficulties with charter flights, which called into question their implementation. However, thanks to the President of the Republic of Tatarstan, Mintimer Sharipovich Shaimiev, these problems were quickly solved.

Some arrived via Moscow. Erwin Hahn was among them. Erwin traveled from Moscow to Kazan by train. I met him at the station. When he appeared at the exit of the carriage and saw me, before even setting foot out of the carriage, he said: "Kev, well, you know how to persuade". The participation of Erwin Hahn, Richard Ernst (Nobel Prize winner) and many other famous scientists made this anniversary meeting of scientists an outstanding scientific event, and a historical and unforgettable event for Kazan.

2.4.2 About My Work as Director of Zavoisky Institute (1988–2015)

In accordance with the program announced when choosing the director, I formulated a project for the creation of a Centre of Excellence in EPR Spectroscopy at KPhTI RAS.

Of course, strengthening the instrumental base was the subject of my special concern as the director of KPhTI.

It was in the early 1980s that the renaissance of EPR spectroscopy began: pulse EPR spectroscopy began to develop rapidly, in the formation of which I took an active part in the late 1960s and 1970s. The application for the purchase of a pulse spectrometer, substantiated and agreed upon by 17 authorities, had to be prepared three times before a positive decision was reached. But we did not have time to receive the money, as the people who made the decision changed so we then had to justify the purchase anew. However, in the end, today, the Zavoisky Institute in the field of EPR has the best instrumentation in Russia.

Of course, my primary concern with EPR does not mean that I have not paid attention to other areas of study. For example, we managed to get a good nuclear magnetic resonance spectrometer and an electron microscope.

The end of the 1980s marked the beginning of an absolutely remarkable era of tunnel and later atomic force scanning microscopy. I phoned the Assistant of the Prime Minister of the Republic of Tatarstan and for half a working day I met with government officials at different levels and told them what a nanometer is, how these microscopes can be used to study objects with nanometer and even subnanometer resolution, to literally manipulate atoms. In passing, I also noted that in Tatarstan there is not a single such branded microscope. KPhTI already had a handmade tunnel microscope, but it was still far behind the best world achievements. In the fifth hour of my enlightenment talk, the Assistant of the Prime Minister asks me how much a good microscope costs. After a while, the Government of Tatarstan made us happy and bought a microscope made in Zelenograd near Moscow. In addition, today this area of work is developing very successfully at the Zavoisky Institute in the A.A. Bukharaev lab.

It was in the difficult 1990s that the first femtosecond laser in Kazan was assembled and mastered at the Zavoisky Institute by V.S. Lobkov. Of course, this gave a new impetus to the work of KPhTI scientists in the field of nonlinear and coherent optics.

2.4.3 My Scientific Work During This Period

During the first decade of my work in Kazan, my scientific work was closely related to my collaboration with colleagues from the Free University in Berlin. They introduced me to an interesting area of EPR research into the primary process of photosynthesis—charge separation in the reaction center. In those years, the instrumental base of KPhTI did not allow me to test my theoretical predictions in Kazan. Therefore, cooperation with colleagues at the Free University of Berlin was very important for me.

I spent a lot of time creating a medical low-field magnetic resonance imaging machine. Together with R.T. Galeev, I took part in the development of the program for calculating the magnet.

Together with M.P. Tseitlin and K.L. Aminov we created the first algorithm for EPR tomography of conducting objects.

Together with O.I. Gnezdilov and A.E. Mambetov, for the first time in Kazan, we began to study the photoinduced hyperpolarization of electron spins in solutions of porphyrins. Now research in this area has been very well developed at the Zavoisky Institute in the works of V.K. Voronkova, A.A. Sukhanov and Yu.E. Kandrashkin.

In 1994, Peter Shore published an algorithm for quantum computing. This sparked an explosion of interest in quantum computers. It also interested me. The electron spins appear to be ideal qubits. In addition, the methods of pulse EPR spectroscopy make it possible to monitor and control the states of electron spins.

I proposed an original teleportation protocol in the electron spin system. A possible variant of the implementation of this protocol for teleportation through the membrane of the state of electron spins in modified reaction centers of photosystems was proposed in our work with D. Stehlik and J.N. Golbeck. Subsequently, this protocol was experimentally implemented by M. Wasielewski and his colleagues.

In our work with M.Yu. Volkov, we found a protocol for the implementation of the logical operation controlled-not (CNOT) with two paramagnetic centers with 1/2 spin. We are aggressively looking for organic radicals or paramagnetic complexes that could be used as qubits. These studies at the Zavoisky Institute are conducted by V.F. Tarasov and R.B. Zaripov. In this search, we fruitfully cooperate with V. Kataev and B. Büchner.

3 My Colleagues are My Community

I see my colleagues in scientific work as another big creative family. These are, first, my colleagues at the Voevodsky ICKC of the SB RAS and the Zavoisky KPhTI KazSC RAS. I am immensely grateful to all of them for the fruitful cooperation.

In addition, of course, I was greatly influenced by the people with whom I had the opportunity to meet, discuss scientific problems, and carry out joint research. I regard all these as further learning opportunities.

I gratefully think of the many situations in which colleagues from across the magnetic resonance community have shown their support. For example, when I conceived the idea of creating an international journal that we know today as 'Applied Magnetic Resonance' (AMR), I sent letters to many scientists. All were in full support of such an initiative. I remember calling Richard Ernst; it turned out that he was visiting the United States at that moment. At first, he met the idea without enthusiasm, but after listening to my ideas, he approved the idea, and moreover, he agreed to join the editorial board. In addition, he was a very active member of the editorial board. When I now look at the composition of the first editorial board, I see that each of the board members could serve as distinguished and honored members for any scientific journal in the field of magnetic resonance. Therefore, it is not surprising that from the very beginning the AMR journal took a worthy place among journals on spectroscopy.

I have already mentioned how Karl Hausser decisively influenced the decision of the Ampere Committee to hold the Ampere Congress in Kazan in 1994.

Ulrich Haeberlen has done a lot for the AMR magazine. At that time, he was the only member of the editorial board to whom I delegated the authority to make the final decision on the manuscripts sent to the journal.

We were friends with many colleagues and are friends of families. I know that in several families in different countries they make the "Zoya" cake with ground walnuts. In addition, they have Zoya's paintings. Our children and grandchildren also know my colleagues well.

I have the brightest, unforgettable impressions about trips to different laboratories and meetings with colleagues, about cooperation. In addition, we became friends, and even family friends.

I am writing for a magazine dedicated to Klaus Möbius and myself. Therefore, in this particular case, I decided to highlight my long-term cooperation with colleagues from the Free University of Berlin, in general.

During a conference in Leiden, Dietmar Stelik told me about his experimental work on the optical polarization of nuclear spins in organic molecular crystals. For interpretation, he used the existing theory. I suggested a different explanation. Dietmar's student Gerd Buntkowsky soon received experimental evidence in favor of my approach. This was the beginning of my many years of very fruitful cooperation with my German colleagues. Together with Dietmar, Gerd, and Hans Martin Vieth and Sergei Dvinskikh, we published a paper on the optical polarization of nuclei.

Of course, cooperation and friendship with Dietmar and Klaus occupies a very special place.

We have fraternized with Klaus for a long time. He was born several months earlier than me, and therefore, he is like an older brother. I was brought up in a peasant family and was firmly taught to respect my elders, so I consult with him when I have to make important decisions. I remember when I spent 9 months in Berlin with my whole family when, based on the advice of Dietmar and Klaus, the international committee chose me as a Fellow of the Wissenschaftskolleg (the Institute for Advanced Study) in the 1992/1993 academic year. This is a very prestigious institution. About 35–40 fellows are elected there annually, in most cases scientists, but also writers, composers, filmmakers. The most favorable conditions are created for work, including, interaction with scientists from Berlin universities and others is strongly encouraged. We could do whatever we wanted. Moreover, fellows could invite other scientists to help them. Under such a quota, I invited Yuri Molin for 2 months and my graduate student Yuri Kandrashkin. Yuri Molin and I performed calculations and published two papers on spin dynamics. In addition, during this time, I wrote the book The Magnetic Isotope Effect in Radical Reactions.

Each fellow had the duty to deliver one public lecture. These lectures took place at the Institute. Scientists and others were invited to these lectures, primarily from the universities of Berlin. The lecture was to be available to the widest possible public. I was a little nervous about this lecture. Finally, I decided to opt for what I knew well and what I thought might be of general interest.

I decided to speculate on how, under certain conditions, very small interactions (perturbations) can cause very tangible effects, qualitatively changing the situation. We know that there are many examples of such effects in quantum mechanics. I would like to note a few examples briefly and tell in more detail about the manifestation of a very weak hyperfine interaction of electrons with magnetic nuclei in chemical reactions, about the possibility of creating technologies to control the course of chemical reactions using these very weak interactions.

I was missing a good title for the lecture. At that time, I did not yet know about the existence of the concept of the ‘butterfly effect’. If I had known it, I would have probably used it in the title. I had to preserve the concepts of weak and strong interaction in the title. But I did not want to do this, since the director of the Institute for Nuclear Research in Paris and the director of a similar institute in Heidelberg were among the fellows and in their science, the concepts of weak and strong interaction have a certain meaning, and this is not what I had in mind. In Berlin, near the city center, there is the huge Grunewald forest and while we were driving through there, I shared my torment with Klaus. We discussed it for a long time, until Klaus proposed: ‘Minor interactions with major consequences in chemical reactions’. Since then, about 30 years have passed, and I am still completely delighted with this title. It is like a song to me. This gave me confidence. In addition, on this high note, as many colleagues noted, I very successfully delivered this public lecture.

All my life in science is connected with my four favorite cities.

- Kazan. I graduated from Kazan University (1954–1959). From 1988 to the present, I have been working at the Kazan Physical-Technical institute named after E.K. Zavoisky of the Russian Academy of Sciences. There I defended my Doctor of science thesis (similar to Habilitation in Germany; 1973).
- Leningrad, now St. Petersburg. I was a graduate student of the IHMC of the USSR Academy of Sciences (1959–1962). This is where I defended my Ph.D. thesis (1963). There I also met Zoya (1960).
- Novosibirsk, Akademgorodok. Between 1963 and 1988, I worked at the ICKC SB RAS, and my children grew up there.
- Berlin. In total, over the last 30 years, I have lived and worked in Berlin at the Free University for about 2 years.

Klaus and Dietmar introduced me to a wonderful field of science that studies the primary photochemical stage of photosynthesis using EPR spectroscopy techniques. Collaboration in this area of science has led to interesting results.

My first work on double electron-nuclear resonance was done with Klaus.

In the last 5 years, I have been participating in the study of the protective role of trehalose in the functioning of the reaction center of photosynthetic systems under stressful external conditions for plants (unfavorable temperature, low humidity).

Klaus and Alexei Viktorovich Semenov (Moscow University) introduced me to this fascinating problem.

I proposed a molecular model for the cryoprotective effect of trehalose. It is based on the assumption that trehalose envelops the reaction center of photosystem I and forms a certain protective layer on the surface of the reaction center, in which a network of hydrogen bonds is formed. The hydrogen bond network can be rearranged. As a result, the entropic contribution to the elasticity of the protective layer may change. When heated, the reaction center tends to expand. But in accordance with the Le Chatelier–Brown principle, the network of hydrogen bonds can be rearranged in such a way as to increase the elasticity of the protective layer and prevent the expansion of the reaction center. A number of EPR experiments with reaction centers in trehalose glasses gave results that do not reject this model. We are continuing this research.

Along the way, we have obtained a good result for the development of EPR spectroscopy. The separated charges in the reaction center represent a spin-correlated pair of radical ions. Theoretically, we have long predicted that the dipole–dipole interaction of spins in pairs leads to modulation of the decay of the spin echo signal, and the modulation frequency is equal to the energy of the dipole–dipole interaction. We have shown that random spin flips during spin–lattice relaxation lead to a red shift of the dipole–dipole modulation frequency of the spin echo signal. Several attempts to measure this red shift have failed. For a pair of separated charges in a reaction center in a frozen trehalose matrix, together with Andrey Sukhanov (KPhTI RAS, Kazan) we were able to register this red frequency shift. Note that this shift is analogous to the red shift of the vibration frequency of a harmonic oscillator due to friction.

Of course, at the Free University, I became friends with all the staff in Klaus and Dietmar's groups.

I am a big tea drinker, so just by getting together for a cup of tea or coffee with members of both Dietmar's and Klaus's groups, these were the moments which enabled us to find possible points of scientific cooperation.

Reinhard Furrer worked in Dietmar's group. During one of my visits, I occupied his office. Reinhard himself at that moment was preparing to become, I believe, one of the first astronauts in Germany. He came to the university, gave me a photograph of himself in the uniform of an astronaut. He said that he would conduct a number of scientific experiments during the flight. One of the alleged experiments was the study of candle burning in zero gravity. It is exciting to know that a Russian doll I gifted him flew into space with him.

Later, one day he sent a letter informing me that they flew in a private plane in Alaska near the Russian border. He remembered me, and from there he sent a message. If I remember correctly, he was flying with Marion Thurnauer and Alex Trifunac (US Argonne Laboratory). I think Reinhard taught Alex to fly, and that was Alex and Marion's plane. I once had a chance to fly with Alex too, but not over Alaska, but over Chicago.

While I have not dealt with picosecond and femtosecond spectroscopy before, I had the opportunity to learn a lot of interesting things in this area from Christian Borczykowski. It was he who gave me a paper to read which showed that photoinduced isomerization of rhodopsin is the fastest monomolecular reaction. Later, after becoming the director of KPhTI RAS, I made every effort to ensure that femtosecond laser spectroscopy appeared at the institute.

In Dietmar;s group, I collaborated a lot and fruitfully with Art van der East, Stefan Zech and Julia Pushkar on the program for studying the primary stage of photosynthesis. I think we managed to obtain a number of interesting results in the study of separated charges of reaction centers of photosynthetic systems. I am pleased to indicate that Stefan was a student of Wolfgang Lubitz.

I have realized a number of joint projects with colleagues from Klaus' group. With Martin Plato and my graduate student Sergei Mikhailov, we published the fundamental theory of the so-called stimulated polarization of nuclei. I also had an interesting collaboration on spin chemistry with Jenny Schlüpmann. Zoya and I became very good friends with Jenny.

Martina Huber, who is now a professor at Leiden University, impressed me immensely with her meticulous presentation of her doctoral dissertation. She had redrawn the figures many times, tried different papers. In addition, she was happy when she achieved a very high-quality drawing.

In Klaus's group, I also met Wolfgang Lubitz and Thomas Prisner. Over the years, this turned into friendship and multifaceted cooperation, including their active participation in the development of the AMR magazine. The fact that Wolfgang is the Editor of this special issue of the journal is further confirmation of our cooperation and friendship.

I am also pleased to note the meetings and discussions with Robert Bittl, first at the Technical University, and then at the Free University. He co-authored an article in which the quantum oscillations of the amplitude of the time-resolved EPR spectrum of separated charges in the reaction center of photosystems theoretically predicted by me were experimentally confirmed.

There were unforgettable trips to the USA and countless meetings with colleagues.

I have the warmest memories of meetings with Williams Mims, James Hyde, George Feher, Sam Weissmann, Gerhard Closs, James Norris, Alex Trifunac, Marion Thurnauer, and Al Garroway.

On Lake Michigan, I first learned what sailing on a yacht with Gerhard Closs and Alex Trifunac in stormy weather looks like.

Erwin Hahn showed me his mechanical model, with the help of which he told his students about the phenomenon of spin echo, which he had discovered.

With the family of Alex Pines, we drove along the Pacific coast in a car and sang the song Moscow Nights, his family sang in Hebrew, and I in Russian. I remember that once Alex sent me a large pile of reprints of his articles with the note: "Kev, I am sending you my recent works so that you don't think that I'm getting my salary in vain". This was his reaction to the fact that I drew attention in my publication to the erroneous statement about the effect of a magnetic field on chemical reactions.

At Jack Freed's invitation, I worked in his laboratory at Cornell University for about a month and a half. There were many interesting things. Every day, Jack found time to briefly talk once or twice with everyone in the laboratory, wondering how things were going. Jack and Rene took Zoya and myself on wonderful trips to theaters, including the Broadway Theatre in New York. At Jack's suggestion, I performed theoretical calculations of spin echo signals for selective spin excitation by microwave pulses.

I have crossed paths with Larry Kevan many times in the USA and Novosibirsk. The most vivid impressions I got were from a trip on his yacht in Detroit. I also met his student, Michael Bowman. Since then we continue to be friends with Mike, discussing scientific problems. He has been to Kazan more than once.

I recall with pleasure my meetings in the USA and Germany with Klaus Schulten. When he was in Germany, I calculated the shape of the Moessbauer spectra at their home on his home computer with a tiny printer.

Sometimes there are unexpected situations when I am asked to speak, but I am not prepared, so based on the circumstances and mood, I might sing a suitable song in Tatar, and people listen and feel the song. Recently, on my 85th birthday, Sandra and Gareth Eaton reminded me of my singing at a banquet in Denver for my EPR gold medal award. I love to sing and it is great that those songs act as sort of an anchor at events for people enough that they remember this for decades after it happened.

An important event in my life was to become acquainted with Japan and Japanese colleagues.

H. Hayashi invited me to RIKEN for the academic year (1996–1997). I still regret that I stayed there for only two and a half months because of my duties in Kazan. I was so excited to be there that in the morning, the first thing I did was go up to the top floor of RIKEN and check if Mount Fujiyama was visible.

During this short time, I was able to make one theoretical prediction regarding the optically detected EPR spectrum of separated charges in the reaction center of photosystems: a two-quantum transition line appears in the optically detected EPR spectrum under conditions when the spin–spin interaction can be neglected at the time the spectrum is observed. I explained this by the fact that in this system, the spins interacted in the past and as a result the considered pair of spins was born in a quantum coherent state.

Several times, I had the pleasure of participating in conferences and meeting with Seigo Yamauchi, Takeji Takui, Hitoshi Ohta and many other Japanese colleagues both in Japan and in Kazan.

At the turn of the millennium, I received an award from the Magnetic Resonance Society of Australia and New Zealand: I was invited to lecture at several universities in both countries. I was asked to name the topics of my lectures so that universities could make a choice. I named five or six lectures on pulse EPR spectroscopy, on the EPR of separated charges in the reaction center of photosynthetic systems. There was one more topic I needed to add, and so I added a lecture on quantum computing. All universities chose this last topic. It turned out that in Australia, there was an incredibly great interest in this problem. Of course, I knew the work of B. Kane, but did not notice that he proposed the approach to the implementation of a quantum computer, while working in the capital of Australia.

Zoya and I visited several cities, were guests of John R. Pilbrow, Graeme R. Hanson (Australia), and Paul Callaghan (New Zealand). It was like traveling around the world.

Marina Brustolon and Carlo Corvaja invited me to give a short intensive lecture course on spin dynamics at the University of Padua (Italy). It was unforgettable.

I hope that the times will soon come when it will be possible to visit friends in different countries again, to enjoy communication and scientific creativity.

With joy and gratitude, I think of all the colleagues I have met at conferences in different cities around the world. I am especially pleased to remember those who came to Kazan. Almost all winners of the Zavoisky award were in Kazan. Wonderful!

The further I write about my colleagues, the more it becomes clear to me that I have undertaken a difficult task to write about all the colleagues with whom I had a chance to meet and discuss scientific problems.

Once again I am convinced that “the immensity cannot be embraced”.

4 About the New Paradigm of Spin Exchange

I am writing this autobiography at an interesting time in my life: 2 years ago I formulated a new paradigm in one particular scientific discipline: spin exchange. This work has helped me to see first-hand that paradigm formulation is a very practical tool for increasing the effectiveness of scientific research.

Here, I would like to emphasize that the creation of a new paradigm has become possible as a result of my on-going conversations with my daughter Assia, who specializes in Structured Communication. As the founder of a marketing agency, she is attuned to the idea of creating clearer communication and is familiar with different communication approaches.

At first, there were random conversations as she keeps in touch with me on a regular basis. From time to time, I was sharing my progress in writing a book, until 1 day we had a conversation where she inspired me to learn more about the idea of the paradigm and what it means for scientific research. Even though it was a rather foreign concept for me at the time, I learned from experience that Assia often helps to bring more clarity even without necessarily understanding the subject. Even though she graduated from Novosibirsk State University’s physics department, she is far from science in her everyday life today.

It took me a very short time to read a fundamental book about paradigms in modern society to recognize that the volume and quality of my research has indeed formed the new paradigm of spin exchange. It took some time to process the idea of a new paradigm, because I did not want to get too far in just philosophical aspects of scientific research.

Although I have to admit in this interesting turn of events, I realized all science came from philosophy a long time ago and events that happened after the book was published proved that as physicists we might need to consider the idea of scientific development as a whole.

Our daily conversations were not only useful but also very pleasant both personally and professionally. Soon they led me to understand many interesting things about how the human brain processes new information, how we all learn and what communication is all about. I would have never expected to learn so many things seemingly unrelated to my scientific research, yet beneficial when I decided on the task of sharing my new paradigm with the world.

An elementary ‘reaction’ of a change in the state of spins in collisions of two paramagnetic particles due to the Heisenberg exchange interaction is called spin exchange. Measuring the rate of this process by EPR spectroscopy methods is widely used in molecular biology.

I have already mentioned how I came to this area of science and about our book, written jointly with K.I. Zamaraev and Yu.N. Molin, first in Russian (Nauka, 1977), and later in English in a slightly abbreviated version (Springer, 1980). In subsequent years, I occasionally turned to the theory of spin exchange. In 2019, Springer Publishing House proposed to release a new edition of the book on spin exchange. We discussed this problem with Yu.N. Molin and in the end, he suggested that I write a book on the theory of spin exchange.

I started writing a new book. I raised all my theoretical works in this area and assessed the situation with a fresh look. Over the years, I received a number of theoretical results that did not fit into the accepted, old existing paradigm of spin exchange but they have not been confirmed by experiments. However, in recent years, my theoretical predictions have been confirmed by careful experiments carried out in the USA by B. Bales and his colleagues and at KPhTI RAS. After reviewing everything that had been done in spin exchange, and rethinking my theories, I realized that the totality of the new results exceeded the critical mass. It became obvious that there was a need to formulate a new spin exchange paradigm which I did. Therefore, in 2019, the book ‘Fundamentals of spin exchange. Story of a new paradigm shift’ was published.

The fundamental difference between this paradigm and the existing one is as follows:

The existing paradigm proposes to describe the manifestations of spin exchange in EPR on the basis of the assumption that even in the presence of spin exchange, i.e., in the presence of interaction of paramagnetic particles, one can use the independent particle model by introducing some effective magnetic resonance characteristics of individual particles.

The new paradigm is based on the fact that the transfer of coherence to a given particle (spin) from the interaction partners creates collective modes of motion. Collective modes can be viewed as independent quasiparticles. Their excitation creates delocalized spin excitons.

In layman terms, here is how I explain this exciting new paradigm to my great-grandchildren: it is as if before we looked at, for example, planets as separate entities, but now we consider them and their movement patterns as part of an entire solar system, we notice the moon and how this affects the Earth’s tides, how it can be seen as both a full moon and as a sickle at different times, and so on. If we consider this on a molecular level, it is a massive change, where many areas of science could see big changes because of this new paradigm.

The fundamental provisions of the new paradigm are as follows:

1. Spin exchange may be nonequivalent: the ‘butterfly effect’ may appear due to hyperfine interaction.

2. The dipole–dipole interaction also contributes to the rate of transfer of spin coherence, and this contribution has the opposite sign compared to the contribution of the exchange interaction to the rate of transfer of spin coherence to spin from the interaction partner.
3. Transfer of spin coherence creates collective modes of spin motion. The excited states of these modes can be called spin excitons.
4. With a slow transfer of spin coherence, each EPR resonance line has a mixed form of absorption and dispersion.
5. In the case of fast exchange, the microwave field excites only one collective mode of motion of the spins.
6. In the presence of a strong microwave field, quasiparticles are formed, which can be called spin polaritons.
7. A kinetic equation has been proposed for the motion of spin magnetization in dilute paramagnets, taking into account the exchange and dipole–dipole interaction of spins.

Over the past 2 years, I have devoted a lot of attention to identifying new predictions within the framework of the new spin exchange paradigm. I think spin exchange in dilute solutions provides a great example of how random collisions can lead to highly organized types of motion.

Thinking in terms of the paradigm allowed me to come up with interesting and novel experiments, which we are working on now and some results are already published. I was pleasantly surprised to see how productive I became after adopting a new way of thinking.

Now I am convinced that my work with Assia on understanding the significance of a new paradigm led us to developing a new, more efficient approach to conducting scientific research.

I believe this is an important outcome especially in the fast-paced world we live in.

5 My Future Plans

Following the establishment of a new paradigm of spin exchange and identifying that this approach can be applied to other scientific disciplines, I am full of plans for the future.

I would especially like to highlight my increased interest in the development of methodology of scientific research. Working on the creation and formulation of a new paradigm in a specific discipline (spin exchange) convincingly showed me that to increase the efficiency of scientific research, a very practical tool is the use of a consistent methodology for the development of science, including the formulation of a clear scientific paradigm of a particular discipline.

I am also intrigued to understand more about how we as humans manage to solve problems and create solutions. In other words, I would like to understand more about how we think, because it appears to be a fascinating subject that can be scientifically explored with great benefit for regular sciences.

I am actively working with scientific centers in Russia to progress these ideas.

One of the practical steps which I plan to start in 2022 will be to establish a new international online seminar "Unexplained Experimental Observations". The idea of this seminar is to open a forum for scientists (initially in EPR related areas, and then expand it to other areas) to share observations that do not fit expected results. All too often, we discount the abnormal result and miss the opportunity to further understand nature surrounding us. I want to get a chance to invite the scientific community to bring their unexplained experiments for consideration to their peers. Perhaps we will start finding new solutions sooner and get a chance to speed up scientific development.

Now, I am looking for like-minded people to develop and apply the methodology for the development of science in various disciplines. My door is always open to new ideas and connections—you can always find me at kevsalikhov@mail.ru.

For readers' convenience, I present a selected list of my publications dealing with the main topics of my long-term research: spin chemistry [1–22]; pulse EPR spectroscopy [23–38]; spin exchange [39–52]; and diverse publications [53–59].

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Full Member of Russian Academy of Sciences.

10. 12. 2021.

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