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The 100th anniversary of the birth of N E Alekseevskii (Scientific session of the Physical Sciences Division of the Russian Academy of Sciences, 23 May 2012)

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The scientific session of the Physical Sciences Division of the Russian Academy of Sciences (RAS) commemorating the 100th anniversary of the birth of RAS Corresponding Member N E Alekseevskii took place on 23 May 2012 at the conference hall of the Lebedev Physical Institute, RAS.

The following reports were put on the session agenda as posted on the website www.gpad.ac.ru of the RAS Physical Sciences Division:

(1) **Kopaev Yu V** (Lebedev Physical Institute, RAS, Moscow) "About N E Alekseevskii";

(2) **Brandt N B** (Lomonosov Moscow State University, Moscow) "My teacher Nikolai Evgen'evich Alekseevskii";

(3) **Peschansky V G** (B I Verkin Physical-Technical Institute of Low Temperatures of the National Academy of Sciences of Ukraine, Kharkov, Ukraine) "Galvanomagnetic effects in layered conductors";

(4) **Krasnoperov E P** (The National Research Center 'Kurchatov Institute', Moscow) "First steps of technical superconductivity in the USSR";

(5) **Nizhankovsky V I** (International Laboratory of High Magnetic Fields and Low Temperatures, Wroclaw, Poland), **Tsebro V I** (Lebedev Physical Institute, RAS, Moscow) "History of creation and growth of the International Laboratory of High Magnetic Fields and Low Temperatures".

Papers written on the basis of oral presentations 2–5 are printed below.

Nikolai Evgen'evich Alekseevskii
(23.05.1912–23.09.1993)

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The best years of my life

N B Brandt

I demobilized in 1946. For me, the war started in Moscow's environs and ended in the town of Breitenau in Eastern Germany. As a demobilized soldier and as a school-leaver who had graduated from the secondary school with distinction, I was eligible to enroll exam-free in the Department of Physics of the Lomonosov Moscow State University. I do not remember now who advised me at the beginning of my third course to attend a seminar at the Institute for Physical Problems (IFP in *Russ. abbr.*) of the USSR Academy of Sciences. The visit to the Institute left an indelible impression on me. I cannot find words to describe my feelings. It was a mixture of fascination and awe. Perhaps this was what a religious person feels when entering a temple. The style itself and the architecture of the Institute, the atmosphere, the furniture, the massive antique grandfather clocks in the reception, the carpeting on the stairs and in the corridors, the measured and unhurried lifestyle, the way in which people working there treated one another; all this created some special atmosphere, very unusual for me, and in addition to this an aura of mystery and importance of whatever these people were doing in their laboratories. I immediately under-

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stood that this was the only place where I wished to learn and work. This is how I was assigned to the Chair of Low Temperature Physics, headed at the time by A I Shal'nikov. There were four of us: myself, Efim Itskevich, Vera Bravina, and Valya Balashova.

I was first introduced to Nikolai Evgen'evich Alekseevskii (N E or Kolya to those close to him) at the Special Practicum on Low Temperature Physics; several experimental setups of the practicum were mounted in the large hall of the Institute on the ground floor where P L Kapitza's famous machine was built for generating superhigh pulsed magnetic fields. I did not know at the time that all my subsequent scientific career would be connected with this man. It was some time later that A I Shal'nikov assigned me to Nikolai Evgen'evich's laboratory. This happened by the end of the 6th term, shortly before the summer holidays. Nikolai Evgen'evich said that there was no work for me in the lab at the moment, so he gave me V L Ginzburg's book, *Superconductivity*, to read during summer. This book was a gift to Alekseevskii from Ginzburg, with his autograph on the front page: "To dear Kolya from the author. V Ginzburg." I should mention that when I showed the book to my classmates, they were in awe, as everyone assumed that the brilliant scientist presented this book to me [Nikolai ('Kolya') Brandt], a 3rd year student. That summer, some of my friends and I went on vacation to Moldova and stayed on the banks of the Dniester River, in the village of Yaruga, where we earned some money by taking photos of local people; we went fishing, swimming, and sunbathing, and travelled around a bit. Nevertheless, in my free time I carefully read *Superconductivity*, which was a mighty surprise for Nikolai Evgen'evich after completion of the summer holiday. Many years later, Nikolai Evgen'evich continued to recall that I was his only student who had spent the summer holiday studying superconductivity.

In September 1948, I started working at Alekseevskii's lab. The laboratory consisted of two parts. The first one, which Nikolai Evgen'evich devoted to superconductivity, was on the ground floor and opened into the hall of 'strong magnetic fields'. It was a small room of 18–20 square meters, with a single window. The central part was occupied by a Swiss-made electromagnet with changeable pole axial cones and a variable gap which allowed the generation of magnetic fields up to about 20–25 kOe. At the time, this unit was a masterpiece of laboratory equipment.

The second part of the laboratory was in the basement. Here, Nikolai Evgen'evich was developing an original design of a mass-spectrometer. An interesting idea was chosen to increase the sensitivity of the spectrometer: charged particles moved in an inhomogeneous magnetic field so that the deflection of a particle was determined not only by the particle mass but also by the magnetic field strength, which was different for the trajectories of heavier and lighter particles; this further increased the resolving power of the device. This work was classified; the basement was specially guarded, and I had no chance to visit that laboratory. The phone line was the only connection between the 'top' and the 'bottom', and whenever I called the bottom, at any time of the day or night, I always heard the noise of the working backing vacuum pump.

Nikolai Evgen'evich assigned me working space to the left of the entrance door. There were only three tables in the 'top' room: one belonged to Nikolai Evgen'evich, the second to his laboratory assistant, and the third, to the left by the wall, fell to my lot.

It should be mentioned that the conditions for studies at the IFP were marvelous, even if somewhat unusual. First, the rule was that everyone made practically everything for his work with his own hands: small-scale glass-blowing, mechanical, radio, and assembly work, repairs and adjustment of instruments, etc. No doubt, some large-scale work, like blowing Dewar vessels, other types of complicated glass-work, as well as parts requiring high-precision mechanical processing, were carried out in the glass blowing and mechanical shops of the Institute. Second, every employee and every student working on a graduation thesis had open access to the warehouse, where one could simply walk in and take any required piece of material, electronic element for a circuit, etc.

On my first day in the laboratory, N E told me: "Try to get used to the lab first; once you stop breaking everything around here, I will tell you what I wish you to do," after which he made me the subordinate of his personal lab technician, Vasya Zertsalov, for doing ancillary jobs. In my utter naiveté, I interpreted Nikolai Evgen'evich's words as a sort of indulgence, absolving me from any material damage I might cause in the laboratory during acclimatization. Therefore, when I smashed a dewar after two or three days of work, I suffered no pangs of conscience and felt quite serene. There was even some feeling of satisfaction that my supervisor's foresight proved right. On the next day, N E grumbled: "Hand me the dewar!" I calmly informed him that I had broken it. My, how he exploded! N E yelled for at least ten minutes. This made such a strong impression on me that I never again broke a dewar at the laboratory.

Working with Alekseevskii was fantastically exciting. The time spent in his lab at the IFP was undoubtedly the happiest time of my life. To say that Nikolai Evgen'evich loved science would be equivalent to keeping mum. Science for Alekseevskii was the main objective in life. It was sacred. It was a religion that he worshiped. Nothing ever existed that would be more important. Every priority belonged to science and only to science. Even the tiniest betrayal of science would be unforgivable. This was THE THING and one shared it with nothing. God forbid if during working hours you made something for yourself or for your home use. This was considered a sacrilege, a crime.

Interestingly enough, outside the lab Nikolai Evgen'evich was very witty, cheerful, and an interesting conversationalist, feeling at home among contemporary literature and poetry, of which he was especially fond. However, once he crossed the lab's threshold, he transformed into a different person. Things outside the lab ceased to exist. N E exuded some inner tension and concentration, he was full of new plans and ideas. He was possessed of a passionate desire to implement them as quickly as humanly possible, no matter what the cost. In my life, I have come across many talented physicists for whom painstaking polishing of the method and of the experimental setup constituted no less important a part, and maybe even a more important one, of their life in research. The elegance of the elaborated methodological and constructive solutions rewarded them with true aesthetic pleasure. Alekseevskii was different. His indomitable energy, temperament, and intensity of effort rejected the slow evolutionary approach to scientific endeavor. He worked for a result. He needed this result. The result at any cost. If the glass wall of the vacuum system cracked during an experiment, he would try to stop the leak without interrupting the work; if helium ended, he would add more; if an instrument died on him, he would replace it with another, but would fight to complete the experiment then and there. Each working day meant too much for him to allow it to go to waste.

But this did not mean that N E would accept any result. He strove only for the correct, absolutely reliable, and reproducible results. Alekseevskii was always very liberal with the time needed for reproduction of experimental data, for variation of experimental conditions, for checking and rechecking results. What he could not stomach was sloppiness of any type, carelessness in measurements or analysis of the data obtained, or any doctoring of data. He was organically against baseless projects, any profanation in science, any half-baked or inadequately argued conclusions. Here he was merciless, especially when dealing with his pupils. At some point I was excited about the idea of realizing superconductivity in nonequilibrium conditions, as it seemed that the constraint on the strength of the electron–phonon interaction due to the stability of the crystal lattice is automatically removed. To be honest, this idea was immediately rejected by V L Ginzburg. His arguments failed to totally convince me, and I talked about it to Nikolai Evgen'evich. Some time later, when I thought up a modified version of the original arguments, N E met me at one of the low-temperature meetings and asked, "How about it, Kolya, do you continue messing up science?" and, in all honesty, I was somewhat insulted by this quip; however, N E was right, even though I understood it much later. I think a certain deliberate rudeness in Nikolai Evgen'evich's jokes, so typical of him, was merely a superficial mannerism, perhaps even a sort of camouflage that protected his kind and easily vulnerable soul.

Nikolai Evgen'evich had one spectacular trait: in the evenings, he was simply unable to leave for home if there was still anyone working in the lab. I should mention here that in the post-war years all of us were working very much in the Institute and our working hours stretched to considerably more than eight hours. At that period, N E focused mostly on his work in the 'basement' laboratory and would typically surface on the ground floor in the evening, about 9 or 10 pm; he would look grim and dissatisfied and ask with irritation: "Where has this son of a bitch Boris disappeared to?" Boris Samoilov was at the time Alekseevskii's postgraduate student, even though he worked in P G Strelkov's laboratory. I would reply: "Look, Nikolai Evgen'evich, it's quite late, in fact 10 pm, and he has a long way to go to reach home. Do you need help? I will be happy to help you."

I really enjoyed working in the evenings. It was too hectic during the day. The lab was tiny and crowded. People came and went. They were a nuisance. But in the evening, when most people would leave for home, the atmosphere was very quiet. At that age, I refused to accept that one may get tired working. Interesting work meant joy and happiness. Because the number of various other interesting objectives was large, I cherished every minute spent in Nikolai Evgen'evich's laboratory. To recapitulate: during the entire time of our joint work (almost 2.5 years at IFP), Nikolai Evgen'evich never left the laboratory before me. There were days when he looked very tired and by about 11 pm began to look at the clock. I always offered to finish the work for him (i.e., prepare the setup for the next day of work, mount a specimen, assemble the cryostat, turn on, adjust, and test the radio electronic circuits, etc.), but not even once would he agree to leave first. He would get up and, having muttered: "Will return with some grub," he would walk out and return a quarter of an hour later with sandwiches; we would have a bite and continue working. Sometimes Nikolai Evgen'evich invited me to share supper with him. He lived quite close to the Institute, in a building for members of staff, with two-story apartments. N E's apartment was on the ground floor; another room, part of a contiguous apartment, was merged with his much later, after he got married. At the time, N E lived there with his mother, a very sweet and helpful lady. We would leave the lab together. New students started working in the laboratory later — Tanya Kostina and Volodya Lipaev. But the work continued to be organized as before. Everyone worked late into the night. This meant more hassle for Nikolai Evgen'evich. The laboratory now had a lady researcher. To allow her to get home alone was somehow unacceptable. However, Nikolai Evgen'evich owned a tiny car, the very first model of the Moskvich. Now, Nikolai Evgen'evich had to deliver his entire team to their home addresses. At the time I lived in the Arbat area and Tanya lived further on the Garden Ring Road, near the American Embassy. Sometimes he had to taxi home four people, three of them in the back seat — a feat that was virtually impossible for a standard model Moskvich. One of us had to half-stretch on the laps of those in the back seat, pressing head and feet into the window glass of the rear doors. I tried to be funny once, saying how wonderful it is that the Central Party Committee and the Council of Ministers published a decree on improving the quality of industrial production: indeed, window glass now withstands enormous pressure. To my great surprise, Nikolai Evgen'evich, who loved a good joke and enjoyed witty ones, remained silent and poker-faced. I understood only later that joking at the expense of Communist Party decisions was still dangerous at the time, and the wise Nikolai Evgen'evich gave us to understand that we had better leave this topic alone. Jokes about everyday matters and matrimonial relations were a different matter, and here Nikolai Evgen'evich knew thousands of them; this happens very infrequently, but they were all really witty and clean. Nikolai Evgen'evich possessed good taste and knew where to stop.

The laboratory had two setups for ballistic measurements of the magnetic moment of superconductors: a specimen was moved by a rod whose upper end went through a gasket at the top of a dewar from one coil into another, and the system recorded the displacement of the light spot on the scale of the ballistic galvanometer. In measurements with superconducting alloys, we observed the so-called time-effect — the kick of a galvanometer needle gradually decreased with time, and one needed to wait until the equilibrium position was reached. It was a long process: sometimes the equilibrium position was reached only after an hour of waiting. Well, once I noticed during such measurements that if I shifted the rubber tube through which helium was pumped out of the cryostat, the galvanometer needle kicked! For a long time no one could find an explanation. Then I realized that the rubber tube was electrified and shifting it generated a pulse of electric current. Nikolai Evgen'evich christened this effect the 'rectum effect' and milked this joke for a long time when entering the laboratory room.

Nikolai Evgen'evich had numerous virtues, but he also had a bad shortcoming: he did not like to write, even scientific papers, let alone all those reviews of theses, expert evaluations, etc. He therefore pushed them all onto me, with the words: "The company gets the honors, you get the money." In those hungry post-war years the money was very important for me. When Nikolai Evgen'evich brought a thesis for reviewing, he usually passed it to me with a kind of label: "Rubbish", "So-so", or "Look at it yourself." This was enough for me. But to the competitor for a degree (PhD) he would say: "If you need to discuss anything, contact only Nikolai Borisovich." Note that these competitors never saw me, had no idea who this Nikolai Borisovich was, but were already full of respect for and in awe of the mere sound of my name. In the same vein, I was in the dark as to how this reputation was created until the following happened. Competitors would typically come to collect a review or to clarify an unclear point at the end of the official working day. They were instructed to phone Alekseevskii's laboratory from the entrance hall of the Institute, but if no-one was there at the moment, then to phone P G Strelkov's lab where Nikolai Evgen'evich's postgraduate Boris Samoilov worked. One such evening I was informed that someone was waiting for me in the entrance hall. I went there and found a very subdued competitor who mumbled that he was so sorry for having interrupted my evening meal. I could not understand what he was talking about. Later it became clear that the man called P G Strelkov's laboratory and asked for me but Boris, a very witty man who loved wordplay and practical jokes, said something like: "They are having their supper. Once they finish the meal, they will emerge and talk to you."

One of the wonderful traits of Nikolai Evgen'evich as a person was the simplicity and natural manner of talking to any staff member or postgraduate or student. He would never pull his official rank with people, and even to students he talked as equal-to-equal, whatever was being discussed: science or an everyday event. Talking science with Nikolai Evgen'evich was always excitingly interesting. First, he was always sincerely happy learning of a successful experiment or a new result. Second, Nikolai Evgen'evich would immediately start generating new ideas and we would discuss together how they could be implemented in the simplest and fastest way. Third, he possessed exceptional intuition, some internal feeling, that allowed him to evaluate the reliability of the reported result and the confidence level it deserved.

My first research task was to develop and build a system for measuring magnetic susceptibility by radio electronic method. A specimen would be placed in a measuring coil, while a second (outer) coil created an oscillating magnetic field of small amplitude. As the magnetic susceptibility of the specimen was varied, the electromotive force generated by an alternating magnetic field in the measuring coil also varied. This signal was compensated for in the initial conditions, and when the magnetic susceptibility of the sample changed, the system recorded the build-up of the signal. This method could be applied to detecting the transition of the specimen into a superconducting state even when the specimen and the pick-up coils were spatially separated. It was, thus, possible to monitor the superconducting transition in a specimen placed in a high-pressure chamber. I needed to mount a narrow-band low-frequency generator, a narrow-band amplifier working at exactly the same frequency, and a system of compensating for the original signal. All this had to be screened from external electromagnetic noise. It took me something like two months to build this system. The day of testing at last arrived. In fact, superconducting transitions were measured before that only by ballistic techniques. Nikolai Evgen'evich walked in. I compensated for the original signal. The pumping out of helium vapor from the dewar started, and the temperature began to decrease; alas, the needle of the millivoltmeter started to move at the same time. I was crushed. The specimen in the dewar had the superconducting transition point at 3.73 K, so the signal should have started changing only when this temperature was reached. In silence, Nikolai Evgen'evich disconnected my measuring system, connected the measuring coil directly to the ballistic galvanometer, and restarted pumping. At exactly 3.73 K, the light spot on the galvanometer scale abruptly moved right. Nikolai Evgen'evich said: "This is science now." And walked out. For Alekseevskii, the most important characteristic of a technique was its reliability, the maximum confidence level in the result obtained. Perhaps even more was the 'gut feeling' of the experimentalist underlying this confidence, the ability to obtain direct, not indirect, information on the properties of the material under investigation.

It took me a great deal of time after this to refine the variable-current technique to this degree of perfection. I again asked Nikolai Evgen'evich to supervise the test. Once he concluded that the method not only detected the emergence of a superconducting phase but also measured the variation in the volume of this phase as temperature decreased, i.e., the degree of homogeneity of the specimen, and that the results of these measurements were perfectly reproduced in subsequent experiments, the method was approved and added to the armory of experimental methods of the laboratory, before it spread to other labs. I deserved a few words of praise, but not more. I understand now that Nikolai Evgen'evich was always afraid of over-praising a younger colleague; he seemed to fear that praise may spoil a researcher. However, he was genuinely happy learning of any new result, so working with him was especially interesting.

Recalling now the time of our joint work, I suddenly realize that Nikolai Evgen'evich never treated me as a 'student', or later as a 'postgraduate'. I was his colleague. Discussions, debates, controversies always unfolded 'among equals'. Only well-reasoned arguments were taken into account. 'Force' achieved nothing. Perhaps, infrequent pressure of the mildest variety: "If I were you, I would do this and that... ." Or: "If you decided to take such-and-such topic for research, you would complete your DSc thesis in five years." Plus total protection from so-called meticulous tutelage. I even think that in N E's case, it was excessively total refusal to exert influence. Like it or not, one owes it to a junior at least to disadvise doing certain things or, at least, to explain why one should not do it in a particular way. Or, even if a supervisor concludes that a particular approach to a solution is glaringly nonsensical, he simply needs to ban any attempt at practical implementation. In this sense, Nikolai Evgen'evich perhaps displayed an excessively fine approach and respect to the opinion of colleagues, or faith in their abilities.

It was a tradition in Nikolai Evgen'evich's laboratory to celebrate successes. In these cases, we used more often the beverage made of diluted pure alcohol. The event would be celebrated in a narrow circle of laboratory colleagues behind locked doors (in the evenings, of course) always with the boss absent (this was checked in advance). Sometimes, however, the rule would be broken. In the post-war years, the wine shop on Gorky Street (on the right if you are going upward from Manezhnaya Square) would sometimes sell 50-year-old vintage wines imported from Germany as reparation charges. Tall bottles of dark glass were hermetically sealed with long corks, and the bottlenecks were wax-coated. White

Mosel and red wines. Once I bought two bottles of different brands and hid them in the lab. One of the evenings Nikolai Evgen'evich and myself were discussing a new result when Petr Georgievich Strelkov walked into the laboratory and asked: "Anything interesting today?" Nikolai Evgen'evich responded immediately: "Digging through a pile of dung, Nikolai Borisovich managed to find a pearl." The thing was, at the time I was studying the galvanomagnetic properties of alloys, and they were a very 'dirty' object of study — not single-crystalline, impurities, deviations from stoichiometry, etc. But in that case I was able to grow a good single crystal of an alloy and to observe quantum oscillations of magnetoresistance (known as the Shubnikov–de Haas effect). After this, I suggested doing some wine tasting with a bottle of Mosel, and all three of us went to the apartment of Nikolai Evgen'evich. As I had a chance to mention, Nikolai Evgen'evich lived in a three-room flat: two joined rooms and a bedroom. The larger room was used as a dining room, and the smaller one as his study. We uncorked the bottle in this small room. The wine's flavor was disappointing (I distinctly smelled hydrogen sulfide). Nikolai Evgen'evich said nothing, while Petr Georgievich remarked: "Soft, unusual bouquet." I suspect that none of us were experts in fine wine bouquets in those days, but striving for learning the unfamiliar united us. We were later able to taste other wines, too, always in the same room, and the company always included Petr Georgievich.

As we were students at the Chair of Low Temperature Physics, special lectures were delivered by the invited IFP scientists, thus holding more than one office. Nikolai Evgen'evich lectured on superconductivity, A A Abrikosov on the theory of superconductivity, I M Khalatnikov on thermodynamics. Head of the Chair, A I Shal'nikov, led a spectacular seminar on experimental physics. Nikolai Evgen'evich was very serious about his lectures. He usually brought to a lecture a pile of various physics journals with a large number of bookmark tabs. I know from my own experience that when I prepare a lecture, I am always anxious that there is not enough material for two hours; in fact, the amount that I collect for the lecture is so large that I can at best squeeze in half of it. For me, the indicator of how seriously Nikolai Evgen'evich prepared his lectures was that, from the pile of journals and books he would bring to the lecture, he would never use more than a third. I still have no clue what was there in the material he would not use in these presentations: would he move them to the next lectures or would they be lost forever? This enigma remains unsolved to this day. As Nikolai Evgen'evich knew his material very profoundly, he was afraid of letting through something imprecise, and therefore inserted all sorts of reservations. However, in order not to waste time, he would switch to rapid patter, as if mumbling something to himself, so it was virtually impossible to understand every fine point. If, however, we repeated our questions or asked new ones, he would get nervous and irritated. During the lectures, Nikolai Evgen'evich was greatly concentrated. He treated his talks at conferences and official meetings with the same degree of seriousness. He prepared them very thoroughly, and the number of slides was always much greater than he would demonstrate during the talk. Typically, the entire staff of the laboratory was involved in preparing graphic materials. Everyone had an assignment to draw some graphs, take new photographs, prepare slides and figures for slide projections, and so forth, the whole process being constantly and tightly controlled. Both during a conference and after a session, Nikolai Evgen'evich would ask one of his staff to come to his hotel room in order to make an additional correction. His talk would dominate everything else; he thought about it nonstop, improved the contents, changed details, or expanded something.

A tell-tale trait: if N E praised anyone, he would almost always do it with irony: "How did it go? Another awl into such-and-such's bottom?" He would mean someone working on a similar problem. The highest form of praise for a well-presented paper was: "Listen, you presented it very well." But this was a rare occasion and would accompany a certain type of circumstance, not just the matter of the talk presented but in connection with a discussion of a whole set of topics. My relations with Nikolai Evgen'evich were excellent both when I was working on my diploma project at the IFP and during the postgraduate years, when, owing to factors beyond my control, I had to work not at the IFP, but at P G Strelkov's laboratory at the Institute of Metrology on Shchusev Street, close to the mansion occupied by Lavrentii Beria. For P G Strelkov, his work at the Institute of Metrology was a combined job. His laboratory worked on developing standard thermometers for establishing the thermodynamic temperature scale; the lab was located in an isolated side building and consisted of a small 'anteroom' with a hydrogen liquefier, a large room on the ground floor, and the basement floor. The laboratory leader was Andrei Stanislavovich Borovik-Romanov. He and Margarita Petrovna Orlova occupied the upper room. Dima Astrov, Natasha Kreines, Efim Itskevich, and myself worked in the basement. I will always cherish the warm memories of the laboratory which gave me shelter in very bad years of our life, and the feeling of profound gratitude to everyone who worked there. The wonderful atmosphere of understanding, help, and warmth made it possible for all of us to work successfully, despite all the hurdles. Nikolai Evgen'evich would often visit our basement with P G Strelkov and take part in joint discussions of the results obtained. His moral support was extremely important.

In many ways, Nikolai Evgen'evich did very much for my sake at the time. As a senior student and as a postgraduate, I had to moonlight 'for an extra buck', so to speak: surviving on a single scholarship supporting a mother whose health did not allow her to work was not easy. A solution: I would usually sit for all exams ahead of schedule and find employment for three summer months on a geophysical research expedition. Soon, the directorate of the Institute of Physics of the Earth started to notice my name, and I began to receive invitations to such expeditions in responsible positions. One of them was as deputy manager of the Geophysical Station of the Crimean peninsula. I was hired as a temporary employee, in order for the Institute not to lose this administrative resource and in the future replace me with a permanent member of staff. The plan was to set up this station in Yalta, in a luxurious mansion which at the time housed the Yalta seismic station. Before the 1917 revolution, this building belonged to one of the ladies of the empress. It was a two-story villa with huge crosswise-arranged outdoor balconies on the ground and first floors. A marble staircase led to one of the balconies of the first floor, with grape vines on top and on the sides. In August, large bunches of grapes hung all around. The villa was surrounded by a garden. To organize the job, I had a truck at my disposal, and many special instruments and equipment arrived from Moscow.

I decided to use the favorable situation and suggested to Nikolai Evgen'evich to come and spend his vacation in Yalta, taking part in trips all over the Crimean peninsula. He happily agreed and, after receiving lots of instructions from his mother about the dangers from which I was to guard Nikolai Evgen'evich, I went by train to Yalta, where I was to meet my teacher several days later. He was arriving by plane. I remember very distinctly how Nikolai Evgen'evich, in a gray suit and tie, emerged from the bus in Yalta. He definitely looked tired. His first words were: "Oops, no need for this now," and he got rid of the tie. The geophysical station was located not too far from the bus terminal but, in order to immediately demonstrate our logistics potential, I arrived to meet N E in the truck. The month spent together with N E in the Crimea is stamped indelibly on my memory. We crisscrossed the most cherished locations on the peninsula, went to the cave town of Tepe-Kermen, walked to (then) almost inaccessible places on the coast and in the mountains, climbed to cascades and mountain lakes, sunbathed on the beach, and swam in the sea. On one such day when we were resting on the beach, we were approached by an itinerant artist who offered to draw the likeness of one of us. Nikolai Evgen'evich immediately pointed to me: "OK, draw us his portrait." After a glance at the finished product, Nikolai Evgen'evich, who attentively followed the entire procedure, said: "The pencil drawing was certainly better. You shouldn't have colored it." I have kept this painting ever since.

Nikolai Evgen'evich was very athletic. He loved swimming. Once, somewhere on the shore, we stopped to have lunch in a comfortable cove. On the left a huge rock jutted into the sea. N E suggested swimming around it and having a look at what was hiding behind it. We started together. The sea was somewhat rough, and the waves would not let one go fast. Nikolai Evgen'evich's swimming style resembled the crawl, so most of the time his head was under water and he could not monitor the surrounding scene. To be honest, he was a strong swimmer. I tried to keep up with him, but this was tough. When we reached the tip of the rock, I realized I could not continue to swim at that speed; I was ready to resign myself to defeat. However, at this very moment Nikolai Evgen'evich suddenly stopped, looked back, and saw me. He was very surprised: "Hey, you are a mighty swimmer. I reckoned you wouldn't make half of this distance." We swam on but at a slower pace. Later on, in Moscow, when discussing what sort of swimmers people in our crowd were, Nikolai Evgen'evich would always say: "Oh, Nikolai is very strong on that point." I also remember that after a walk to an ancient cave settlement at the top of a mountain, N E ran down — all three or four kilometers to the car. This is not all. Several times we had long walks from Yalta along narrow canyons. Once we went quite far. The canyon became very narrow and at last we reached a nearly vertical stone wall some 6–7 meters high. To my greatest surprise, Nikolai Evgen'evich, without saying anything, started to climb up the wall. I was really afraid he might fall. I was therefore very happy when, having climbed about four meters, my professor safely climbed down, with these words: "Pity you decided not to climb, the going was very easy."

Nikolai Evgen'evich effortlessly integrated into a new group of people, behaving very simply and treating everyone as his equal. On one of our trips, we stopped to spend the night near a small settlement. We went to bed without taking a supper. We could sleep in the house, but N E preferred sleeping in the car. To keep him company, I decided to do the same. Alas, we had no sleeping bags: just one blanket per person. In the army, I learnt to sleep in any conditions and treated the absence of comfort as something natural. Even though my professor used to live in a comfortable home, I did not hear even a single word of complaint from him that night.

Despite his constant concentration and immersion in science, Nikolai Evgen'evich was able to relax — especially if there was a reason for it. At one of the conferences on low-temperature physics (organized in Kharkov), Tanya Kostina, Slava Ponomarev, Lena Svistova, Yura Pospelov, Maya Kostryukova, and myself decided to mark a successful talk at the session with a nice evening meal at the restaurant. We all loved Nikolai Evgen'evich and, when he also went there for taking a supper, invited him to join us. We all felt wonderful. We ordered two bottles of Ukrainian pepper-saturated vodka. We drank them up with suspicious speed. We ordered another, and it was also emptied in the blink of an eye. A discussion followed: shall we order another? We agreed to resolve the quandary by secret ballot, gave everyone a piece of paper with YES and NO on it, and voted: YES — 6, NO — 0. We ordered the fourth bottle and drank it up again. It was all wonderfully merry. Nikolai Evgen'evich was telling us funny stories and jokes. We decided to run another ballot. The result: YES — 6, NO — 0. Then another ballot. This time the result was not unanimous: YES — 5, NO — 1. I know precisely who voted against it, and it was not Nikolai Evgen'evich. After the restaurant we had a long walk through the sleeping Kharkov. We rode on the merry-go-round at a playground slid down the sliding board, sang aloud. This was the only time I saw N E so perfectly relaxed.

Nikolai Evgen'evich was a man of straightforward manners, absolutely incapable of weaving intrigue or behaving dishonestly. He would alway give his opinion openly, although sometimes this required certain courage. If he disagreed with his opponent, he sometimes preferred to keep silent, but this was rare. But as far as I know, he was invariably honest. When the Donetsk Research Center was being established under the auspices of the local governmental authority, Nikolai Evgen'evich and myself were invited to travel to Donetsk by Alexandre Alexandrovich (A A or Sasha) Galkin, who occupied the position of the research center organizing director. He needed us there as consultants on a number of scientific matters. On the plane, N E warned me: "Seems to me, Sasha is playing games. Be careful, avoid a trap." Once the plane landed, we were met in the gangway by an entire delegation headed by the secretary of the regional Communist Party Committee and his black Volga limousine. Nikolai Evgen'evich smelled something fishy in the air and looked grim. The hosts chauffeured us to the hotel, gave each one of us a luxury suite with a table in one of them already covered with numerous hors d'oeuvres and beverages. When N E and I were left alone for a short before supper, while with A A, Nikolai Evgen'evich gave Galkin a piece of his mind in very sharp terms, saying that serious things "are not done in this way," "what are you brewing here?", "I am not a boy of 5 to treat me in this manner," and so forth. It became clear that the idea was to offer us positions at the Donetsk Research Center, and the moment we agreed to this proposal, we would be elected Full Members of the Ukrainian SSR Academy of Sciences, that apartments and villa plots on the shores of the Sea of Azov for us had already been prepared, and that all of it would be officially shown to us during this visit. We were given no time to reply — the entire team of big guns walked in and sat down to take a supper. Sasha begged us not to take any decision just yet. Note that such a grand scale and such a reception were truly A A's style; he was perhaps the last romantic in physics. Then we surveyed the territory assigned for the research center, the building plan, the apartments for the staff (I doubt that these two were meant for us, but were showpieces on display). Both were on the top floor, and when Nikolai Evgen'evich asked: "Why the top floor?" — Sasha responded with something like "Easier to add a fireplace" (he knew how to seduce). Then there was a trip to the shore of the Sea of Azov. We were given accommodations in villas belonging to the CP regional committee. The next morning, we were invited to take part in fishing for sterlet. We were helped into a fisher's launch. It was windy, approximately wind force 3. However, the helmsman tried all the time to set the launch with the side to the wind, presumably to make us feel the hardships of a sailor's life. Obviously, we were not catching any sterlet on our boat; nets were thrown off other boats, while we kept making turns around them at an angle of about $\pi/2$ to the wind. I get seasick very easily, and soon this disgusting game got the better of me. Nikolai Evgen'evich, although he expressed no joy in sailing, drank the entire cup of poison. After we moored the launch, we learnt that the booty was 26 kilos of sterlet, and the director of the fishing sovkhoz invited us all to breakfast at his house. As we walked up the stairs to a large veranda where a table was laid with a few clay bowls with fresh black caviar, one of the big shots asked the accompanying party instructor in a low voice: "Have the crates been delivered?" It soon became clear that he meant crates of vodka bottles. At the table we sat separately: I sat next to the director of the fishing sovkhoz, while Nikolai Evgen'evich was among bosses of a higher caliber. Vodka was poured into 250 ml glass tumblers. Toast No. 1 was to the health of the visitors from Moscow. We drank a third of our glasses, taken with a tea-spoonful of black caviar. At this point, my neighbor demanded in a thundering voice: "What is it, Borisovich, too proud to drink with working class guys? Is this how we eat after a shot of vodka?" He took a bowl of caviar and ladled half of it (no less than 0.5 kg) onto my plate. The same fate befell Nikolai Evgen'evich. We had to empty the glasses and follow it with a few of spoonfuls of black caviar. Then we had sterlet fish soup, and had it not been for the caviar, we might have soon been killed by the amount of vodka consumed. The feast was not over yet when A A asked Nikolai Evgen'evich what our decision was. The answer was straightforward and unambiguous: "Why have you, Sasha, set it all up? You knew perfectly well that neither Kolya nor myself would leave Moscow." I would not pluck up courage to respond like that. Thank you, Nikolai Evgen'evich. Such a relief. Hospitality is a burden if you cannot respond in kind.

Things that were meaningful for Nikolai Evgen'evich Alekseevskii, he passed on to his disciples; when doubts creep in or when we simply get tired, the memory of him — the Scientist, Teacher, Human Being, Indefatigable Laborer — helps us live and work.

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Galvanomagnetic phenomena in layered conductors

V G Peschansky

It is with great pleasure that I recall the years of active collaboration with Nikolai Evgen'evich Alekseevskii and staff members of his laboratory, when a reliable spectroscopic method was being developed for studying the topological structure of the electron energy spectrum of metals by measuring galvanomagnetic characteristics in strong magnetic fields.

The theoretical aspect of this problem was developed in Kharkov. Analysis of galvanomagnetic phenomena in metals with an open Fermi surface (FS) without using model concepts of the electron energy spectrum demonstrated that the presence of planar sections of a constant-energy surface $\varepsilon(\mathbf{p}) = \text{const}$ passing through many cells of the momentum space significantly affects the dependence of resistance on the intensity of a strong magnetic field.

In contrast to the drift of free electrons, the open-trajectory drift of charge carriers in momentum space deflects from the direction of the magnetic field by an angle depending on the momentum projection of the conduction electron, $p_H = \mathbf{pH}/H$, onto the magnetic field direction. Consequently, the resistance is very sensitive to the magnetic field orientation relative to crystallographic axes; at those orientations under which open trajectories of electrons in momentum space with energy $\varepsilon(\mathbf{p})$, equal to the Fermi energy ε_F, appear or disappear, peculiarities arise in the angular dependence of magnetoresistance [1].

These peculiarities have been classified, and in paper [1] the inverse problem of studying the topology of the FS, $\varepsilon(\mathbf{p}) = \varepsilon_F$, was essentially formulated; this is the main feature of the electron energy spectrum of degenerate conductors obtained by measuring the anisotropy of the magnetoresistance in a sufficiently strong magnetic field, when the radius of curvature of the trajectory of charge carriers is much less than their mean free path. Using a model of an open FS shaped into a corrugated cylinder, it was shown that, even in a very strong magnetic field, a complete revolution of the conduction electron along a very elongated closed orbit without collision may prove impossible.

As a result, the magnetoresistance as a function of the angle between the magnetic field vector and one of the crystallographic axes has a sharp peak at angles θ close to $\pi/2$; the peak width is inversely proportional to the magnetic field induction.

At $\theta = \pi/2$, the fan of all possible directions of electron drift fills the entire plane xy, and the resistance to the current flowing along the z-axis increases quadratically with increasing the strong magnetic field. If the magnetic field vector deflects from the y-axis by a small angle $\varDelta$ which equals the

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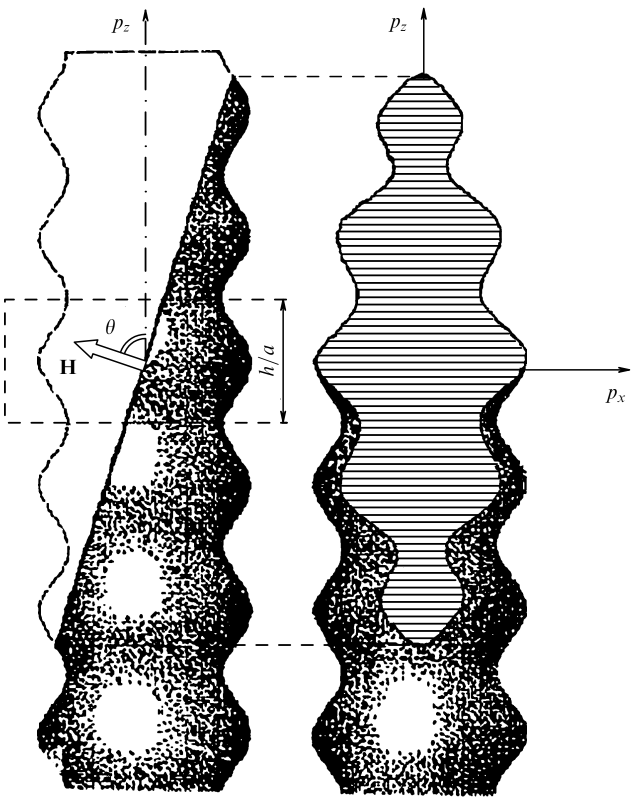

Figure 1. Fermi surface as a corrugated cylinder and trajectories of the conduction electrons in p_y-p_z and p_x-p_z projections.

ratio of the period T of the electron motion around a closed orbit to the time τ of electron mean free path, the resistance remains almost independent of the magnetic field. As the resistance maximum in a strong magnetic field is very sharp, any averaging over angles in the range which includes $\theta = \pi/2$ results in a linear increase of the resistance with increasing field in a polycrystalline conductor with the FS in the form of a corrugated cylinder (see Fig. 1), and even in a single-crystalline conductor with a small mosaicity. In this way, it was possible to explain the linear dependence discovered by Kapitza in a large number of metals for resistance as a function of the magnetic field [2].

In the same year, 1958, Alekseevskii and Gaidukov [3] showed, studying the magnetoresistance of a single crystal of sufficiently pure gold, that the averaged value of resistance for four distinct orientations of a magnetic field grows linearly with the field.

That was the beginning of my collaboration with Nikolai Evgen'evich and with the staff members of his laboratory. I had access to the 'kitchen' of experimental projects, since my line of inquiry was the theoretical interpretation of experimental results, and I had to visit Moscow roughly once every two months.

Differences in the rate of growth of magnetoresistance with increasing magnetic field for different crystallographic orientations of the magnetic field vector were observed as early as the 1930s and at the beginning of the 1940s by Justi and Scheffers [4] in copper, by Lazarev, Nakhimovich, and Parfenova [5] in zinc, and by Justi [6] in gold. The rotation of the magnetic field in the plane orthogonal to the current density **j** increased the resistance at the maximum of such a 'rosette' much more greatly than at the minima. This behavior seemed baffling, because it contradicted the description of electron transport based on the concept of the energy spectrum of charge carriers in metals as a quadratic function of the momentum dependence of their energy, as well as the assumption that the periodic potential of ions affects only the effective mass m^* of conduction electrons. In the absence of a magnetic field, this interpretation of the energy spectrum of conduction electrons did not lead to contradictions between experiments and theory, and was thus quite acceptable up to the 1950s. At the same time, Arnold Sommerfeld and Hans Bethe [7], in their excellent book on the electron theory of metals, gave the computed constant-energy surfaces for electrons in metals with face-centered cubic symmetry in the entire conduction band. Most of these surfaces were open, but this circumstance did not receive proper attention.

Alekseevskii was a brilliant experimentalist and immersed himself fanatically in physics. His creative career began in L V Shubnikov's Low Temperatures Laboratory at the Ukrainian Physical-Technical Institute in Kharkov, where he discovered about a dozen new superconducting alloys and studied the effects of magnetic fields on their superconducting state. In his position of head of laboratory, Nikolai Evgen'evich loved to work on the technical preparation of experiments and to conduct measurements. In the first half of the day, he ran through the laboratory in his lab coat. Then he would disappear, as he still had plenty of energy left to lecture students at Lomonosov Moscow State University and be Head of the Chair of General Physics at the Moscow Institute of Physics and Technology. N E Alekseevskii could reappear at the lab half an hour before the official end of the working day and dump plentiful assignments on the desks of his team members. He was known for his immense ability to work, and he demanded the same from his disciples; hence, he considered staying in the lab after working hours a normal phenomenon, and even a duty. But it should be added that he treated his staff with care and attention.

Owing to the research into anisotropy of magnetoresistance of a large number of metals in Alekseevskii's laboratory and in theoretical papers [1, 8, 9], the topology of the FS was determined over a relatively short time in gold [3, 10, 12], tin [9], copper and gallium [11], magnesium [13], lead [14], silver and zinc [15], thallium [16], rhenium [17], vanadium and tungsten [18], palladium [19], osmium [20], indium [21], titanium and chromium [22], niobium [23], platinum [24], and beryllium [25].

The structure of the FS was soon learnt in detail from review papers and monographs; for instance, the results of studying the FS of metals are adequately presented in the monograph by Cracknell and Wong [26]. It was found that the FSs are open and quite complex in almost all metals except for alkali metals and semimetals such as bismuth. Nevertheless, they can be composed of sufficiently simple topological elements, such as corrugated cylinders and corrugated planes. According to the results obtained in Alekseevskii's laboratory, the FSs of noble metals are 3D networks of corrugated cylinders, and the FS of tin consists of corrugated planes pairwise-connected by struts.

The resistance of polycrystalline gold type conductors, averaged over the different orientations of the crystallites, increased with a strong magnetic field proportional to $H^{4/3}$ (Stachowiak [27]). This result was later derived by Moscow theoreticians Dreizin and Dykhne [28] in a stricter and more elegant way. We see, therefore, that the quadratic dependence of the resistance of polycrystalline specimens of metals with an open FS on H in a growing magnetic field is replaced by slower, nearly linear dependence, in agreement with Kapitza's observations.

The last 25 years have seen a considerable intensification in the interest in galvanomagnetic phenomena in fairly complicated complexes for charge transfers possessing metallic conductivity, which include, in particular, layered conductors with a quasi-2D energy spectrum. This interest was stimulated by the discovery in one of the modifications of the organic layered conductor β-$(BEDT\text{-}TTF)_2IBr_2$ of the clearly pronounced Shubnikov–de Haas effect in the range of magnetic fields up to 14 T [29, 30]. In such high fields, the dependence of the magnetoresistance across the layers on the orientation of the magnetic field was found to be very esoteric [30]. The curve of magnetoresistance as a function of the angle θ between the vector $\mathbf{H}$ and the normal $\mathbf{n}$ to the layers showed sufficiently sharp maxima, whose positions were periodically repeated as a function of $\tan\theta$ in the entire range of angles, except for a small neighborhood in the vicinity of $\theta = \pi/2$.

In layered conductors, closed constant-energy surfaces in momentum space are possible only near the bottom and top edges of the conduction band, while all other constant-energy surfaces, including the FS, are open. The reason for this is that the energy of charge carriers is given by

$$\varepsilon(p) = \sum_{n=0}^{\infty} \varepsilon_n(p_x, p_y) \cos\left(\frac{anp_z}{\hbar} + \alpha_n(p_x, p_y)\right), \qquad (1)$$
$$\varepsilon_n(-p_x, -p_y) = \varepsilon_n(p_x, p_y), \qquad \alpha_n(-p_x, -p_y) = -\alpha_n(p_x, p_y),$$

where the distance a between the layers depends weakly on the projection of the momentum $p_z = \mathbf{pn}$ onto the normal $\mathbf{n}$ to layers, which is responsible for the strong anisotropy of their electrical conductivity: conductivity across the layers is lower by several orders of magnitude than that along the layers. The velocity of motion of charge carriers along the normal to the layers, namely

$$v_z = -\sum_{n=1}^{\infty} \frac{an}{\hbar}\, \varepsilon_n(p_x, p_y) \sin\left(\frac{anp_z}{\hbar} + \alpha_n(p_x, p_y)\right) \leqslant \eta v_F, \qquad (2)$$

is much smaller than the characteristic Fermi velocity v_F of electrons along the layers; here, the parameter η of quasi-two-dimensionality of conductor is the ratio of the maximum value of v_z on the FS to v_F.

The Fermi surface of layered conductors is weakly corrugated along the p_z-axis, and it can be multisheeted and be composed of topologically distinct elements formed into slightly corrugated cylinders and slightly corrugated surfaces in momentum space. The discovery of Shubnikov–de Haas quantum oscillations in almost all currently synthesized layered organic conductors is an indication that at least one sheet of the FS in such conductors comprises a cylinder whose cross section falls within one unit cell of the momentum space. In the case of periodic motion of the charge in a magnetic field, with a period of T_H, the components of the conductivity tensor that link current density to the electric field $\mathbf{E}$ take the form

$$\sigma_{ij} = -\frac{2e^2H}{c(2\pi\hbar)^3}\int d\varepsilon\, \frac{\partial f_0(\varepsilon)}{\partial\varepsilon}\int dp_H \int_0^{T_H} dt\, v_i(t) \times \int_{-\infty}^{t} dt'\, v_i(t') \exp\frac{t'-t}{\tau}, \qquad (3)$$

where e is the charge, $f_0(\varepsilon)$ is the equilibrium Fermi distribution function of conduction electrons, and t is the time of motion of the charge through the magnetic field.

If we consider the leading approximation in small parameter η, the resistance to current across layers equals the inverse value of σ_{zz}. The asymptote of the component σ_{zz} for $\eta \ll 1$ in the magnetic field $\mathbf{H} = (0, H\sin\theta, H\cos\theta)$ then becomes

$$\sigma_{zz} = \sum_0^{\infty}\int_0^T dt \int_{-\infty}^{t} dt' \left(\frac{an}{\hbar}\right)^2 \varepsilon_n(t)\, \varepsilon_n(t') \exp\left(\frac{t'-t}{\tau}\right) \times \frac{e^3 H\cos\theta}{ac(2\pi\hbar)^2}\cos\left[\frac{an}{\hbar}\left(p_y(t) - p_y(t')\right)\tan\theta\right], \qquad (4)$$

where all integrands depend only on t and t'. In a sufficiently strong magnetic field, where an electron completes many revolutions over the duration of one mean free path, i.e., for $\gamma = T/\tau \ll 1$, the component σ_{zz} of the electric conductivity tensor takes the following form, after we take into consideration small corrections over the parameters $\gamma \ll 1$ and $\eta \ll 1$:

$$\sigma_{zz} = \frac{ae^2m^*\tau\cos\theta}{2\pi\hbar^4}\sum_n n^2 I_n^2(\theta) + \eta^2\sigma_0(\eta^2\varphi_1 + \gamma^2\varphi_2), \qquad (5)$$

$$I_n(\theta) = T^{-1}\int_0^T dt\, \varepsilon_n(t)\cos\left(p_y(t)an\,\frac{\tan\theta}{\hbar}\right), \qquad (6)$$

where σ_0 is its electric conductivity along the layers in a zero magnetic field, and the functions φ_1 and φ_2 are on the order of unity.

The functions $I_n(\theta)$ possess a set of zeros which periodically repeat for $\tan\theta \gg 1$ with a period $\Delta(\tan\theta) = 2\pi\hbar/naD_p$, where D_p is the cross-sectional diameter of the FS along the p_y-axis [31]. As a result of studying angular oscillations of magnetoresistance for different orientations of the magnetic field, it is possible to completely reconstruct the shape of the slightly corrugated cylinder. In the case of a multisheeted FS, a contribution to the oscillatory dependence of resistance is also provided by carriers on a flat-sheet FS, and then the corrugation amplitude can be found from the periods of these oscillations [32].

If $\theta = \pi/2$, the main contribution to the component σ_{zz} of the electric conductivity tensor comes from a small fraction of conduction electrons whose orbits lie close to the self-intersecting section of an FS: $p_H = p_c$. These charge carriers move slowly along z-axis with a period $T(p_H)$ comparable to (or greater than) the mean free path duration τ at an arbitrarily high value of the magnetic field. Low velocity $v_x = \partial\varepsilon/\partial p_x$ in electron orbits with $p_H = p_y$, close to p_c, corresponds to a weak dependence of ε on p_x, so, in this case, when calculating the period $T(p_H)$ of electron motion, we are allowed to make use of the expansion of energy into a power series in small p_x. Omitting higher-order harmonics in formula (1), we obtain

$$\varepsilon = \varepsilon_0(0, p_y) + \frac{p_x^2}{2m_1} + \varepsilon_1(0, p_y)\cos\frac{ap_z}{h}. \qquad (7)$$

Using last relationship, we can easily calculate the period of motion of an electron in nearly self-intersecting orbits:

$$T(p_y) = \eta^{-1/2}T(0)\int_0^{\pi} d\alpha\,(\xi^2 + \sin^2\alpha)^{-1/2}, \qquad (8)$$

where

$$\xi^2 = \frac{\varepsilon - \varepsilon_0(0, p_y) - \varepsilon_1(0, p_y)}{2\varepsilon_1(0, p_y)} . \tag{9}$$

As an electron approaches a self-intersecting orbit, ξ becomes arbitrarily small, and the integral in formula (8) diverges as $\ln(1/\xi)$. In a quasi-two-dimensional conductor, the condition $T > \tau$ can hold true in a wider range of electron orbits than in ordinary metals, since the period of motion of electrons near a self-intersecting orbit is inversely proportional to the small parameter $\eta^{1/2}$, even if $\xi \sim 1$.

If $\eta^{1/2} \leqslant \gamma_0 = T(0)/\tau \ll 1$, the cross-layer electric conductivity

$$\sigma_{zz} = \eta^2 \sigma_0 \gamma_0 \tag{10}$$

diminishes in proportion to $1/H$ with increasing magnetic field. In ultimately strong magnetic fields, i.e., when $\gamma_0 \ll \eta$, the fraction of electrons (for which $T > \tau$) decreases, and as H increases the linear increase in the resistance of single-crystalline samples is replaced by quadratic growth. Notice that the contribution to σ_{zz} from the small fraction of charge carriers on open cross sections of the FS near a self-intersecting orbit, namely

$$\sigma_{zz} = \eta^{3/2} \sigma_0 \gamma_0^2 , \tag{11}$$

is still greater than the contribution to σ_{zz} of all remaining conduction electrons [33, 34]. Formulas (10), (11) also hold for small deviations of the orientation of the magnetic field from the surface of the layers, when $\eta \tan\theta \gg 1$, and there are self-intersecting sections of the corrugated cylinder. If $\eta \tan\theta \leqslant 1$, magnetoresistance becomes a nonmonotonic function of angle τ [35], and if $1 \ll \tan\theta \ll 1/\eta$, a transition is observed to strictly oscillatory behavior.

In some tetrathiafulvalene-based compounds, magnetic breakdown is possible between sheets of the FS (see, e.g., paper [36]). In this case, the movement of charge carriers is complex and very intricate. If the probability w of magnetic breakdown is low, namely $w < T/\tau$, i.e., an electron can make only a single hop from one sheet of the FS to another over a mean free time, then the probability of reconstructing the form of a weakly corrugated cylinder, as well as the definition of the corrugation of a planar FS sheet, remains the same as at $w = 0$. If, however, $w \gg T/\tau$, the contribution to electric conductivity of charge carriers participating in oscillatory motion is found to be asymptotically the same as in the case in which the electron inevitably hops to another sheet of the FS, once this chance arises. This leads to the generation of combination frequencies of angular oscillations of magnetoresistance.

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At the origins of applied superconductivity

E P Krasnoperov

Superconducting magnetic systems are widely applied in designing scientific instruments, in engineering, and in medical diagnostics. No serious medical examination can be complete without resorting to magnetic resonance (MR) tomographic imaging, in which magnetic field is a critical factor for the resolution of the instrument. The materials for generating strong magnetic fields are type II superconductors, mainly alloys and intermetallic compounds. First type II superconductors were discovered and described by L V Shubnikov at the Kharkov Institute of Physics and Technology (KIPT). Unfortunately, the term *Shubnikov phase* is rarely used in the literature in this country, even though it would be historically justified to speak of the "Shubnikov phase for which Abrikosov predicted the vortex structure" [1]. N E Alekseevskii started his first experiments on superconductivity under L V Shubnikov's supervision in the 1930s. He studied the decay of superconductivity caused by the action of electric current. N E Alekseevskii continued to be keen on superconductivity throughout his life, putting in a lot of effort toward its practical implementation.

I met N E Alekseevskii while a student at the Moscow Institute of Physics and Technology (MIPT), where he was Head of the Chair of General Physics from 1960 to 1965. He delivered no lectures, as he did not consider himself a born public speaker, and so he avoided speaking in public. Teachers at the Chair remember him as someone who insisted that every teacher take part in research. I I Igoshin, Yu A Samarskii (currently a vice-rector MIPT), and many other staff members began their scientific careers under his guidance.

After P L Kapitza was allowed to return to the Institute for Physical Problems (IFP), N E Alekseevskii headed the laboratory which studied superconductivity and the electron properties of metals. This symbiosis of these two fields of research was legitimate: once the Bardeen–Cooper–Schrieffer superconductivity theory of superconductivity had been created, many believed that the specifics of the Fermi surface may hold the key to calculating the critical temperature. The modern theory has a capacity of explaining, starting from the normal properties of a material, why a material becomes superconductive, but it unfortunately is incapable of predicting this and, even more sadly, it fails to precalculate the superconducting transition temperature T_c.

Alekseevskii's laboratory had only five staff members: two lab assistants, two engineers, and a mechanic. Against this background, the actual span of research conducted by the physics department's students, postgraduates, and visiting researchers seems unrealistic. I will point only to results in the field of superconductivity. First and foremost it was a search for new superconducting materials. Every day, the laboratory fabricated several compositions which were sintered, fused, and vacuum-deposited or were produced by some other methods. Then the achievable conductivity was detected by measuring resistance or by a contactless method (using alternating current), and the possible superconductivity was determined. Alekseevskii constantly lamented that his competitor B T Matthias's team in the USA could produce 10 times as many specimens and, therefore, Matthias discovered new superconductors more often. At the end of the 1960s, both scientists created practically simultaneously the compound $Nb_3Al_{0.8}Ge_{0.2}$ with a critical temperature of $T_c = 20.7$ K, which at the time was a record [2].

To study the electronic properties of metals and the critical parameters of superconductors, high magnetic fields were needed. In the early 1960s, the main materials for superconducting magnets were wires made of the Nb-Zr and Nb-Ti alloys. These alloys were brittle, were insufficiently stable, and allowed the generation of fields no higher than approximately 5–7 T. Every third graduate and postgraduate student in Alekseevskii's laboratory would wind coils of superconducting magnets for the most varied applications. It appears that a superconducting solenoid for nuclear magnetic resonance (NMR) with a field of $H = 5.5$ T was first fabricated in 1970. Owing to the superconducting switch, the magnet had remarkably high stability: the position of the NMR line remained unchanged after a week of operation of the magnet. I remember that Alekseevskii wrote a negative assessment of the proposal to buy a French patent for a stabilized power supply unit (PSU) for superconducting magnets; he was very proud of this. Some time later, an NMR broad-line spectrometer was created in collaboration with the L V Kirenskii Institute of Physics in Krasnoyarsk. The device was demonstrated at the All-USSR Exhibition of Achievements in the Soviet Economy on the occasion of the 250th anniversary of the creation of the Academy of Sciences and won a Gold Medal.

To meet the demands of special communication channels, the laboratory developed high-frequency superconducting circuits with a quality factor of up to 10^5. In one of Leningrad's research institutes, a radio receiver with a superconducting circuit was built; it was successfully tested in Alekseevskii's laboratory.

Alekseevskii was very attentive to superconducting generators for power production. V N Shakhtarin from the staff of the All-Union Scientific Research Institute of Electrical Engineering was a frequent visitor at N E's laboratory and discussed with Alekseevskii numerous problems that they encountered in the course of machine building.

One of the important topics for the laboratory was the research of thin-film properties. Cold deposited films attracted maximum attention; in a number of cases, they were superconducting (e.g., beryllium films) and possessed high critical temperature T_c. Laser-deposited aluminum films displayed a size effect — the dependence of T_c on film thickness. This work on films was naturally extended to designing SQUIDs. Alekseevskii, known very well in the GDR, supervised joint work with Jena University on the application of these devices in magnetometry.

An important event for applied physics took place in 1961. J E Kunzler [3] compressed a mixture of niobium and tin powders in a tube. After annealing at 950 °C, the mixture

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produced a familiar intermetallic Nb_3Sn compound, which had considerably higher critical parameters than alloys of niobium with titanium and zirconium. Two years later, D Martin, M Benz et al. [4] wound from niobium wire a solenoid that they immersed into a bath of tin at $T = 900\,^{\circ}\mathrm{C}$. Solid-state diffusion of the tin resulted in the formation of an Nb_3Sn layer, and the solenoid produced a magnetic field of 10 T at $T = 1.8$ K. Alekseevskii immediately understood the great potential of the new material. He proposed forming a Nb_3Sn layer on niobium tape and, having insulated the tape, making from it wafer windings for solenoids. For ten years this technology of solid-state diffusion of tin followed by the formation of a superconducting layer on niobium tape remained his main passion.

The search for new superconductors, the investigation of their properties, and the development of applications brought to Alekseevskii well-deserved fame and respect, but they also led to sad events in his life. We can only wonder how he managed to courageously withstand the approaching misfortunes. In 1967, he had to stand down as Chairman of the Council on Low Temperature Physics — just when he earned the State Prize for the work on studying the galvanomagnetic properties of materials. One of the reasons for the replacement of Alekseevskii was his all-consuming passion of improving superconductor technologies. He was accused of shifting the brunt of the activities of the Council on Low Temperature Physics to the development of applications at the expense of 'highbrow science', for which the Academy of Sciences was the standard bearer.

A bench unit for fabricating Nb-Sn-coated tape was built by a team involving N N Mikhailov (Head of the Chemical Laboratory at IFP). The size of the bench was impressive, and many chemists contributed to its creation. Work on this technology was simultaneously started at the I P Bardin Central Research Institute of Iron and Steel Industry, the A A Baikov Institute of Metallurgy and Materials Science, and the Physical–Technical Institute for Low Temperatures (PTILT) (Kharkov). Alas, the scientific community of the IFP regarded such 'industrialization of superconductivity' very negatively. P L Kapitza removed N N Mikhailov as Head of Chemical Laboratory and replaced him with G E Karstens, who was N E Alekseevskii's right-hand man. This seems paradoxical, but at the beginning of 1968 all work with Nb-Sn-coated tape at the IFP was discontinued. N N Mikhailov transferred to the Kurchatov Institute of Atomic Energy, where a technological laboratory was created with A P Aleksandrov's support, specifically for manufacturing superconducting tape. The loss of two assistants and of his favorite technological offspring was the heaviest blow, but Alekseevskii proved strong enough! You may hear people say that he looked dour and at times even grim. Perhaps, this is true; but would it be possible to remain cheerful and optimistic after such twists of fate?

Fully conscious of the importance of applied superconductivity research, Alekseevskii wrote a memorandum to the USSR Academy of Sciences on the need to develop the tape technology for critical current density up to 10^5 A cm^{-2}. This tape was demanded for accelerators under construction, for high-field research magnets, and for industrial-scale power generation.

On 27 September 1968, the Presidium of the USSR Academy of Sciences issued a Decree No. 632 entitled "Prospects for research in superconductivity, development of superconducting materials, and applications of these phenomena in science and engineering", as part of the program of preparing forecasts for the most important research areas. In accordance with this decree, a temporary Commission on Superconductivity was organized (chaired by N E Alekseevskii).

A year later, on 18 July 1969, Alekseevskii presented a report to the Presidium of the USSR Academy of Sciences, "On prospects for research in superconductivity, development of superconducting materials, and applications of this phenomena in science and engineering." (I helped Alekseevskii to illustrate the graphics and remember that day very well.)

The extensive report covered the search for new superconductors, especially high-temperature ones (which at the time meant 25 K!), touched on approaches to the theoretical analysis of the phenomenon as such, and went into detailed problems of applying superconductivity in power generation, electric transmission lines, superconducting energy storage, and electric engines. The report outlined how superconductivity is used in large-scale magnetic systems of accelerators (they were being built on a grand scale then). The need in superconducting materials for a single accelerator, for a thermonuclear station, and for an MHD generator was evaluated; these were impressive numbers: 10–50, 60, and 7 t, respectively. The report stressed the importance of developing an industrial cryogenic potential, and underlined the need to greatly increase the production of gaseous helium in the country for cryogenics.

The report was endorsed by the Presidium.

The floor was now taken by P L Kapitza, Director of the IFP of the USSR Academy of Sciences. In his habitual unhurried manner, Kapitza remarked that superconductivity has become a very broad area of research and suggested highlighting the principal directions along which to concentrate the available resources. Academy President M V Keldysh abruptly interrupted Kapitza and reminded him of the history about a hydrogen-driven rocket engine: "At some point in the past we passed a Resolution that ordered to cease all hydrogen-based research in view of the inherent dangers. Now the USA has launched Saturn 5, while we do not have a single laboratory capable of working with hydrogen." Kapitza retorted that hydrogen was less dangerous than oxygen (he had a lot of experience in this field). "Hydrogen does not explode," — says Kapitza, "until the mixture has been prepared." He referred to Alekseevskii, who constantly worked with liquid hydrogen in the course of testing new superconductors.

The debate was blocked by A P Aleksandrov. He informed the meeting that the Ministry of Medium Machine Building and the Kurchatov Institute would take on the problems of developing and manufacturing superconducting materials. He went into details of the available options and outlined the current situation. The issue of superconducting wire, the main one, was thereby resolved. The Presidium then switched to the less pressing problems of the development and application of superconductors.

On 1 August 1969, the RAS Presidium passed Resolution No. 751. Its article IX stated: "To endorse the key messages of the report of Corresponding Member of the USSR Academy of Sciences N E Alekseevskii, "On prospects for research in superconductivity, development of superconducting materials, and applications of this phenomena in science and engineering", prepared by the Commission on giving the Forecasts for the Development of Superconductivity. To establish a permanent Commission on Superconductivity for the Coordination of Research and Further Developments in This Field within the Section of Physico-Technical and Mathematical Sciences of the Presidium.''

The Commission was to monitor a wide range of superconductivity research projects, the latest search for new superconductors, and the expansion of applications-oriented work. The RAS Presidium assigned eleven institutes of the Academy (Institute for High Pressure, Institute of Solid State Physics, P N Lebedev Physical Institute, IFP, PTILT, Institute of Chemical Physics, Institute for High Temperatures, etc.) specific tasks covering the entire spectrum of research activities and applications of superconductivity. A P Aleksandrov, as a member of the Presidium, was chosen to head the Commission on Superconductivity. As he was already responsible for an immense field of atomic power research, the entire work of the Commission on Superconductivity lay on the shoulders of his deputy, namely N E Alekseevskii.

At the same time, the RAS Presidium submitted to the State Committee on Science and Technology under the USSR Council of Ministers its recommendations to greatly expand the industrial production of superconducting wire and devices requiring it. It looked as if the road was opened for the Nb-Sn tape, all the more so since the General Electric factories (USA) had begun to work on a technology for depositing the Nb_3Sn coating onto the tape from the gas phase. At the beginning, the solid-diffusion technique appeared to be simpler, but the actual process proved to be much more complicated: liquid tin through which the niobium tape was drawn collected pollutants, perhaps because of insufficient purity of niobium; as a result, it was impossible to produce long specimens of a superconductor with the same high critical parameters. Later on, another drawback inherent in the tape itself was revealed.

In 1974, the Intermagnetics company brought to Moscow superconducting magnets with coils implemented as Nb-Sn wafers made of General Electric tape. The magnets produced a magnetic field of 17.5 T. This was phenomenal! It was soon found, however, that wafer coils were not very reliable. After a few transitions to the normal state (these are unavoidable), the superconductor irreversibly degraded, and the magnetic field dropped by 2–3 T. This deterioration was caused by the low mechanical strength of niobium, which formed the base of the tape. The reputation of the tape was greatly 'marred', and in the race to lower costs it soon lost the battle to a new, 'bronze' technology.

The essence of the bronze technology consists in the fact that niobium rods were immersed into the matrix of tin bronze ($Cu + 13\%$ Sn). This half-product was drawn through a die until the required wire diameter was achieved. The cable wire obtained in this way was coated with a heat-resistant insulator and then used for winding the solenoid. The prepared coil was placed in a furnace at $T = 900\,°C$. Tin diffused out of bronze, and a micron-thick layer of Nb_3Sn formed on the surface of the niobium. The technology of manufacturing the original wire became simpler, while that of magnet assembly became more complex. In the USSR, the bronze technology was initiated by researchers at the All-Union Research Institute of Inorganic Materials (currently known as the A A Bochvar All-Russian Research Institute of Inorganic Materials): V Ya Fil'kin, A D Nikulin, and A K Shikov. This technology is nowadays the main one in the industry of superconducting cable wires for high-field magnetic systems. Although the maximum field produced by magnets with wire coils is 1 or 2 T lower than that of tape (wafer) magnets, their stability and reliability are higher. The bronze technology offered several advantages: the absence of other stannides in the diffusion layer, in addition to Nb_3Sn, high strength of the cable wire, automatically produced current and magnetic stabilization, the high current-bearing ability, and — the main advantage — the constancy of parameters over great lengths of the wire.

In the mid-1970s, all work on tape superconductors was discontinued. Interest in them returned to some extent with the advent of high-temperature superconductors. It is possible that the method of fabricating superconducting tape using solid-state diffusion, which N E Alekseevskii started to develop, may resurface, provided the latest technologies and multilayer (reinforced) niobium tape are merged into an integrated process.

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International Laboratory of High Magnetic Fields and Low Temperatures: how it was set up and how it evolved

V I Nizhankovsky, V I Tsebro

When paying tribute to the 100th anniversary of the birth of the outstanding physicist, Corresponding Member of the USSR Academy of Sciences Nikolai Evgen'evich Alekseevskii, an eminent expert in superconductivity, in low-temperature physics and the physics of metals, we should never forget the tremendous contribution that Nikolai Evgen'evich made to setting up and expanding the International Laboratory of High Magnetic Fields and Low Temperatures (IL) of the Polish Academy of Sciences (PAS) in Wroclaw. N E Alekseevskii's achievements in the above areas of physics are well known and widely recognized. When reviewing N E Alekseevskii's scientific organization activities and his intense work on

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a number of Boards and Commissions, including international ones,[1] the undoubtedly most significant achievement of Nikolai Evgen'evich as science organizer was the creation in 1968 of the International Laboratory in Wroclaw and his active participation in its life for a quarter of a century.

The motives that drove N E Alekseevskii into tackling this difficult task, which consumed a great deal of his time at the expense of his beloved physics, are outlined at the beginning of his article (written together with E P Krasnoperov) in a collected volume [1] outlining the multilateral cooperation of the Academies of Sciences of socialist countries; it was published in 1978 when the IL had its 10th anniversary: "Progress in modern solid state physics depends to a large extent on the possibility of conducting experiments in high magnetic fields and at very low temperatures. Research facilities for such work are typically quite expensive. Their joint operation reduces the costs of expensive experiments, avoids duplication of effort, and permits sharing the experience accumulated in different countries; in other words, this increases the efficiency of research. Research projects requiring complicated and very expensive equipment then become affordable for every country, opening to each of them the chance of conducting fundamental research."

According to the recollections of C Bazan, who headed the International Laboratory from 1968 until his retirement in 1985, the idea of establishing it belonged to the Polish side. In memoirs published in book [2], he wrote: "The very idea of creating a laboratory of this caliber seems to be traceable to the Polish physicist Prof. Roman Ingarden. In the 1950s, he organized in Wroclaw the Low Temperatures Laboratory of the Polish Academy of Sciences; he also visited low-temperature research centers of Europe, including those in Moscow and Kharkov. It so happened that he met N E Alekseevskii and even at that early date they were able to conduct preliminary discussions on the expedience of setting up the IL.... An official proposal was submitted in 1964 at the regular meeting of the Expert Commission of CMEA (Council for Mutual Economic Assistance) in Minsk.... The idea of creating this laboratory was immediately and warmly supported by Nikolai Evgen'evich, and he immediately started fostering its implementation...."

Notice that the choice of location for the future International Laboratory was, to a great extent, predetermined, and certain favorable factors facilitated anchoring the IL in Wroclaw. First, the Institute of Low Temperatures and Structure Research of the Polish Academy of Sciences already had a laboratory (headed by R Ingarden) in Wroclaw; it was developing water-cooled magnets and after 1961 conducted experiments in the 4-T magnetic field of a Bitter solenoid. In addition, by 1968 when the IL began to function, K Trojnar (who became the IL's chief designer of Bitter solenoids [3]) had already designed a 10-T solenoid. Second, since Wroclaw's electric tram network was at the time redesigned to a rectifier power supply, the town released a set of motor-generators with a total power of about 9 MW, so the Wroclaw subdivision of the power supply network transferred to the Polish Academy of Sciences the building of the power station (Fig. 1a) which housed the AC/DC current transformation machinery, including its motor-generators (Fig. 2), switches, and transformers. Finally, there were no problems with liquid helium supply for the experiments: there was a Philips helium liquefier and a cheap helium resource (the deposits of natural gas near Wroclaw

Figure 1. The old (a) and the new (b) buildings of the IL (photos from 2003).

Figure 2. Motor-generator room for feeding the Bitter solenoids in the IL old building (current look).

[1] From 1952 to 1954 and from 1962 to 1967, N E Alekseevskii headed the Scientific Council on Low Temperature Physics, was for many years the Chairman of the Commission on Superconductivity within the Section of Physical-Technical and Mathematical Sciences of the Presidium of the USSR Academy of Sciences, and from 1966 to 1972 represented the USSR in the Commission on Extra Low Temperatures of the International Union of Pure and Applied Physics (IUPAP).

showed very high helium content, and later production for helium there and liquefaction grew to an industrial scale).

After numerous preliminary consultations and reappraisals, representatives of four Academies of Sciences — the People's Republic of Bulgaria, the German Democratic Republic, the Polish People's Republic, and the Union of Soviet Socialist Republics — on 11 May 1968 signed "The agreement on the establishment of the International Laboratory of High Magnetic Fields and Low Temperatures" with a view to conduct theoretical and experimental research on strong static magnetic fields and at low temperatures. The agreement defined the organizational principles of leadership and the main areas of work at the IL. The activities of the IL were to be guided by a collective body — the IL Scientific Council, and the IL Director. At the first session of the Council, N E Alekseevskii was elected the Council Chairman, and the post of Director went to W Trzebiatowski,[2] one of the active initiators of the creation of the IL. The Protocol to the Agreement on the foundation of the IL stipulated that the size of the participating share for the running cost of research be defined as: Bulgarian Academy of Sciences — 8%, German Democratic Republic Academy of Sciences in Berlin — 25%, Polish Academy of Sciences — 26%, and USSR Academy of Sciences — 41%.

Under Article III of the Agreement, the main tasks of the IL were outlined as:

— study of the properties of superconductors, novel superconducting systems, shapes of the Fermi surface in metals, electron structure of magnetic materials, interaction between magnetic moments of nuclei in solids;

— generation of the lowest possible temperatures and development of adiabatic demagnetization methods;

— development of techniques for inducing very high magnetic fields;

— designing coreless electromagnets cooled by water or condensed gases;

— metrological work in strong magnetic fields and low temperature environments.

The most important dates and stages in the history of the IL from its foundation to these days are given on the web site of the laboratory (Table 1) [4].

At the first stage, the main efforts of the IL staff members focused on the development of methods for generating high magnetic fields with the aid of water-cooled Bitter magnets, and on designing magnets of various powers as the backbone of the experimental base of the laboratory. As a result, several Bitter magnets were completed by 1975 at the IL one after the other (Table 2), including the largest three-section E200 magnet (Figs 3 and 4). In parallel, a large-scale research program was carried out to modernize water-cooled magnet systems. Thus, the effect of cooling orifices in copper disks of the solenoid on its efficiency was thoroughly investigated in order to achieve the maximum field at a given power, and their optimized distribution was found [6]. It was suggested that the experts from the L V Kirenskii Institute of Physics of the Siberian Branch of the USSR Academy of Sciences

Table 1. Stages of development of the International Laboratory.

1964	The idea of creating the laboratory is launched (Roman Ingarden)
1968	Agreement on creating the IL is signed between CMEA member countries — Bulgaria, East Germany, Poland, and USSR
1975 – 1980	The commissioning of the 20-T Bitter electromagnet and 48-T pulsed magnet
1982 – 1985	Development of the idea of substantial modernization of the laboratory
1988 – 1989	Beginning of modernization, which assumes generation of a permanent magnetic field of up to 30 T and switch to thyristor power supply
1993 – 1994	Changing the direction of development towards creating quasistationary magnetic fields with long pulse duration
1999 – 2001	Partial completion of modernization program. Commissioning of the new laboratory building
2002 – 2005	Designing and starting operation of a series of quasistationary magnets
2004 – 2007	Significant upgrade of instrumentation. Acquisition of a new superconducting magnet (15 T)
2006	The laboratory was assigned the status of an International Institute of the Polish Academy of Sciences

Table 2. Bitter magnets of the International Laboratory.

Type of magnet	Maximum field, kOe	Diameter of the working orifice, cm	Power, MW	Heat removal, W cm^{-2}	Year of commissioning
E40*	43	8.5	1.5	218	1961
E100	90	4.3	1.8	450	1969
E150	145	3.4	4.2	540	1971
E200	185	2.5	5.2	660	1975
* Dismantled in 1973 (see Ref. [5]).					

modify the traditional Bitter design by replacing the demountable disks by epoxy-resin-compounded wafers. Experiments conducted at the IL showed that heat removal in channels cooling the wafers was higher and, moreover, this structural design could hold higher mechanical loads [8]. In the end, however, such compounded wafers had to be rejected, as their service life was too short.[3] However, another idea of Krasnoyarsk scientists B Khrustalev, Yu Kocharov, and Yu Katrukhin proved completely successful — to use 'polyhelix' type coils as the inner section of the three-section E200 magnet [9]. In so doing, the maximum field achieved in the E200 magnet was 207 kOe [instead of 185 kOe (see Table 2)] but — more importantly — the service life reached 15 years(!).

In addition to water-cooled magnet systems, superconducting solenoids were also used at the IL. One of them (ordered by the IL and manufactured in 1975 of niobium–tin tape by Intermagnetics[4]) generated a magnetic field up to 15 T and continues to be used. Another superconducting solenoid

[2] Wlodzimierz Trzebiatowski (1906–1982) — Polish chemist, a foreign member of the USSR Academy of Sciences (1976), Academician (1952), President of the Polish Academy of Sciences (1972–1977). In 1966–1967, organized in Wroclaw, along with R Ingarden, the Institute of Low Temperature and Structure Research of the Polish Academy of Sciences, which he continued to guide until his death in 1982. Published work on inorganic and physical chemistry, technology of nonferrous and rare metals, and magnetism of rare-earth and uranium compounds.

[3] K A Trojnar recalls in his memoirs [3] that: "...we have learnt from employees of the MIT Francis Bitter Magnet Laboratory at the conference on magnetic technologies in Grenoble in 1983 that they also worked on this problem there and failed as well."

[4] Note that this was one of the first pilot designs of ribbon type superconducting solenoids manufactured by Intermagnetics (USA), which later produced a whole series of such solenoids.

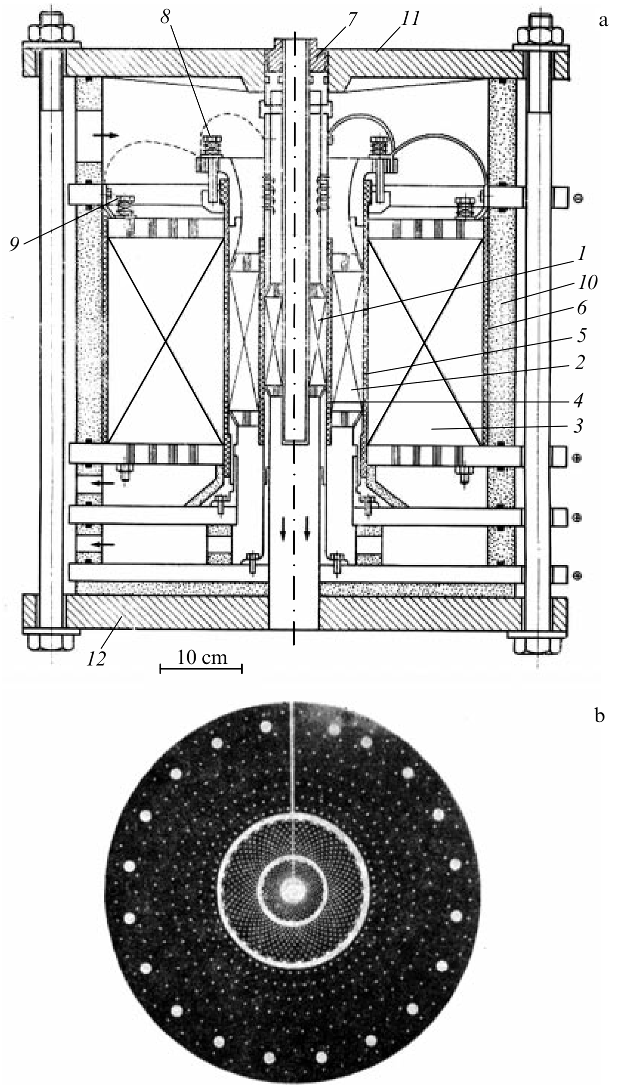

Figure 3. (a) Cross section of an E200 Bitter magnet: *1* — inner, *2* — intermediate, and *3* — outer sections; *4*–*6* — insulation piping, *7* — nut, *8* — bolts, *9* — insulated studs; *10* — housing, *11* — top, and *12* — bottom lids. (b) Configuration of Bitter plates.

Figure 4. (a) E150 Bitter magnet. (b) E150 and E200 magnets (current appearance).

was manufactured of niobium–titanium wire for measuring heat capacity at the IL; it generated magnetic fields of up to 6 T and worked in the persistent current mode. As the next step in extending the methods of generating high magnetic fields, the construction of a hybrid solenoid was considered with a magnetic field of 30 T. The outer section (for a field of approximately 10 T) was to be made of superconducting cable, while the inner water-cooled section (for 20 T) was to comprise novel materials with improved insulation and mechanical properties. (Alas, a number of primarily financial factors led to the cancellation of this project.)

The first ten years of operations of the IL were characterized by intense work in all targeted research areas. N E Alekseevskii focused the thrust of his effort on superconducting systems with high critical parameters, such as compounds with the A-15 structure, and also ternary molybdenum chalcogenides. Research institutions in the USSR and GDR, together with the IL, developed superconducting tape based on a tin–niobium compound, and a cable based on a gallium–vanadium compound. In niobium–tin tape, a record-high critical current density was achieved at the time at liquid helium temperature in a magnetic field of 10–15 T. The properties of ternary molybdenum chalcogenides were actively studied: at superconducting transition temperatures of ≈ 14 K, they possessed very high values of the upper critical field, much higher than the so-called paramagnetic limit.

The study of magnetic materials, such as binary and ternary compounds of uranium with elements of the groups V and VI of the Periodic Table, was conducted at the IL under W Trzebiatowski's supervision in close contact with his Institute of Low Temperatures and Structure Research of the Polish Academy of Sciences. This work, which included measurements of magnetization in a wide range of temperatures and magnetic field inductions, as well as measurements of heat capacity and thermal conductivity, aimed at clarifying the mechanisms of magnetization, determining the magnitude of exchange interaction, and identifying magnetic structures and causes of transition from one magnetic phase to another. In the course of this work, a number of uranium compounds displayed abnormally high magnetic anisotropy fields reaching 10^6 Oe by order of magnitude. Such a strong anisotropy was explained by the presence of an anisotropic crystal field whose interaction with the uranium ion exceeded the exchange interaction by almost an order of magnitude.

N E Alekseevskii also directly supervised galvanomagnetic research at the IL, in which an important place was occupied by the study of the electronic properties of various metals, primarily of the Fermi surface topology and by the study of such a phenomenon as the magnetic breakdown in which electrons in a strong magnetic field tunnel between different parts of the Fermi surface. The experiments conducted at the IL on the study of magnetoresistance in strong magnetic fields revealed magnetic breakdown in niobium, vanadium, ruthenium, and iron. It was also demonstrated that magnetic breakdown as a phenomenon could be employed for practical purposes. Thus, as a result of studying the giant magnetic breakdown oscillations of magnetoresistance in beryllium, a beryllium-based sensor was designed with the sensitivity of 5×10^{-7} to changes in the magnetic field in the range of field induction of 7–8 T. We need to emphasize here that in those years the IL in Wroclaw was among the leading laboratories in the work on magnetic breakdown. The most important factor here was the availability of single crystals of various transition metals having record-high purity; this, in turn, became possible as a result of close cooperation among the Academies of Sciences of the GDR, Poland, and the USSR.

The already mentioned article in Ref. [1] (marking the 10th anniversary of the IL foundation) said: "The International Laboratory grew, over a relatively short period after its creation, into an active international organization. It can be proud of its vigorous international team of researchers who carried out a number of very interesting studies and obtained important practical results.... At the moment, the International Laboratory is cooperating with two institutes of the Bulgarian Academy of Sciences, three institutes of the Academy of Sciences of the GDR, three institutes of the Polish Academy of Sciences, and ten institutes of the USSR Academy of Sciences; their number is constantly growing... ."

Further expansion of the research program at the IL in the 1980s was greatly thwarted by political developments in Poland: the introduction of the martial law in 1981, and the inevitable difficult economic situation in industry and science. Despite these economic hardships, however, the idea of essential modernization of the laboratory started to take shape. This meant first of all the generation of constant magnetic fields of up to 30 T, and transition to thyristor power supply for Bitter magnets. N E Alekseevskii's heroic efforts brought to the project the necessary funding, and by the mid-1980s the USSR gave the project about 1 million rubles. The order to design and build 30 MW thyristor power sources purposely for the Wroclaw IL was placed at factories in the Estonian SSR where it was successfully performed.

The initial plan of modernization assumed expansion of the IL and construction of a new laboratory building on the outskirts of Wroclaw near the River Odra, so as to use river water for cooling the Bitter magnets in the secondary loop of the cooling system. It was assumed that the flexible control system for power supply would make it possible to experiment using four setups in parallel with an 18-T field or, if the entire power was fed into just one setup, then up to 30 T. This was the future as N E Alekseevskii outlined it for the IL. The real history unfolded quite differently. The demise of the USSR and its political and economic implications pushed back the realization of modernization plans for the IL. The new building for the laboratory (Fig. 1b) with the assembled high-power thyristor equipment was commissioned only at the end of the 1990s, several years after the death of N E Alekseevskii. And it was not erected on the banks of the Odra; instead, it was constructed close to the old building.

In the early 1990s, N E Alekseevskii faced serious health problems; in the days that were tragic for science in the USSR,

Table 3. Chairs of the IL Council and IL Directors in successive periods of its operation.

Chair of IL Council	IL Director
N E Alekseevskii, 1968 – 1993	W Trzebiatowski, 1968 – 1982
A S Borovik-Romanov, 1993 – 1997	B Staliński, 1982 – 1993
A F Andreev, since 1997	J Klamut, 1993 – 2012

he practically stopped helping the project. In April 1993, six months before his passing away, N E Alekseevskii delegated his powers of the Chair of the IL Council to A S Borovik-Romanov. (The list of chairs of the IL Council and its directors and the corresponding dates are given in Table 3.) A month before that, a team of experts, composed of scientists from the Russian and Polish Academies of Sciences, addressed the issue of the future of the IL. The main conclusion of that meeting was a proposal to develop at the IL the technology of quasistationary magnetic fields with a strength above 40 T. The reason for this was that electric power required for the regular work of Bitter solenoids became unaffordable in the conditions of the new post-reform Poland (first, the Wroclaw division of the power supply network refused the IL the necessary several MW of power in daytime, so that the IL switched to nighttime experiments on Bitter magnets, and, second, the cost of electric power became so high that the laboratory budget was very negatively affected). It was, therefore, suggested to use high-power thyristor equipment for working in the long pulse mode in which the required electric power is consumed within several tenths of a second [10].

In the second half of the 1990s and at the beginning of the 2000s, a huge amount of work was put into implementing this idea and creating at the IL the technology for the generation of quasistationary magnetic fields with a strength above 40 T. The work proceeded in several research areas at the same time. First, computations were performed of the electric and mechanical properties of the pulsed coils that the laboratory would have to build. Second, it was necessary to select the material for each section of the pulsed solenoid. This was to be a composite alloy of ultimate mechanical strength, possessing a prescribed electric resistivity. The work on such alloys, specifically Cu – Nb microcomposites, was conducted in this country at the Academician A A Bochvar All-Russia Research Institute of Inorganic Materials. This unique material has a record-high strength ($\sigma_B > 1000$ MPa) and high conductivity, about 75% that of pure copper. Third, the thyristor power supply, which was originally meant to be used with Bitter magnets in the steady-state mode, had to be greatly modernized for operating in the long pulse mode. The final debugging of the system controling the power supply to the pulsed coils and of the measuring equipment to study the transport and optical properties of materials samples took more than two years.

Finally, large efforts went into the development and refinement of the technology of winding the pulsed magnets, building the equipment, and manufacturing specialized rigging. To couple the parameters of the magnets to those of the thyristor power supply, the pulsed magnets (Fig. 5) were multisectional, with a combined series–parallel connection of individual sections. Sectioning also achieved a reduction of mechanical stresses in the magnet: it consisted of two or even three coils embedded into one another at a small radial

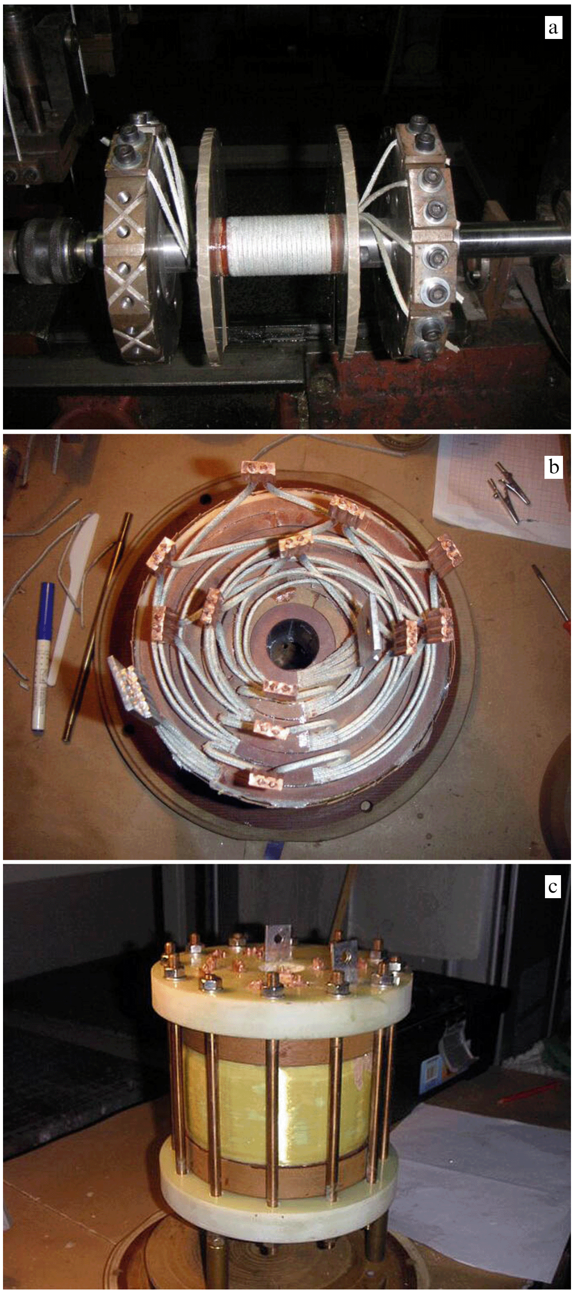

Figure 5. Fabrication of a pulsed magnet: (a) four of 32 layers have been wound up. (b) Connections between layers and sections have been pressure-tested. (c) The magnet in final form.

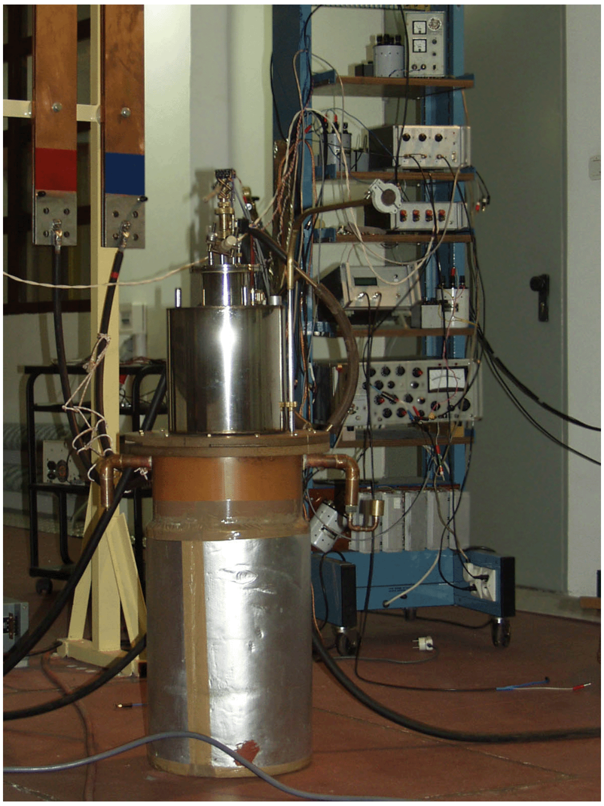

Figure 6. Pulsed magnet (48 T) with helium cryostat and measuring insert in the magnet room of the IL. The magnet is placed in a plastic cryostat with liquid nitrogen.

clearance [11]. As a consequence, a magnet with a working bore of 25 mm could withstand 200 pulses of a 30–38-T amplitude. Figure 6 shows a working 48-T pulsed magnet in the magnetic room of the IL, with a helium cryostat and a measuring insert. A magnet has recently been built at the IL, with the bore widened to 30 mm. Once an appropriate He^3 cryostat is built, the temperature of experiments could be lowered to 0.4 K, retaining a sufficiently high working volume.

The International Laboratory began operations some 44 years ago, of which nearly 20 have been without N E Alekseevskii. Still, his favorite creation lives and expands. Interest in conducting measurements at the IL is still very high, and about 10% of incoming applications have to be declined, because the program is overflowing. Thus, in 2011 experiments at the IL involved 71 scientists (not counting Polish participants), of which 35 were Russian citizens representing 15 institutes of the RAS and 5 universities. In the same year, the IL published 54 papers in periodic journals, and 20 reports in conference proceedings.

The composition of the IL has changed, too. The full (permanent) members of the laboratory paying their dues are the Bulgarian Academy of Sciences (5%), the National Academy of Sciences of Ukraine (5%), the Polish Academy of Sciences (60%), and the RAS (30%). The IL now has associate members — research institutions from the UK (2), Moldova (1), and Germany (1), which pay for cryogenic liquids and electric power. Their right to make decisions concerning the operations of the IL are very limited. The annual budget of the IL is at present slightly higher than 1 million dollars.

"The most important achievement is the team work... Collaboration makes it possible to carry out the most pressing work in solid state physics... . As science the world over becomes more and more expensive, this work has to be conducted by collective effort.'' (From N E Alekseevskii's reply to a question from a local journalist about its most important achievement, in connection with the 20th anniversary of the IL, Wroclaw, 1988.)

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