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Kirill Borisovich Tolpygo: Teacher, Advisor and Scientist

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My recollections of Kirill Borisovich go back to the 1946/47 Academic year at the Physics Department of Kiev University. Probably, these are the earliest dates in this special issue of the journal of "Low Temperature Physics" dedicated to Tolpygo' 100th anniversary. He was a great theorist who shaped the development of theoretical physics in Ukraine, first in Kiev and afterwards in Donetsk, and strongly influenced experimental work. Kirill Borisovich served as our instructor at practices in "Electrodynamics" that were termed "seminars". They followed and supported the lectures by Solomon Isaakovich Pekar* on this chapter of theoretical physics.

These were the first post-WWII years, the central street of the city Kreshchatik still was in ruins. Our small class, about 24 students, was enrolled in 1944, less than a year after the liberation of the city from Nazis. It included six veterans of the war, and other students spent years either under occupation or in evacuation. Therefore, everybody had a weak background still from high school, and the situation was aggravated by the fact that the Chair of the Physics Department was a Party functionary having only little knowledge in physics and no interest in it. Students were not warned regarding the problem, and no measures were taken for mitigating it. As a result, even good and devoted lecturers were forced to reduce requirements. I joined the class in 1946 after returning to Kiev from Kazan where I completed the second year of education at the Department of Physics and Mathematics of the University of Kazan. There teaching and requirements were kept at a decent level even under the difficult conditions of the war-time, and I sensed the difference immediately. The problem with our class became explicit in 1949 at the final State exam in physics, with V.E. Lashkaryov** as Examiner and N.N. Bogoliubov*** as the Head of the Committee, when a quarter of the class ended with a mark of 2 and had to retake the exam to earn the minimal required 3.

And it was to this class which came a young and enthusiastic instructor who had a lot of knowledge which he was eager to share with us. I was excited by the interesting problems that he prepared for seminars. These were the circumstances in which I met Kirill Borisovich for the first time, and it was only afterwards when I learned that he served in the army for six years, from 1939 to 1945, and taught us being only a second year graduate student. Service in the army during WWII was a severe school and those who managed to go through it retaining excitement about science constituted an important part of the post-war generation of Soviet scientists. It is easy to figure out that Kirill Borisovich could not feel any special satisfaction while teaching our class but he persistently did his best and never showed even the slightest dissatisfaction.

Feeling strong disparity between the level of education at the Kazan University and the Physics Department of Kiev University, I considered switching to the Department of Mathematics and Mechanics where the situation was

* Solomon Isaakovich Pekar (1917–1985) was a theoretical physicist most widely known for initiating and advancing the theory of polarons. He introduced the term "polaron" for charge carriers in solids. He also advanced theory of electronic polaritons proving that their strong dispersion results in emergence of additional light waves.

** Vadim Evgenievich Lashkaryov (1903–1974) was an experimental physicist with interests in the range from electron and x-rays diffraction to physics of semiconductors. In 1941 he proved, by detecting the sign change of the thermo-e.m.f., that the majority carriers on both sides of cuprous oxide and silver sulphide photocells and selenium rectifiers have opposite signs, in contemporary terms that the barrier layers are *p–n*-junctions [*Izv. AN SSR, Ser. Phys.* **5**, 442 (1941), English translation: *Ukr. J. Phys.* **53**, Special Issue, p. 53–56 (2008)].

*** Nikolai Nikolaevich Bogoliubov (1909–1992) was a famous mathematician and theoretical physicist. He is most widely known among physicists for his theory of weakly nonideal Bose gas and his method in the theory of superconductivity.

considerably better. It was the lectures in theoretical physics by A.S. Davydov* and S.I. Pekar and the seminars by K.B. Tolpygo that kept me at the Physics Department. Now, completing my professional career, I am sure that staying in physics was the right decision for me, and it was the *first* time that Kirill Borisovich influenced my destiny.

As a Diploma student I was in the audience of Tolpygo's PhD (Candidate of Sciences) defense in 1949. His thesis included the very first consistent derivation of the dispersion of coupled polar optical phonons and electromagnetic waves. These mixed excitations are currently known as lattice polaritons, and the figure with avoiding crossing near the resonance appears in many textbooks. The central achievement was the proper account of the electron polarizability of ions that resulted in correct expressions for speeds of electromagnetic waves below and above the polariton resonance. This was a break-through result, and the Thesis was highly praised by the official Opponents and the members of the Scientific Council. The only voice of caution came from Tolpygo's maternal grandfather Boris Ya. Bukreev, a famous Professor of mathematics. At his age of 90 he was in the audience and was curious whether the convergence of Ewald series used in the Thesis had a rigorous mathematical proof. He was concerned that in the absence of such a proof "one can fail as it happened to Sofia Kovalevskaya". Fortunately, fast convergence of Ewald series is well established.

The paper by Kirill Borisovich** was published in 1950 in JETP which was not translated into English at that time. In the meantime Kun Huang who worked with Max Born in England derived an equivalent result in a different way and published it in 1951. Because his result was included into the classical book by M. Born and K. Huang (1954) it became widely known internationally, and it took of all of us special efforts to bring the earlier result by Tolpygo to the attention of our Western colleagues. In the early 1950s Huang and his English spouse Avril Rhys left England for China. Huang was led by a patriotic idea to help in the reconstruction of his country after the end of the Civil War. We have heard that he was "reeducated" there in one of Mao's facilities, but I'm not sure about this. In 1963 Tolpygo and Huang met on friendly terms in Moscow at the International Conference on the theory of solids that was organized and chaired by Ilya Mikhailovich Lifshitz***. Huang looked to be a very nice and intelligent person. Our KGB agents behaved politely at that Conference, they were not seen explicitly. On the contrary, the Chinese guard of Huang followed him openly and most closely, hence, only science could be discussed.

During the five years after my graduation I met Kirill Borisovich only occasionally. These last years of Stalin's rule were marked by extreme chauvinism. I was forced to change jobs five times. But after Stalin's death in March 1953 the atmosphere started changing. This period is known as the "Khrushchev's Thaw". Physicists around the country tried to use the new opportunities to bring back to science their former able students. In Kiev such an opportunity opened because Vadim Evgenievich Lashkaryov led the Kiev part of the All-Union Government Project on transistor electronics aimed in reducing the lag of USSR in this technologically important field. Lashkaryov wished to advance the theoretical part of the effort, the only one where it was practically possible to get any new scientific results. Tolpygo who collaborated with him but was also involved in a number of different projects convinced Lashkaryov that any real progress was only possible by hiring a young able theorist, and he named me as the only candidate. Tolpygo remembered me from the 1946/47 seminar class and Lashkaryov from the 1949 State exam. Pekar who served as my Diploma advisor also strongly recommended me. As a result, Lashkaryov managed to get permission from the First Vice-President of the Academy of Sciences to hire me. Such an extraordinary procedure for hiring at the lowest scientific position was only required because of my "wrong ethnicity". On June 16, 1954, I started working at the Semiconductor division of the Institute of Physics. In this way Kirill Borisovich influenced my destiny for the *second* time.

In the Institute of Physics friends and colleagues, including me, used to address Tolpygo, both warmly and respectfully, as K.B. For brevity, I will use this abbreviation in what follows.

I was highly excited by my acceptance to the Institute, but some internal fear tortured me. I was hired for working on transistors, but during the five years of my nearly complete isolation I had not even heard about them. Would I be able to work equally with those who were privileged to be accepted to the Institute directly after graduation? If not, this would be an absolute catastrophe for me. Surprisingly, I understood very soon that these fears were ungrounded because there were around only two people who really understood the subject, and these were Lashkaryov and Tolpygo. K.B. had some ideas and even brief notes about the future directions of the work, and we started practical work along these lines. To get a regular education in the subject we started translating the seminal 1949 paper by

* Alexander Sergeevich Davydov (1912–1993) was a theoretical physicist most widely known for the "Davydov splitting" in exciton spectra of molecular crystals.

** This paper by K.B. Tolpygo, *Zh. Eksp. Teor. Fiz.* **20**, 497 (1950), is currently available in English translation as *Ukr. J. Phys.* **53**, Special Issue, p. 93–102 (2008). Direct link to the Issue: http://ujp.bitp.kiev.ua/index.php?item=j&id=110

*** Ilya Mikhailovich Lifshitz (1917–1982) was a condensed-matter theoretical physicist most widely known for his work on normal metals (Lifshitz transition) and disordered systems (Lifshitz tails).

Shockley published in Bell Systems Technical Journal. I translated it and in this way studied English (which was new for me) and K.B. edited the translation. After the translation was completed, it was retyped for the experimenters of the division. When I got some knowledge in the field, Lashkaryov ordered me to come to his office in the morning to participate in his meetings with the members of his team. This was a great school for me in understanding experiment.

Of course, of most importance was the research with K.B. during the 1954/55 years. For me this was the most regular and valuable instruction that I ever received in my life. It included both discussing physical mechanisms of phenomena and theoretical approaches for describing them. By the end date of the Government Project in the middle of 1955 we completed several papers. The report of the Institute consisted of two volumes. First volume described experimental results and the second one our theoretical results. At that instant I regained a confidence in myself and was prepared for independent research. In this way K.B. influenced my destiny for the *third* time.

During the work on the Government Project we closely collaborated with experimenters, and K.B. proposed a "Method of constant response" that allowed increasing the accuracy of measuring diffusion lengths of nonequilibrium charge carriers by about a factor of 2 by eliminating errors originating from nonlinearity of the collector. The method was checked in Kiev and afterwards became a part of a standard device developed by GIREDMET (State Institute for Rare Metals) for measuring basic parameters of semiconductor materials.

Our theoretical research was the very first in the USSR on bipolar injection. When we submitted papers to the Journal of Technical Physics in Leningrad, we received absolutely meaningless reports. The Referee did not understand why bipolar injection is central for *p–n*-junctions. K.B. had to write a letter to Abram Fedorovich Ioffe who regained his influence after Stalin's death to request for a competent review. Finally the papers were published and even noticed abroad despite the fact that there still was no regular translation of Soviet journals into English.

At the end of Stalin's era scientific life in the country nearly disappeared beyond the Nuclear Project. The concluding lines of the play of a Russian writer Alexei K. Tolstoy "Death of Ivan the Terrible" come to the mind:

"This is the penalty for autocracy,

This is the outcome of our degradation".

Under the conditions of global secrecy normal contacts between scientists working in the same field but in different cities were broken nearly completely, and only classified meetings inside specific programs were held. In the meantime the Thaw facilitated restoring scientific life in the country. In Leningrad Anton P. Komar who practically destroyed the Physical-Technical Institute (currently A.F. Ioffe Institute) was fired and Boris P. Konstantinov, one of the leaders of the Soviet Atomic Project, was appointed as a new Director of the Institute, and A.F. Ioffe founded a new Institute for Semiconductors in Leningrad. Both of them started restoring theoretical Divisions.

Pekar and Andrei Ivanovich Anselm, the head of the theoretical division of the Institute newly established by Ioffe, agreed on starting All-Union Conferences on the Theory of Semiconductors with the first Conference in Leningrad. It took place early in 1955. The Kiev group of six people was the largest one, I remember four people from Leningrad, a couple from Moscow, and probably a couple from somewhere else. K.B. and I presented our work on transistor electronics. Despite the fact that in the Leningrad Physical-Technical Institute there were several experimental groups working in this field, they had not a single theorist to support their efforts. One experimenter who attended our presentations asked whether we might be interested in giving a seminar in their Institute, and we were happy to accept the invitation. We were brought across several security offices and a gate in a fence enforced by barbed wire (and that's all despite the fact that K.B. had no clearance because of the difficult history of his father). A seminar given by external speakers was such an extraordinary event at that time that Zhores Alferov* told me about 25 years afterwards that he remembered it well.

After the work on the Government Project had been completed, Lashkaryov decided, while retaining the transistor research in his division, to go back to photoelectric phenomena in CdS/CdSe systems which were of primary scientific interest for him. For this reason it was decided that K.B. continues to supervise the Ge research while I switch to physics of these new for me systems. As a result we never worked with K.B. together again, but we regularly discussed all the problems and a close friendly relationship was established among us. Because Lashkaryov was widely interested in physics and deeply understood it, he never pressed me to restrict my work by photoconductivity and supported my research on excitons and spin-orbit coupling that finally brought more valuable fruits.

Meanwhile, a more general drama was developing in the Institute. The policies of M.V. Pasechnik, the Director of the Institute, made the everyday life and work of Lashkaryov and Pekar practically intolerable. They finally came to the conclusion that, to enable normal productive research of their own and for their groups, it was necessary to get independence from Pasechnik by founding a new Institute of Semiconductors. This also would provide a proper platform for technological applications. Both K.B. and I were highly supportive of this project. In the new Institute two theoretical divisions were envisioned, one

* Zhores Ivanovich Alferov, born in 1930, one of the pioneers of heterostructures and 2000 Nobel Laureate.

division headed by Pekar and the other by Tolpygo, with me joining Tolpygo's division.

Unexpectedly for me, K.B. changed his mind. For many years his basic position was in the Institute of Physics, and he also kept a part-time teaching position at the University. While the Chair of Theoretical Physics and afterwards the specialization in theoretical physics (with the first class graduating in 1950) were established by Pekar, he was periodically fired from the University depending on the political climate in the country and trends in the University's internal policies because of his being Jewish. That's why Tolpygo who worked in the University for many years (being Russian he was less vulnerable) gradually acquired the central role in educating young theorists there. Relations between Pekar and Tolpygo were always most friendly and based on mutual respect and trust. But their temperaments were very different. Pekar was reserved, he carefully weighted his every action and word, while Tolpygo was spontaneous and reacted immediately and emotionally. Both the difference in their temperaments and the atmosphere in the University become obvious from their dialog that I witnessed.

K.B.T. How can you return to the University from which you have been already fired twice?

S.I.P. What does it mean University? That's walls. How can any reasonable person be resentful at walls? Students never fired me. They need me and we need them for our future work. That's the reason why I decided to come back to the University.

This dialog also explains why I was so surprised that K.B. took risks to cast in his lot with Kiev University. Definitely, he dreamed to change the atmosphere there for the better, an undertaking with very problematic perspectives from my standpoint. As a practical result, I was appointed as the head of the division of the theory of semiconductor devices and K.B. took a part-time position in the Institute of Semiconductors.

The very first years of the Institute of Semiconductors were marvelous. Lashkaryov was enthusiastic and energetic, and the newly established Colloquiums were exciting. Lashkaryov understood in detail all presentations, from theorists through experimenters to chemists who were responsible for growth of crystals. The general atmosphere in the Institute was favorable and focused on research, the actions of the Administrations were transparent, and people looked ahead with confidence. In 1963 Lashkaryov's 60th birthday was celebrated in this jubilant atmosphere. Unfortunately, soon afterwards Lashkaryov's health started to deteriorate, and a new Deputy Director, a former Instructor of the Central Committee of the Party, was appointed. He was a person of Pasechnik's type but was much simpler and therefore even more dangerous. The atmosphere started to deteriorate fast and drastically. On a number of occasions, I had to bring his actions to the attention of Lashkaryov and ask for his involvement. There was no doubt that the authorities prepared this person to become Lashkaryov's successor as the Director. This was absolutely unacceptable, and I was forced to think about my future.

While I am not aware of the details, K.B. also got into troubles in the University. I became involved only in a single episode. Aiming to advance the education of young theorists, K.B. decided to arrange lectures on applications of group-theoretical methods in solid state physics. He and Pekar proposed me to give such a course. Initially I resisted explaining that I graduated as an experimenter, have never been a graduate student, spent five years after the graduation practically isolated from scientific community, and still need to concentrate on my own education. However, they insisted saying that this was my duty; hence, I had to agree. But when K.B. came with this suggestion to the Chair of Physics Mikhail Biliy, he responded bluntly that he will not allow me into his Department in any capacity. Due to the involvement of Lashkaryov, the lectures were arranged at the Department of Radio-Physics. By the way, the course turned out useful. After I left for Chernogolovka it was continued by my former student Valentin Sheka. And thirty years afterwards, already in the US, I was approached by a graduate of Kiev University named Yuri Sirenko who greeted me with thanks for educating him in group theoretical methods. He took this course with Sheka and at that time applied his knowledge by developing a theory of semiconductor lasers in the University of North Carolina. One can only guess which principles guided some officials in Kiev when they sacrificed the interests of their own students.

Finally, K.B. and I decided to leave Kiev, a beautiful city in which both of us were born. While this happened nearly at the same time, in the middle of 1966, our decisions were taken independently. K.B. accepted the invitation of a newly established Institute of Physics and Engineering in Donetsk, and I moved to a newly established Institute for Theoretical Physics in Chernogolovka near Moscow, currently L.D. Landau Institute.

K.B. was our very first guest in Chernogolovka. We invited Lev Gor'kov and Igor Dzyaloshinskii with his wife Lena and spent a nice evening together. I was in Donetsk twice, the first time as an Opponent at a PhD defense and the second time for a meeting of the Program Committee of probably the very last All-Union Conference on the theory of semiconductors. K.B. termed them "Pekar Conferences" after the death of Pekar. During my first visit to Donetsk, K.B. invited me to his apartment where I was introduced to his mother Tatiana Borisovna. At that time she approached her 100th birthday, and I was surprised that she didn't wear glasses. I asked K.B. whether she also can read without glasses. He responded that she could but did not because she already knew Lev Tolstoy's "War and Peace" by heart and nothing else was of interest for her.

Everywhere above I have only described the events about which I have firsthand knowledge. Scientific

achievements of Kirill Borisovich deserve special discussion. Because of his deep interest in science, wide knowledge of it, fast reaction and excellent memory, he worked simultaneously on several projects, by himself or with various collaborators. He definitely was the greatest expert in the country in lattice dynamics where, in addition to lattice polaritons, he also introduced long-range forces in homopolar crystals that renormalize the deformational potential and manifest themselves in a number of different ways. His scientific interests extended from fast polarons in semiconductors through rare gas solids to physical mechanisms of muscular action.

Most importantly, the scientific results of Kirill Borisovich Tolpygo became a constituent part of condensed matter physics that is the basis of contemporary nanotechnologies. The condensed matter physics *per se* became so intertwined that it is practically impossible to follow all the connections between its branches. But working in the years 2000/02 on spin injection into microstructures I essentially used the experience that I acquired when working on transistor electronics under the guidance of K.B. Tolpygo in 1954/55. Moving from Kiev to Donetsk, further from the Party and scientific bureaucracy, provided Kirill Borisovich with wider opportunities in growing the new generation of researchers and to share with them his enthusiasm for teaching and science. I hope that my notes about Tolpygo's life and work in his Kiev period, including his persistence in following the highest scientific and ethical standards and the unconditional support of young researchers, will inspire his followers to work with the same dedication even in hard times.

Kirill Borisovich knew by heart a lot of good poetry and played piano.

I learned about his death being already in the US.