CONCLUDING TALK AT THE WORKSHOP "QCD AT THE THRESHOLD OF THE FOURTH DECADE/IOFFEFEST"

Landau's Theoretical Minimum, Landau's Seminar, ITEP in the Beginning of the 1950's

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In this talk I would like to share with you recollections which refer mostly to the beginning of my professional career. They carry an imprint of the epoch long gone...

1 Landau's theoretical minimum

I start with the story of how I became Landau's student. I was a third year student at the Physics Department of Moscow University. My desire was to be enlisted in the theoretical group, and I managed to do so. The professors of the Physics Department were strong in the Marxist philosophy, but almost all were weak in physics. Especially bad was the situation in the theoretical group: all high class theoreticians — Landau, Tamm, Leontovich and others — had been expelled. I was dissatisfied by the quality of education at the Physics Department, but was in doubts, whether my abilities were enough to become Landau's student. Finally, I collected all my courage and in the summer of 1947 I made a decisive step: I called Landau and asked him whether I could become his student. He invited me to come the next day. This was an entrance examination on mathematics, which I passed easily. Landau gave me the programs of eight courses on theoretical physics. Besides, there was one more examination on mathematics — complex variables, special functions, the Laplace transformation, etc. By that time only a few books of the famous Landau course were published: Mechanics, Classical Field Theory, The Theory of Continuous Media and the first (classical) part of Statistical Physics. One had to study all other courses by reading various books and original papers. For example, let me display the list of books/papers which we were supposed to study for the Quantum Mechanics course:

Quantum Mechanics

1. Blokhintsev, *Introduction to Quantum Mechanics* (in Russian), Chs. 3–14, 17–22, 24;
2. Kronig, *Striped Spectra and Molecular Structure* (in Russian);
3. Rosenthal and Murphy, *Rev. Mod. Phys.* **8**, 317 (1936);
4. Bethe, *Ann. der Phys.* **3**, 133 (1929);
5. Pauli, *Hdb. der Phys.* XXIV-2, II, 2, 12;
6. Brillouin, *Quantum Statistics* (in Russian) §124;
8. Bethe, *Ann. der Phys.* **5**, 325 (1930);
9. Mott and Massey, *The Theory of Atomic Collisions* (in Russian), Chs. 2 and 5;
10. Landau, *Sov. Phys.* **1**, 68 (1932), **2**, 46 (1932);
11. Bethe and Peierls, *Proc. Roy. Soc.* **A 148**, 146 (1935);
12. Breit and Wigner, *Phys. Rev.* **49**, 519 (1936).

The papers were either in English or German and some of them were very long (e.g., each of Bethe's papers was about 100 pages). Therefore, it was implicitly assumed, that the student-to-be knew well both foreign languages, which was very unusual at that time.

The examination proceeded as follows. An aspiring student would call Landau and say: I would like to pass an exam on such and such course (the order was more or less arbitrary).

— OK, please come on a certain day and time.

When students arrived at Landau's apartment, Landau would ask them to leave all their books, notes, etc. in the garderobe and invite them into a small room with a round table, with a few pages of blank paper on it, and nothing else. Then Landau would formulate a problem and leave, but every 15 to 20 minutes he would reappear and look over one's shoulder.

If he was silent, then this was a good sign, but sometimes he would say "hmm" — this was a bad sign. I have no failed examination experience of my own. However, once, when I was passing statistical physics, I started solving a problem in a way that Landau did not expect. Landau came, looked and said: "hmm." Then he left. In 20 minutes he came back, looked again and said "hmm" in an even more dissatisfied tone. At that moment Evgeny Lifshitz appeared, who also looked at my notes and shouted: "Dau, do not waste time, throw him out!" But Dau replied: "Let us give him another 20 minutes." During this time I got the answer and it was correct! Dau looked at the answer, looked again at my calculations and agreed, that I was right. After that, he and Lifshitz asked a few easy questions, and the exam was over.

The problems given by Landau in the examinations were sometimes very complicated and the student had to solve each of them in about an hour. (As a rule, they would get 2 or 3 problems in the examination session). So one had to have a lot of practice in advance. In order to get experience, I tried to find problems wherever I could. I asked Abrikosov, who had passed Landau's minimum before me, what problems he got (but not their solutions!) and solved them. After a few examination sessions I realized that Landau had only a limited number of problems — sometimes he would give me the same problem which he had given to Abrikosov. I gathered that Landau understood that his students would inform each other as to what problems had been given, but that did not worry him: to estimate the student's ability it was enough for him to observe the process of the solution. Here I will give you an example of Landau's problems – the one from macroscopic electrodynamics. A dielectric sphere with the electric and magnetic susceptibilities ε_1 and μ_1 is rotating with angular frequency ω in a constant electric field $\vec{E}$ in a medium, characterized by the parameters ε_2 and μ_2. The angle between the rotation axis and the direction of $\vec{E}$ is α. Find the electric and magnetic fields inside the sphere and in the medium.

It took me almost two years to pass Landau's minimum. (During the same two years, I did two scientific works under Pomeranchuk's supervision). In June 1949, after the last examination, Landau officially recognized me as his scholar and included my name in the list.

2 Landau's seminar

To be Landau's disciple implied no privileges, only obligations. That's because anybody could have scientific discussions with Landau and get his advice. Moreover, only a few among those who passed Landau's minimum became his graduate students (I did not). Landau's students enjoyed full rights as participants of Landau's seminar. But, again, anyone could participate in his seminar, ask questions and make remarks. The obligations of the "full-right" participants were to prepare, in a regular way, in alphabetical order, review talks for the seminar. After each seminar Landau would take a recent issue of Physical Review (at that time it was not divided into sections) and point out to a speaker-to-be which papers he was supposed to report on at the seminar. As a rule, he would choose a dozen such papers from all branches of physics. Mostly, they were experimental or part theoretical-part experimental. Sometimes, it could also be short theoretical papers, such as Letters to the Editor, etc. The speaker not only had to review the paper, i.e. present its basic idea and final results, but was supposed to understand well how the results were obtained, present and explain to the audience all necessary formulae, including experimental techniques, and have his own opinion, as to whether or not the results were reliable. In short, the speaker was almost as much responsible for the reported paper (and for errors in it!) as if he were the author. As I have already mentioned, the subjects of these papers were quite varied — from particle and nuclear physics to properties of metals and liquids. Landau's special love was the properties of alums. Landau knew well all subjects (despite the fact that he almost did not read papers, only listened to their presentations) and put questions which had to be immediately and definitely answered — general words or statements like "the author claims ..." were not accepted. In the audience there were always specialists on any subject, and they also put questions and made remarks. Therefore, it was a hard task to present such a talk. (Luckily, this would happen once or twice a year). Sometimes, when Landau was dissatisfied with the presentation of a paper, he would stop the speaker and ask him/her to go to the next issue. If such an event occurred two or three times during a given report, Landau would say: "You did not prepare your lesson! Who is the next speaker?"

In the worst cases, when the speaker failed a few times, he was ostracized — excluded from the list of the seminar participants, and Landau would refuse to have discussions with him, but, of course, he (the ostracized person) could attend seminars. (I remember two such cases — in one case the speaker was a famous physicist, V.G. Levich, who eventually became a Member of the Academy of Sciences). Only after a long time, a year or more, and after being advocated by the most respected seminar participants, could such a person be pardoned by Landau.

The presentation of a theoretical report would proceed differently. A person, who wanted to present a theoretical investigation at the seminar (his own or from the literature) was first supposed to tell the story to Landau privately. If Landau agreed with the basic points of the work, then the talk at the seminar would be allowed. During the talk, Landau gave clarifying comments and quite often his explanation of the work was strongly different from that of the author. A hot discussion would then often follow. One could hear from Landau: "The author, in fact, did not understand what he did." Landau's understanding in all cases was quite original and for normal people it was not easy to follow his line of reasoning. For me (and not only for me) it would require a few hours (sometimes, a few days) before I could understand how deep his remarks were, which often would turn the problem upside down and shed light on it from a different side. Theoretical talks freed a speaker from obligatory presentations of Physical Review papers; therefore, it was a serious stimulus to present theoretical talks at Landau seminars. (Pomeranchuk, for example, never did reviews, since he always presented theoretical talks). Sometimes, speakers from the outside, who were not from Landau's school, presented theoretical talks. Up until 1955 no foreign physicists visited Moscow.

So, by outside theoreticians, I mean the ones from FIAN, Mathematical Institute and Moscow University (Bogolyubov, Gelfand), as well as from Leningrad and Kharkov.

This was the normal routine of Landau's seminar at the end of the 1940's and in the beginning of the 1950's. There were exceptional persons, however: Ginzburg and Migdal. Once upon a time Landau said about Ginzburg: "Ginzburg is not my disciple — he just jumped onto the bandwagon." Indeed, Ginzburg came from Tamm's school, but was a very active participant of Landau's seminar. He did not follow the standard seminar routine with presentations of review talks, etc. Each time he arrived, he was full of new facts and ideas and presented them with brilliance and sharp wit. I vividly remember his impressive talk on supernovae, with a historical introduction on their observations in ancient Babylonia, Egypt and China. It was no accident that the famous phenomenological theory of superconductivity, the predecessor of many modern models of spontaneous symmetry breaking, was done by Ginzburg and Landau.

Another exceptional person was Migdal. His name is absent in the list of Landau's disciples, written by Landau himself: he did not pass Landau's minimum, but he was a full-fledged participant of the seminar. It was only Migdal whom Landau allowed to be late to the seminar and, nevertheless, enter the hall through the front door. As a rule, the seminars would start just in time, up to a minute. But sometimes Landau would say: "Let us wait for five minutes — these are Migdal's five minutes." One day, in the middle of the seminar, the front door of the hall opened and a person wearing a fireman's helmet and jacket appeared. "Get out! Leave the hall — we will perform here anti-fire exercises!" — exclaimed the man in a severe tone of voice. Lifshitz jumped up and shouted: "We have a seminar here every Thursday! You have no right!"

"Get out!" – repeated the man inexorably. People started standing up and moving to the doors. Then the man took off his helmet and the thread, which held his nose up — it was Migdal!

Another good joke was a letter from Pauli, which Landau had received through Pontecorvo. It was in 1958. At that time Landau was enthusiastic about Heisenberg's recent papers, where a universal nonlinear fermion theory was suggested. In a short letter, which Landau read at the seminar, Pauli claimed that he had found new arguments in favor of Heisenberg's theory and, moreover , there were new experimental facts supporting it. The facts, however, were not mentioned explicitly, and there was only a hint about their origin. The majority of the seminar participants became very excited. Somebody even went to the blackboard and tried to imagine which experiments they could be. Meanwhile, Migdal took the letter, read it carefully and said: "Please, look. If you read the first letters in each line, you will find the Russian word "duraki" (fools); what could that mean?"

In 1950–1951 the first experimental data on the pion production in pp collisions appeared. Because of the low energies available, the data referred only to the threshold region. Migdal had immediately formulated the theory of this phenomena: he had demonstrated that pn interaction in the final state dominated here and proved that this interaction reduced to pn scattering phase in the S-wave. He had also calculated the ratio $\sigma(pp \to pn\pi^+)/\sigma(pp \to pd\pi^+)$, which was in good agreement with the data. Migdal presented a talk about his results at Landau's seminar, which was met with great enthusiasm. However, he had failed to publish this paper. In Kurchatov Institute (Laboratory No.2), where he worked, the paper was classified and its publication forbidden. In the US the same results were obtained by K.M. Watson (Phys. Rev. **88** (1952) 1163) a year later and were called "the Watson effect." Migdal managed to publish his paper only in 1955 (JETP **28** (1955) 10). In ITEP, the papers on related subjects, π^- capture in hydrogen and deuterium, photoproduction of pions on deuterium, etc., were not classified and were published thanks to Alikhanov.

The third exceptional person was Pomeranchuk. I will dwell on him later.

3 My senior thesis under Pomeranchuk's supervision

Now I return to 1947. I was a student in the theoretical group for a very short time — about a month. Then an order from the Dean's Office came, and I and a few of my friends were transferred to a Department called "The Structure of Matter." This name was a camouflage: in fact, this meant nuclear physics. My friends (David Kirzhnitz among them) and I did not want to go into this field. For a month we tried to resist: we visited the Dean of the Physics Department, a few times, arguing in various ways. But the order was strong, and we had to submit to force. Only later, after a year or two, I realized that, in fact, this was my luck. Just because of this transfer, I became what I am now: if I had stayed in the theoretical physics group, then, probably, I would have faded away. The Structure of Matter Chair belonged to I.M. Frank, the future Nobel Prize Laureate. Although the Chair was mostly experimental, a theoretical course was also offered there. Moreover, it was possible to do a theoretical diploma and take, as a supervisor, anybody involved in the atomic project. (The rule of the Theoretical Physics Chair was that a supervisor had to be from this Chair). I wanted to have a supervisor from Landau's school, and by chance, chose Pomeranchuk. (I did not know him before). I called him, introduced myself, and told him, that I am a student at the Physics Department, I am taking Landau's examinations and have already passed three of them: mechanics, classical field theory and math-II. Pomeranchuk invited me to come for a conversation. When I came, after a short conversation, Pomeranchuk agreed to supervise my diploma work. He said, however, that first I had to pass all of Landau's examinations. I believe that there were two motivations which prompted Pomeranchuk's decision. First, I was a student at the Physics Department of Moscow University during the time when Landau's name was notorious there, and the fact that a student would like to have him — Pomeranchuk — as a supervisor, was not trivial. And, secondly, in 1948 only a dozen persons passed Landau's minimum, and all of them were outstanding physicists (with the exception of the last two — Ter-Martirosyan and Abrikosov — who had passed the minimum just before me and had no time to manifest themselves). Later, Pomeranchuk told me that he was surprised by the way I was dressed: it was a very cold winter day of 1948, but I came very poorly dressed. In turn, I was surprised by the absence of furniture in Pomeranchuk's apartment: a bed covered by a soldier-type blanket, a table, a bookcase, and nothing else.

Thus, I was continuing my preparations for Landau's quantum mechanics examination.

Here is an episode, characterizing the levels of education at Moscow University and Landau's minimum. In the spring of 1948, the time came to pass the quantum mechanics examination at Moscow University. The lecturer was Blokhintsev, but I did not attend his lectures. I was busy studying quantum mechanics according to Landau's program, and at that time I estimated the level of my knowledge of the subject to be low: I had to work much more. One day, I met D. Shirkov , who was a student in the theory group. "I am going to pass Blokhintsev's quantum mechanics exam. Do you want to join me?" he asked.

"OK, I'll put Blokhintsev's book in my bag in order to look at it, just in case," I replied.

We passed this examination successfully, I got an A, and Shirkov a B. I managed to pass the same examination given by Landau only in September.

Since I was successfully progressing with my examinations, in the late autumn of 1948 Pomeranchuk formulated a problem for my senior thesis work: it was the calculation of neutron polarization in the scattering off nuclei due to interference of the Coulomb and nuclear scattering. The calculation was based on Schwinger's paper, but I had to invent some elements on my own. The second part of the work was the calculation of the neutron depolarization in the course of the neutron deceleration in the medium. This part was merely educational: I had to study the theory of neutron moderation as well as some aspects of the theory of nuclear reactors. During this winter, until March of 1949, I had finished my senior thesis and almost finished Landau's minimum (except for the last examination — the theory of continuous media, which I passed in June of 1949). So, Pomeranchuk gave me a new problem: to calculate the cross-section of e^+e- pair production on nuclei by linearly polarized γ-quanta and the related cross section of *bremsstrahlung* of polarized γ. At that time there was no Feynman diagram technique — the famous papers of Feynman were published at the end of 1949. Therefore, I did the calculations in the old Heitler technique with the account of the electron transitions to negative-energy states, using non-covariant Dirac matrices, etc. You can see how complicated the old technique was, looking at the original Bethe-Heitler paper, where the calculation of the electron *bremsstrahlung* was performed. (The calculation of polarized γ *bremsstrahlung* was not easier!) To give drastically different problems to students was typical of Pomeranchuk's (as well as Landau's) style: a student had to be able to solve problems in many (if not in all) fields of physics. Pomeranchuk recommended that I write two short papers: one on the neutron polarization and another on e^+e^- pair production by polarized photons. I did that, but, probably, he forgot about this, and I hesitated to remind him. Thus, these papers were not published. (They are being published in this book, in the English translation from the Russian original manuscripts.)

Later, in the 1950's, a few papers appeared in which calculations of e^+e^- pair production by polarized photons and electron *bremsstrahlung* of polarized photons were performed, using the Feynman technique. I felt sorry that I did not publish these papers.

In the spring of 1949 Pomeranchuk introduced me to A.I. Alikhanov, the Director of Laboratory No.3 (now ITEP), as a person whom he would like to take to the Theoretical Physics Division of Laboratory No. 3. Alikhanov had a custom — to have a prior conversation with any prospective new worker in the Laboratory. After a short conversation, Alikhanov signed a letter, requesting my assignment to Laboratory No. 3 after my graduation from Moscow University. This was an extraordinary case. The anti-Semitic campaign was in full swing, and I was the *only* Jewish student from the whole Physics Department who got an appointment in Moscow, and in a good place. All others were sent far away (for instance, my friend Kirzhnitz was sent to a factory in Gorky), or got no jobs at all.

4 ITEP in the 1950's

On January 1, 1950 (a symbolic date — the beginning of the second half of the 20^{th} century!) I started my work in the Laboratory of Theoretical Physics of ITEP. The Head of the Laboratory was Pomeranchuk. In the beginning, Pomeranchuk "lent" me to the ITEP Vice-Director V. Vladimirsky. I had to calculate electric fields in the electron linear accelerator, which he wanted to construct. I did not like this job: I had no idea on how to calculate the electric field for complicated configurations of electrodes, and this work seemed to me to be very gloomy. So, instead of doing it, I read the papers of Feynman, Schwinger and Dyson that had just appeared (I translated some of them to Russian, and the translations were published in Russian review journals).[a]

I wanted to educate myself in the new approach to quantum electrodynamics (the Feynman diagram technique, renormalizations, etc.). At that time nobody in Moscow was proficient in these new QED methods, and only a few people (Galanin, Abrikosov, Khalatnikov, maybe, somebody else) learned them. Such a situation — neglecting my job in favor of Feynman, Schwinger and Dyson — could not continue for a long time and was destined to end in a scandal. But I was "lucky" again. An order came from the highest level (probably, from Beria, or, maybe, even from Stalin himself) — the Institute was supposed to present in the shortest time — within a few weeks — a project of the heavy-water nuclear reactor on enriched uranium, for tritium production. All theoreticians, including myself, were mobilized to do the physical design of this reactor. I was returned under Pomeranchuk's guidance, and starting from this time (the spring of 1950) I worked for many years in parallel on elementary particle physics and on nuclear reactor design.

There were three principles which Pomeranchuk put in the basis of the work of Theoretical Laboratory:

1. "The Directorate must be respected." This meant that all problems formulated for theoreticians by the Institute Management and devoted to applied physics, such as nuclear reactor design, had to be solved with priority and full responsibility; any errors had to be completely ruled out.

2. "The experimentalists must be respected." This meant that if an experimentalist came with a question to our Theoretical division, or asked for help, the question had to be answered, and assistance provided, even if this required a complicated calculation.

3. "You may do science from 8 p.m. till 12 p.m." This meant that young

[a]It should be mentioned that getting American physics journals in Moscow was a problem at that time: they would often come with great delay, and sometimes they were stamped "classified." We knew that they were delivered illegally, through Sweden.

people, even if they were busy doing their jobs, according to the points 1 and 2 above, had to find time for *the* science (i.e. purely theoretical work).

In one particular aspect Pomeranchuk and I had something in common — we had a common hobby, reading newspapers. At that time almost nobody read newspapers regularly — there was no information there. All newspapers were filled with articles that would start with words like "New successes in the production of ... are achieved at the factory ... " or "The worker Ivanov (Petrov, etc.) in one shift produced ..." In order to get information from newspapers one had to be a professional in this business, and we — Chuk and me — were. (From now on I will call him Chuk, as many did.) In the morning, just after arriving at the ITEP, Chuk would come into my office and ask:

— " Have you read *Pravda* today?"

— " Yes," I would reply.

— "And what did you pay attention to?"

— " To a small article on the third page."

— "Oh!" and Chuk would raise his forefinger.

— "And you?"

— " Probably, the same, the report on the meeting of the Voronezh District Party Committee."

— "Yes."

— "And what was interesting for you in this report?"

— "The greeting to the Politbureau."

— "Oh!" And Chuk's forefinger would go up up again. "What concretely?"

— "The ordering in which the names of the Politbureau members were listed."

We understood each other well. From this ordering one could obtain an idea of who was going up or down in the Politbureau, and estimate political trends.

In 1950 all members of the ITEP Theoretical Physics Laboratory — V. Berestetsky, A. Galanin, A. Rudik and myself — were intensively studying new methods in QED. Pomeranchuk strongly supported this activity, but in1950 and in the first half of 1951 he did not participate in it too much himself: he was busy with other problems — in 1950/51 he was sent for half a year to Arzamas-16, to work on the hydrogen bomb project. Landau was sceptical of the new trends in QED. He did not believe that the problems of infinities in quantum field theory could be circumvented by the mass and charge renormalization. Two attempts to present Feynman's papers at Landau's seminar failed: the speakers were thrown off the podium after 20 or 30 minutes of talking. Only the third attempt succeeded (if I remember correctly, this was in 1951 or even in

1952). But still he had no interest in these problems: the dominating subjects on his seminars were what we called "alums."

Landau called me a "snob." He repeated this even in public: "Boris is snob!" The meaning of his words was that I did not want to solve real physical problems and, instead, preferred to study a refined theory. His words had no influence on Pomeranchuk, because we were allies, but — what was the worst — he said the same thing to Alikhanov, the ITEP Director. And for Alikhanov, Landau's evaluation of anyone in theoretical physics, had the highest weight. So, Landau's words could have resulted in undesirable consequences for me. Fortunately in this case Alikhanov already had his own opinion. He knew very well that I was performing calculations of nuclear reactors for him (as well as calculations of his experimental set-up) and by no means thought I was a snob.

5 Pomerancuk's seminar

Pomeranchuk tried many times to convince Landau to shift his interests to QED and mesonic theories. Once in a while, he would repeat:

"Dau, there are a lot of problems here. They are hard, but they are just for a person of your class!"

In response, Dau would say:

"I know my abilities — to solve the problems of infinities in field theory is above them."

In fact, contrary to common belief, Landau was very modest in his self-evaluation. He underestimated rather than overestimated his abilities and achievements. Experimental facts in particle physics were always reported on Landau's seminar, but theoretical papers were reported only as an exception.

Then Pomeranchuk decided to organize a separate theoretical seminar devoted to quantum field theory and particle physics. The seminars could not proceed at ITEP, since all participants had to have permission to enter ITEP territory, and by no means did everyone have such a permission. Therefore, Pomeranchuk made an arrangement with Landau that the seminar will proceed at the Institute of Physical Problems, on the same day of the week, Thursday, as Landau's seminar, but two hours earlier. Pomeranchuk nominated me as the seminar secretary. The first meeting took place on October 1, 1951. I reported on the famous paper of Dyson at this meeting. Alikhanov, as the ITEP Director, asked me to present to him an official letter regarding the creation of a new seminar, and I did this. (This document still exists). Almost all famous theorists participated in the seminar. The main papers on quantum field theory were reviewed followed, as a rule, by heated debates. Sometimes Landau would peep in the hall from the door. Chuk would invite him: "Please come in, we are discussing this and that". But Landau would only condescendingly smile: "If young people want to spend time on nothing, then let them do it". With time the number of participants of Pomeranchuk's seminar was increasing, as well as the enthusiasm and excitement around the problems under discussion. This excitement eventually spilled over into Landau's seminar which, as I already mentioned, used to start just after Pomeranchuk's seminar. Then Landau decided: his seminar had to precede, not follow Pomeranchuk's. In 1953, when the restrictions regarding the admission of outsiders to ITEP were somewhat relaxed, the seminar was transferred to ITEP. It still exists, to this day, every Monday, at 3.30 p.m. (except holidays), the doors of the main ITEP conference hall open for the ITEP theoretical seminar.

6 The history of the making of Landau, Abrikosov and Khalatnikov's papers

Aleksei Galanin and myself educated ourselves in the calculation of radiative corrections in QED and meson theories, and in performing the mass and charge renormalization — first, at lowest order, then at higher orders. I succeeded in writing an exact infinite system of coupled equations for the Green's functions in the meson theory. In the paper by Galanin, Pomeranchuk and myself the mass and charge renormalization was performed in this system of coupled equations. The solution of this system of equations was shown to have no infinities after renormalization, it had to be finite. However, we did not succeed in solving this system in a recurrent way by cutting it off at some fixed number of equations. After the cut-off the infinities reappeared; in order to get rid of them, one had to sum the whole infinite series.

Upon calculating several first-order corrections in perturbation theory Galanin and myself realized that large logarithms of the type $\ln(p^2/m^2)$ appear in the polarization operators and vertex function far off-shell, $p^2 \gg m^2$. In the first order one deals with $ln(p^2/m^2)$, in the second order with terms proportional to $[\ln(p^2/m^2)]^2$, in the third order $[\ln(p^2/m^2)]^3$, and so on. In this aspect the paper by S.F. Edwards (Phys. Rev. **90** (1953) 284) was important for us. Edwards considered the equation for the vertex function in the ladder approximation and demonstrated that at the n-th order the terms $\sim (e^2 \ln p^2/m^2)^n$ appear.

In the 1950's Landau visited ITEP every Wednesday. He participated — very actively — in the ITEP Wednesday experimental seminars, steered by Alikhanov. After the seminars he used to come to the theorists office, for discussions which would normally go on for 1 or 2 hours. In these discussions we explained to Landau the situation with the radiative corrections and he came up with the idea to sum up the leading logarithmic terms, i.e. the terms $\sim (e^2 \ln p^2/m^2)^n$ in QED. Initially, when Landau formulated his idea, he believed that he would find in QED what is now called asymptotic freedom. These expectations were formulated in the first papers by Landau, Abrikosov and Khalatnikov which had been sent for publication before the final result was obtained. At one of the following Wednesday visits Landau showed us their result, confirming his expectation: the effective charge in QED was decreasing with energy. Galanin and myself decided to check their calculation, because we had a desire to use this idea in our coupled system of the renormalized equations. (We did this later, in collaboration with Pomeranchuk). But the first-loop calculation demonstrated the opposite behavior. The effective charge was increasing with energy! Next Wednesday we told Landau about this and convinced him that we were right. Landau, Abrikosov and Khalatnikov's paper which was already prepared for publication, had a sign error, drastically changing the final conclusion. S.S. Gershtein who worked at the Institute of Physical Problems at that time, wrote later in his memoir that upon returning from ITEP, Landau said:

— "Galanin and Ioffe saved me from shame."

After the publication of Landau, Abrikosov and Khalatnikov's papers, in approximately one year, Landau got a letter from Pauli. In this letter Pauli informed him that his graduate student, Walter Thirring, had found an example of a theory, in which there was no zero charge problem — the theory of the scalar meson-nucleon interaction. The manuscript of Thirring's paper was attached to the letter. Dau gave this paper to Chuk, and Chuk asked me to check the paper. I studied Thirring's paper and came to the conclusion that it was wrong. The origin of the mistake was that the Ward identity arising from differentiation over the nucleon mass, was exploited, which in fact was violated by renormalization. I told Chuk about this.

— "You should write a letter to Pauli" — was Chuk's response.

I hesitated: to write to Pauli, that his graduate student had made a mistake and he, Pauli, had overlooked it! Chuk insisted and, finally, I wrote a letter ending up with the signature: "Respectfully yours ..." The answer I received was not from Pauli, but from Thirring. He accepted his error, and Thirring's paper was never published.

7 Papers on P, C, T non-conservation

Now I would like to tell you about another episode which adds important touches to Landau's portrait.

In 1955-56 the $\theta - \tau$ puzzle agitated all physicists. The K-meson decays in 2 and 3 pions had been observed experimentally. Under the condition of parity conservation, which was taken for granted at that time, one and the same kaon could not decay in 2 and 3 pions simultaneously. For this reason most physicists believed that θ and τ were two different mesons. As the precision of experiments grew, however, it became clear that their masses coincided. At that time, in the spring of 1956, Lee and Yang came up with their revolutionary paper in which they put forward a parity non-conservation hypothesis which explained the $\theta - \tau$ puzzle. Moreover, Lee and Yang calculated parity non-conservation effects in the β decay and in the $\pi \to \mu \to e$ cascade.

Landau vigorously rejected the possibility of parity non-conservation, saying "space cannot be asymmetric!" Pomeranchuk preferred the hypothesis of parity-degenerate doublets of strange particles. A.P. Rudik and I decided to calculate some additional effects based on the assumption of parity non-conservation in weak interactions, other than those considered by Lee and Yang. We decided to examine $\beta - \gamma$ correlations. I made an estimate and found that the corresponding effect had to be large. Rudik turned to detailed calculations. In some time he came to me and said:

"Look, Boris, the effect vanishes!"

"This cannot be the case," I replied.

We began trying to make sense of this result. I observed that Rudik, being a well-educated theorist, had imposed the condition of C invariance on weak interaction Lagrangian. As a result, the coupling constants in front of the parity-nonconserving terms turned out to be purely imaginary. The constants in Lee and Yang's papers were arbitrary complex numbers. (If one assumes them to be purely imaginary, then all parity-nonconserving effects disappear.)

A question arose as to the connection between C and P invariance. I discussed this problem with Volodya Sudakov; an earlier paper by Pauli surfaced in this conversation. Although I had read this paper previously, I had forgotten about it. In part, the reason was that Landau regarded this paper with scepticism — he believed that the CPT theorem was a trivial relation satisfied for any Lagrangian and, for this reason, no physical consequences could follow from the CPT theorem. I noted that Lee and Yang's paper did not mention the CPT theorem at all, and nothing was said on the connection between C, P and T invariance. I read Pauli's paper again, more attentively than the first time, and it became clear to me immediately that if P were violated, then either C or T, or both had to be violated with certainty.

This observation gave rise to the following idea: two K^0 mesons with drastically different lifetimes may appear only provided one of the invariances, C or T, takes place, at least approximately. Rudik and I considered a number of effects and observed that P-odd pair correlations of spin and momentum

($\sim \vec{\sigma}\vec{p}$) appear if C is violated and T is conserved. In the opposite case they are absent. (In my subsequent paper I proved this theorem in a general form, and found the type of P-odd terms corresponding to T violation.) We wrote a paper and told its contents to L.B. Okun. Okun made a very useful remark that analogous effects which unambiguously differentiate models with C invariance from those with T invariance, appear in K^0 decays into pions too. We included this remark in the paper and I suggested to Okun to become a co-author. At first he refused, saying that such a remark deserves a mention in the acknowledgments, but later I persuaded him.

After that I reported our results to Pomeranchuk. Pomeranchuk decided that we had to tell them to Dau — immediately, next Wednesday. On Wednesday, Dau's first reaction was to refuse to listen.

"I do not want to hear anything about parity nonconservation. This is nonsense!"

Chuk persuaded him:

"Dau, have patience for about 15 minutes, listen to what young people have to say."

With heavy heart Dau agreed. I spoke not for long, perhaps, for half an hour. Dau kept silent, and then went away. Next day in the morning Pomeranchuk called me: Dau solved the parity non-conservation problem! We were supposed to come to him immediately.

By that time both of Landau's papers — on the conservation of the combined (CP) parity and on two-component neutrinos, with all formulations, were already ready.

Our paper and that of Landau were sent for publication prior to experiments of Wu *et al.*, where the electron asymmetry in polarized nucleus decay was observed (i.e. the correlation between nucleus' spin and electron's momentum). In this way the parity non-conservation was discovered. Our results then implied that the C parity was not conserved in the β decay either. The corresponding note was added in proof in our paper. An analogous statement was made in the paper by Wu *et al.*, who referred to the paper by Lee, Oheme and Yang, which, in turn, was published *after* our paper. In their Nobel lectures Lee and Yang emphasized our priority in this problem.

Landau considered the CP conservation to be the exact law of nature; he did not admit the possibility of its violation. Concerning CP, Landau would say exactly the same words on the space asymmetry as he used to say previously with regards to P violation. I constructed an example of the Lagrangian in which CP was violated, and nothing bad happened to the vacuum, and tried to change Landau's mind, but he did not want to listen.