

## My meetings with Yuri L. Klimontovich

I first got acquainted with Yuri L. Klimontovich in mid 1970s in the Moscow State University (MSU), where he worked practically all his life – from 1955 until his death on 27 November, 2002. Just in those years, I started to deal with the problems of kinetic theory of plasma systems with strong Coulomb interaction between particles, and I had some unclear and crucial questions to discuss with theoreticians having wide experience and essential achievements in kinetic theory. In fact, I had heard about YLK before, in particular – from V.P. Silin and from my Candidate thesis' supervisor A.A. Rukhadze who were very active and very successful in those years dealing with the theory of plasma in the Physical Institute of Russian Academy of Sciences (FIAN), both in fundamental and applied aspects. As a matter of fact, I addressed YLK on Rukhadze's advice, since the problems I was interested in were better correlated with the style and the trends of YLK's research, rather than with the works on theory of plasma performed in FIAN at that time. By the way, in those years V.P. Silin strongly believed that theoreticians should not investigate the systems without small parameters or with unknown ground state, though in reality, their number is by no means less than the ones with a small parameter. And if problems exist, they need to be solved.

Undoubtedly, I also met YLK at the Physical Faculty of MSU, where I studied, but I did not know him personally. As for YLK, both at that time and later on, he focused on the development of fundamental problems of statistical theory and kinetics of many-particle systems, not only plasma physics in general. By that time, he had already become widely known, first of all, as author of a new method in kinetic theory of non-ideal gases and plasma, based on the exact equation, introduced and investigated by YLK for microscopic density in the six-dimensional phase space. Later on the method and the equation were called by his name.

As early as our first meeting with YLK, I noticed some of his features, which, as I got convinced later, were typical of him when discussing scientific problems. Having made an appointment, he was waiting for his interlocutor and he never made people wait for himself, even having a discussion with a person who had come earlier. Furthermore, he was never late for an appointment. However in essence his pieces of advice were always very laconic. He would set out his opinion on an issue and then immediately go on developing the problem, passing to the tasks which were of major interest to him at the moment. Then he would start to discuss them in detail as if changing places with his interlocutor and wishing to hear the opinion and objections of the person who came to him for advice. I think this was not due to lack of interest to his counterpart's problems, but first of all because of his extraordinary excitement

with his scientific ideas, with which he was always overwhelmed and which he was developing until the last days of his life.

Later on, we met many times in Moscow (both in MSU and at his home) and abroad. Practically always these meetings, as far as their scientific part is concerned, went under the scenario described above. As after a number of years our relations became even more close, during our meetings at YLK's home, we spent a part of the time in the kitchen, where, by a good Moscow tradition, over a cup of tea or a glass of wine, we went beyond purely scientific or "near-scientific" problems. YLK's attitude to any opinion, which was distinctly contrary to his one, was very peaceful, but during our conversations, I can never remember him change his own point of view. I think, this dealt with the fact that he, as a rule, was not simply discussing a problem trying to find a correct opinion during the discussion, but he always had his own, well-considered beforehand and mature view almost on all the problems discussed. And this concerned not only scientific problems, but all the other issues as well. In this connection, I recall a big International conference on Statistical Physics held in Berlin in 1992. This conference, which – if I am not mistaken – took place in the Technical University, was attended by hundreds of specialists, in particular there were a lot of physicists from Russia. Among them, there were many Russian scientists (as well as scientists from CIS, which had already been formed by that time), who had gone abroad in search of both quiet conditions for work and (there is no need to make a secret of that) more stable and secure life as compared to the one provided to them in the severe period of "perestroika", which is still going on.

I arrived at the conference from The Netherlands, where I was working on a grant in Eindhoven Technical University (TUE) in the group of my colleague and friend Pieter Schram who knew YLK personally and had a deep respect towards him. In particular, P.P.J.M. Schram promoted publication of YLK's monograph "Statistical Theory of Open Systems" in the Dutch publishing house "Kluwer Academic Publishing", where it was issued before it was published in Russia. As my report was presented on that conference on behalf of the Eindhoven University, it was written "TUE" on the badge under my surname, not "Institute for High Temperatures, Russian Academy of Sciences", which was the place of my regular work. It was a real surprise to me because my German colleagues knew the place of my regular work very well. Probably, in the appropriate column of the conference application form, I had indicated "TUE". I myself did not think it was so essential, but for Russian scientists, the problem of working abroad was rather important and actively discussed. At first, during my conversation with I.M. Khalatnikov (I cannot remember the main subject of this talk, because he had never been engaged in plasma physics), he started to make inquiries about my work and, by learning that I was temporarily in The Netherlands, he noted that it was wrong to reflect the place of non-permanent work on the badge, since Russian science was not duly propagandized. As for him, he had already been working abroad for a long time, but probably considered it important to support Russian Institutes' reputation. A more serious conversation, cast by this badge, I had later with YLK. We were going by bus to a banquet on Wansee, and our seats were nearby. He started talking about departure of many

Russian scientists to the West for scientific research. “And here you are as well”, he said. I explained to him that I had left temporarily, though I mentioned that, in my opinion, if there is a choice, scientists have work in the place where the best conditions are created for them. He argued that for the youth studying in MSU and in other high schools, as well as for post-graduate students, it was very important to have strong supervisors and a powerful scientific community in general, not somewhere far away, but nearby. But this community in Russia had been considerably weakened and it continued to decay. YLK said in particular, “Well, you see, Khalatnikov has left, and he is, by the way, the Institute Director”. As I understood it was the problem of Russian scientific potential weakening that deeply disturbed YLK. Though YLK worked abroad occasionally, he never left the country for long, completely following his understanding of the problem.

Here it should be said that material living conditions were of little interest to YLK. This seemed merely beyond the sphere of his attention, and he just could not afford wasting his time on such inessential things. One day I came to his home and saw that parquet in his study was inflated and in some places it was just ruined. He explained to me, that it had been flooded by water, but when I next time came to his place (it was not very soon), everything remained as it was.

Just here, it would also be appropriate to mention his very easy-tempered attitude to the Academy of Sciences Membership, which is immensely highly quoted in Russia and which brings huge moral satisfaction and (at this point – not so essential as before) material well-being as well as authority in scientific hierarchy, usually so important to scientists. I am certainly unaware of any of YLK’s internal motives. He probably balloted in RAS once or several times, but this issue was far beyond his interests. I think, as it was the case with material welfare issues, that he never wanted to spend his time on that: he realized, what efforts, nerves and sometimes moral concessions even the most talented scientists had to apply in order to participate in the struggle for a position in the Academy. By saying this, I am far from blaming the scientists who have done everything in their power to become members of the Academy of Sciences, especially if they have really deserved this. On the contrary, such aspirations seem quite natural to me. However, in this relation also, YLK seemed to perceive and comprehend everything very differently as compared to most people of science, even the people who were rather close to him, and he was guided by rather different, other than standard, motives. Probably, it was also related to his profound knowledge of science history, especially the history of statistical physics. Perhaps, he also lived in a kind of a historical dimension, which gave him an opportunity to look at the reality from a certain remote point and to interpret it differently, unlike most people tend to do it.

I had many meetings with YLK during the last years of his life. He used to come to Berlin at the invitation of his former disciple and big friend Werner Ebeling who was the head of Statistical Physics and Nonlinear Dynamics Department of Humboldt University (HU). In fact, YLK had many disciples and friends in the scientific community in Germany, where a number of universities have strong scientific groups on statistical physics, plasma physics and nonlinear phenomena. Such an activity in

these directions of scientific research in Germany was partly stimulated by contacts with Y.L. and by his participation. YLK's disciples and colleagues in Germany, most of whom have become well known scientists, treated him with invariable respect and loving care, and I would even say, they just deeply honored him. I could probably give the most precise description of German specialists' attitude towards YLK by saying that there was a cult of Klimontovich in Germany, in the best sense of this word. YLK was invited just to every conference on statistical physics or plasma physics held in Germany. It should also be noted that such conferences have been organized and held in Germany on a regular basis, especially by the scientific collectives of Rostock and Greifswald Universities, and they have actually become a major tradition, which contributes to progress in these fields of physics and leads to combining scientific efforts and different scientific schools and trends, first of all, those of Russia and Germany.

By attending these conferences I observed YLK's works development, however, it was impossible to closely communicate with him, since, he was always surrounded by a dense crowd of his German disciples and colleagues. Unfortunately, in my opinion, this attitude somewhat contrasted with YLK's work and significance underestimation by physicists in his home country, though he had been awarded with honorary prizes and ranks. It is difficult for me to make comments on that, I am unable to make a convincing analysis of the reasons. But to tell the truth, I doubt if this should be done. History in general and history of science in particular, are full of similar examples.

In recent years, my relations with YLK received further development. At that time I worked at Ebeling's department and already lived with my family in Berlin, which, as it is seen from the above-stated, contradicted YLK's principles regarding the problem of Russian scientists moving and working in the West. Nevertheless, my relations with YLK were better – and to be more exact, even warmer – than ever. This fully corresponded to his democratic nature, which recognized the right of each person, within the limits of all possible and legal things, to act in compliance with his or her own views and aspirations.

YLK used to arrive for some weeks at Humboldt University at Ebeling's invitations on repeated occasions, where there were all the possibilities for personal contacts with him. It should be said that he was very anxious to communicate and he was first of all looking forward to scientific contacts, which, as I believe, for the last several years in Russia were insufficient and not quite favorable for him. I will provide more details about that below.

He also visited our home in Berlin, and during those visits he told us Moscow scientific and non-scientific news, as always – gave interesting and original comments, and in fact, we were very pleased to communicate with him. Hopefully, he also enjoyed to stay with us.

I cannot avoid on a well-known discussion concerning YLK's recent works dealing with a certain fundamental problems of statistical physics and, in particular, Callen-Welton theorem. I don't think that some unproved and may be even mistakable hypotheses can belittle YLK's merits or depreciate him. For it is known that even

the grate of the Earth were mistaken: On the contrary, from my point of view, concealment of this discussion is unworthy of YLK's blessed memory, even though in this particular case his hypotheses, based on some physical arguments and examples, was possibly incorrect (as I suppose, basing on concrete examination of the particular section of his work). I would like to dwell on these events, as I perceive them now, since they played a considerable role in the last period of YLK's life.

Besides, it is essentially important to emphasize that YLK never presented his results in this field as something accomplished and unconditionally correct. He was looking forward to discussing his considerations and calculations with the theoreticians' community, – perhaps, this is the most precise definition of his efforts in this direction. However, this turned out to be very difficult and as a result, this was treated sometimes as his errors. As it is known, YLK doubted Callen-Welton quantum systems theorem and proposed to modify it. His work in that direction caused an explicitly negative response from some of the well-known theoreticians, first of all – from V.L. Ginzburg and L.P. Pitaevskii in Russia. He also had problems with publications of his results in the wide popular Russian physical journals. YLK was suffering about this discussion very much, he thought that publishing of even disputable scientific papers in the open press should not be restricted (certainly, keeping in mind quite qualified authors). Besides, YLK considered (and actually convinced me during one of his visits to Berlin) that the criticism of his statements was not enough motivated. This criticism rather denied the things, which contradicted the proved theorem, but it did not specify actual inaccuracies or miscalculations in YLK's work.

It was just this thing that distressed him, because again, his main goal was an objective discussion only. On the other hand, presentation of this hypothesis, made by YLK, at first seemed insufficiently transparent to me and, probably, it was just insufficient clearness and generality of the presentation that caused V.L. Ginzburg and L.P. Pitaevskii not to substantially analyze YLK's argumentation. It should also be said that the seminars, particularly in Germany, at which YLK presented his results, failed to add clearness to the problem. It is quite typical that nobody, as far as I know, was eager to go into the root of the new matter and to examine everything in detail. Apparently it was caused by the fact that the latest theoretical results (if they are not directly related to explanation of the experiments) stay – just by virtue of their novelty – away from the direct interests of theoreticians, working at their problems and economizing their time. Once, after a seminar in Berlin, I expressed to W. Ebeling my surprise with such apathy saying that I wanted to try to understand the results on basis of attentive consideration of some example, described by YLK. After several conversations with YLK, which failed to make the situation clear, I looked through all the discussion materials provided to me by YLK. There was a great deal of them. Both YLK's works and his responses to criticism were published in hardly accessible editions, in view of the above mentioned reasons. Some materials were not published at all, and YLK gave them to me in a hand-written form. I chose only one of the problems – it dealt with a possibility of violation of Callen-Welton theorem for alternating-current circuits. YLK had used quasi-static expression for conductivity, though to prove violation of Callen-Welton theorem, it was required

to perform integration on all the frequencies, including the high ones, where quasi-static approach is inapplicable. On the basis of this argument, I decided not to examine other examples as I felt negative to YLK's hypothesis on violation of this theorem as a whole. My attempts to discuss these arguments with Ebeling (by that time YLK had already departed to Moscow) met neither interest, nor understanding from his part. Perhaps, he did not have enough time to go in details of this objection, but in my opinion, he, most likely, was avoiding a possibility of finding himself in the position of a person denying any results received by YLK. Afterwards, I called YLK in Moscow. I tried to tactfully explain my doubts to YLK, by asking him about limitations for use of quasi-static approximation in his consideration. I think, he was already feeling bad at that time or he could be in low spirits. Perhaps, he himself had some critical questions concerning his hypothesis. Anyway, his answer was rather laconic, which made it impossible to pursue any further discussion. I remembered that answer just word for word: "Well, this is a special example". So we have never returned to that issue.

After that, YLK arrived in Berlin once again, but his health was already poor. That is why he visited Humboldt University rather rarely. He also did not come to our home, though he intended to visit us. Instead, I went to his place. He was staying in a rented apartment, located not far from HU. The main thing we spoke about, was his memoirs, which draft manuscript he brought to Berlin and gave W. Ebeling for reading. Then he handed it over to me asking me to make notes and to correct possible inaccuracies and misprints. There were very many of them, and afterwards I understood that YLK was in a hurry: he probably understood that very little time was left. Certainly, the material was very interesting: YLK had met with many famous physicists and besides, his understanding and perception of the situation was of major value. At the same time, the memoirs contained various scientific problems including too specific details for non-physicists. It was quite obvious that this could limit the circle of future readers. Therefore Ebeling right away suggested YLK to differentiate the memoirs and the specific scientific insertions by attributing the latter to appendices. YLK easily agreed to that proposal. As the manuscript gave, in particular, a detailed description of Vlasov's extraordinary role in kinetic theory development and contained a violent discussion on Landau damping, which Vlasov had missed in his pioneer work and afterwards denied that, our conversation with YLK mainly concerned this sensational story and everything that surrounded it many years ago. Within three hours, I tried "to worm" out of YLK any additional details of the events he witnessed while being a student of the Moscow State University (MSU) and later – N. Bogolyubov's post-graduate student. Being alone in this apartment, he felt lonely and besides, he was unwell. That is why he was glad to spend time on history and he really was inclined to tell everything in detail. Here I am not going to give any details of our conversation, but YLK considered that both the response on Vlasov's works and the form of criticism were intolerably harsh and they were mainly caused by personal attitude to Vlasov, rather than the subject of the scientific discussion. Vlasov did not deserve such an attitude, even though some of his actions and deeds were not quite correct – in short, that was YLK's

position. Hopefully, YLK's memoirs will be published. No doubts, they represent a major interest not only to physicists.

That was the last meeting with YLK. Several months later, he died. In my opinion, in the course of time, appreciation of YLK's huge contribution in the kinetic theory of non-ideal systems and the theory of open systems will only increase. His moral character and devotion to science represent a rare example, which the senior physicists' generation can envy. As for the younger generation, this example is not too late to follow.

S.Trigger

