Victor Valentinivich Eremenko: Early Days in Science

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Viktor Valentinovich Eremenko: Early Days in Science

It was at the early fall of 1955 that Antonina Fedorovna Prikhot’ko asked me to join her on the examination Committee for accepting students for postgraduate study at her Department of Spectroscopy of the Institute of Physics in Kiev. Postgraduate students were called aspirants, the whole body as aspirantura, and I will use these terms in what follows. All together, there were two students, and one of them, Viktor Eremenko, came from the Kharkov University where he fulfilled his undergraduate (Diploma) work on the transport phenomena in metals, the field that was far away from Prikhot’ko’s scientific interests. Hence, she felt herself being not in a position to evaluate his research and wished me to help her. In my opinion, both the research and presentation were excellent, and after the exam I strongly encouraged Prikhot’ko to invest maximum efforts to get Viktor accepted.

It is needed to explain why special efforts might be required for accepting an able young scientist to aspirantura. At that time, aspirantura has been considered by high authorities as a privilege rather than access to hard and productive research work for three years. Indeed, it was the supervisor rather that the aspirant himself who was primary responsible for aspirant’s success. Therefore, acceptance to aspirantura provided a nearly 100% warranty for earning a Candidate of Science (PhD) degree in three years, and with it a rent for life in the form of a quite decent salary because most of former aspirants were automatically accepted to the staff of the institutes. People’s wisdom of that epoch regarding the Candidate of Science defenses sounded as A single hour of shame and a Candidate for life. In the framework of this entitlement system each potential aspirant had to be endorsed at multiple levels from the Institute to the Personnel Department of the Academy to the very top, the Department of Science of the Central Committee (TseKa) of the Party, that citadel of ignorance and obscurantism where relativity, quantum mechanics, genetics, and cybernetics were questioned and considered as reactionary bourgeois pseudosciences. Aspirant positions were assigned the rank of the nomenclatura of TseKa, this means that nobody could be accepted to aspirantura without his (her) approval.
by TseKa. In the long run, this entitlement system strongly contributed into the crisis of Soviet academic science: mammoth-size institutions with mosquito-size scientific outputs.

Fortunately, Victor Eremenko was accepted. However, as in the famous Nekrasov’s poem, “Одной бедой Бог миловал, ... другая подошла” (God’s blessing diverted one problem, but the next one developed). Eremenko started working, and rather successfully (I will come back to this subject below), but he needed place to live. Because he was already married, he could not live in the aspirant dormitory and needed a room, a small and modest, but private. To be honest to the Director of the Institute of Physics, he was rater efficient in securing funds for building housing for the Institute’s personnel. However, under the conditions of global housing crisis, the housing available for the personnel of the Institute was also highly scanty. Not surprisingly, Director used it to manipulate people. I became involved in the first attempt of getting a living room for Victor in a highly untypical for me capacity of a member of the Housing Committee of the Institute where I represented the Theoretical Department. But the story, which might be of some interest by itself, goes wider. For this reason, I complete this paragraph with a line of one of Yury Zhivago’s verses from the famous novel by Boris Pasternak, “судьбы скрещенья” (destinies crossing).

It is currently widely known that at the very end of WWII a number of German physicists were brought to the USSR for working on military projects, in particular on the Atomic Project. Around 1954 the Government decided to let them go back to Germany, but before leaving they had to “cool down” by spending some time at unclassified facilities. At that time we were not aware of these issues and were surprised to see three Germans appearing in the Institute and three small cottages, one for each of Germans, being assembled near the wall of the Institute. It might sound amazingly, but the minor problem of a modest living room for Victor’s family became intertwined with this political process. The events happened more than half a century ago, and I’m trying to recover in my memory the developments as accurately as I can. They were rather deplorable but also included a comical component.
The three Germans were very different. Two of them were young and expressed absolute neither interest nor knowledge in any sort of science. They spent whole working days in their offices with doors open to the corridors and their eyes looking onto ceilings. No piece of paper or any journals on their desks. Their idleness looked as demonstrative. Meanwhile, according to the rumors that circulating across the Institute, their salaries were higher than those of the Heads of Departments. We speculated that they were Nazi functionaries rather than scientists, as Nazis they occupied high administrative positions at German facilities, and were misidentified by our KGB agents as real scientists and brought to the USSR. But this only was our hypothesizing; probably nobody ever spoke with them. If I’m not wrong, these two guys left for Germany in 1955.

The third German, Professor Max Steenbeck, was quite a different sort of person. He was a prominent industrial physicist with impressive career in Siemens. Steenbeck submitted to the Institute a scientific project focused on checking accuracy of the Onsager reciprocal relations supported by detailed calculations, which was discussed and approved by the Scientific Council of the Institute. He also submitted the list of equipment which he needed for his project, asked for one assistant (a young experimenter Petr Baranskii worked with him), and for support of theorists. Solomon Isaakovich Pekar and I were cleared for discussing theory with Steenbeck, Pekar the fundamental issues and I for more technical ones. During our conversations I found out that Steenbeck had excellent knowledge of such classical chapters of theoretical physics as hydrodynamics and classical electrodynamics, but a generational gap did exist, and sometimes he needed help in quantum mechanics and statistical physics. In 1956 Steenbeck has been allowed go back to Germany, and he vacated the cottage. Here “destinies crossing” emerged between the government problem of the repatriation of German scientists and a modest private issue, a room for Victor’s family.

As I remember, the cottage included two adjacent rooms and one isolated small room, and the Housing Committee was summoned for deciding the tenant of this latter room. Director ran the Institute autocratically and took all decisions by himself. However, it was ordered by law that all decisions regarding housing should be made by the Housing Committee that was considered as representing
the whole collective of the Institute. Hence, the Deputy Director for management Luk'yanetz summoned the Committee and told us that there is a single room and a single applicant, a clerk of the procurement Department named Shilkin-Chub, and proposed to vote. I intervened, told about Eremenko and asked to put his name on vote, too. The Deputy responded in typical of him pidgin language, a medley of broken-Russian and broken-Ukrainian: ``Йероменка нье встро́йть, а Шилкина встро́йть” (Eremenko will not be satisfied, but Shilkin will). His move violated the tradition that families enjoyed precedence compared to single persons. Not surprisingly, I lost the vote (who might wish to vote against Director’s will?) but submitted a written “Opinion of Dissent” and ordered to attach it to the protocol. The Deputy objected saying that we are supposed to keep the vote secret, but I responded that I don’t care because I’m not ashamed by my vote. And what was the worst, the whole story made it obvious to me that Victor had only a minor chance of receiving any housing in a foreseeable perspective.

Despite the troubles explained above, the year 1955 was good for both of us, Victor and me, because we worked enthusiastically in a blossoming field of solid state physics. Here I need to say a bit about my personal story and the developments in physics of semiconductors around that time. I graduated from the Kiev University in 1949, and the following five years were `“dark ages” of my life. During these years, I worked in five different places. In particular, I worked at two engineering Institutes of the Academy on strains in dams and made a paper that I consider as one of my very best ones. However, I was fired from both Institutes by the Personnel Department of the Academy because I have not been entitled to do science. The situation somewhat changed after Stalin’s death in March 1953 and the following period that is known as the `“Khrushchev’s thaw”. During this transitional period, when the officials were not sure how they were supposed to rule over us, Vadim Evgenievich Lashkarev managed to get permission of the First Vice-President of the Academy to hire me into his Department of Semiconductors of the Institute of Physics for working on the Government Project on transistor electronics. During the 1954/55 year, until the end of the Project, I worked under the supervision of Kirill Borisovich Tolpygo on
the theory of semiconductor devices. I consider this year as my equivalent of postgraduate study. Simultaneously, I participated in meetings that Lashkarev held with the members of his Lab, and this became an excellent instruction for me in planning experiments and evaluating their results. Lashkarev was a great scientist with wide interests in physics and deep understanding of it. I think that his most fundamental scientific achievement was the experimental discovery, in 1941, of p-n-junctions in semiconductor rectifiers of that epoch.

After completing the work on the Government Project, in the middle of 1955, Lashkarev decided to return to the study of photoelectric properties of the crystals of CdS/CdSe group, and I also had to make switch in my work to support their efforts. However, there was no simple way back to previous studies of the CdS family compounds because the transistor epoch fundamentally changed the physics of semiconductors. Before it there existed basic concepts of the theory of solids such as the Bloch theorem, and semiconductors that were considered as implicitly dirty materials. Practically nothing has been known about their basic properties such as the band structure. Hence, there was plenty of space for speculating about the processes in them. Investigation of germanium, the material used for producing first transistors, established new standards for the research. Germanium could be properly cleaned, prepared as a nearly intrinsic, and then n- or p-type doped in a controlled way. Step by step, its band structure has been recovered: positions of the minima of the conduction band and of the maximum of the valence band in the Brillouin zone, degeneracy of the valence band and its spin-orbit splitting, effective masses of charge carriers, etc. Under such conditions, the space for speculations had been strongly constricted, and physics could be investigated in well defined terms.

As distinct from Ge, scientific information on CdS-type compounds had been highly restricted at 1955. Under such conditions, the study of kinetics of room-temperature photoconductivity could provide only strictly limited information because it depends critically not only on the intrinsic properties of the material but also on uncontrolled defects and impurities acting as recombination and trapping centers. Therefore, it was decided to join efforts with the Spectroscopy Department of the Institute which possessed techniques
and experience in taking absorption and fluorescence spectra in polarized light down to 20K. The group of Vladimir Broude, to which Victor has been assigned after his admission, has been charged with this task. The group had wide experience in the spectroscopy of excitons in molecular crystals, especially of the crystals of the benzene row. However, they never worked on semiconductors and therefore had to adjust to working with a different sort of material and learn physical concepts that were new for them. Because I already had some experience of working with Broude group, I was assigned to help them with theoretical aspects of this study, while Viktor became the principal working horse of the project. He managed to adjust to the new for him techniques and concepts and move ahead with taking and processing data so fast and with such efficiency that he earned inside the group a facetious title of a ``genius aspirant”.

It is proper to mention that while understanding of the electronic energy spectrum of CdS-type crystals was absent at that time, their spectroscopy was already a field of active research, in Leningrad in the USSR and also in a number of laboratories in the West, but assignment of the absorption and emission bands observed at different laboratories remained a subject of disputes. From the standpoint of photoconductivity, the research on excitons was also important because in some early studies their presumable role in photoelectric phenomena has been invoked but never justified.

In direct-gap semiconductors excitons are expected to manifest themselves as narrow absorption bands below the low-frequency edge of the fundamental (band-to-band) absorption. In perfect crystals they are the lowest excited states of the electron subsystem. Broude and Eremenko observed up to ten narrow bands at the low-energy edge of the absorption region, but they found that the relative intensity of those bands (and even the very fact of the existence of some of them) changed from sample to sample, and even from point to point inside the samples, and depended on etching of them. Therefore, we concluded that this group of bands cannot be attributed to free excitons but might be assigned to excitons weakly bound to impurities or lattice defects. We made a conjecture that they might be similar to α- and β-bands in alkali halides that were previously attributed to excitons weakly bound to lattice defects (F. Seitz, 1954); such
entities are frequently quoted as “impurity excitons”. Our results were published in *Soviet Physics Doklady* in 1957. The spectrum of free excitons in CdS was investigated and assigned by Gordon Thomas and John Hopfield at Bell Labs two years afterwards, in 1959, through measuring reflection spectra and performing Kramers-Kronig inversion of them. Such quantitative techniques were not available to us, and while we reported a few strong absorption bands assigned afterwards to excitons, we could not measure their intensities and even evaluate their polarization ratios along the crystal axes. Hence, we were not in a position to propose their assignment.

It is important that CdS spectra were investigated by Broude’s group in parallel with the spectra of molecular crystals of the benzene family where similar bound-exciton bands were observed below the low-energy edge of the intrinsic-absorption spectrum. They were sometimes disproportionally strong, if estimated per molecule, as compared to the intrinsic exciton absorption, and possessed anomalous polarization ratios. Comparing similar properties of the edge spectra of two different groups of solids allowed us to develop a unified viewpoint onto such spectra that we presented in a short review paper published in *Physics-Uspekhi* (1959). In the perspective, it allowed identification of exciton absorption bands and investigation of the energy spectra of molecular excitons by doping crystals with impurities of a controllably-changed isotopic content, and developing the concept of giant oscillator strength of “impurity excitons” in semiconductors. They are working as antennas harvesting oscillator strength from a wide area of the crystal around an impurity or a lattice defect.

A few years after his PhD defense, Victor received and accepted an offer from the newly established Institute for Low Temperature Physics and Engineering and went back to Kharkov. This allowed him not only to establish his own laboratory, but also to get a decent apartment. It was my pleasure to serve as an Official Opponent at his Doctor of Science defense there. We retained friendly relations for life and are still in touch through Skype.

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