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LEV DAVIDOVICH LANDAU

(on his 60th birthday)

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THE remarkable Soviet physicist Lev Davidovich Landau celebrated his sixtieth birthday on 22 January 1968.

The name Landau is among the best known of our contemporary physicists. There are many reasons for it: the greatest accomplishments in various fields of physics, the popularity of the books written by him (particularly the many-volume course of theoretical physics written together with E. M. Lifshitz, which has been partly or fully translated into ten languages), a brilliant individuality and a unique talent for polemics, and the tragic history of the automobile accident and the subsequent struggle for his life.

Landau's articles and books do not gather dust on the shelves - they live and work among us and for us. Landau's biography is also well known, and it is hardly necessary to repeat it here, both from the scientific and from the personal point of view. At the end of this article I present a list of works in which Landau's biography is treated rather in detail. At the same time, it can be assumed that many physicists, particularly the young ones, would like to understand better what is it that distinguishes L. D. Landau and why his students and colleagues speak about him in some "extraordinary" manner. Unfortunately, it is much easier to raise such a question than to answer it, and I am therefore capable of making a few remarks. It goes without saying that I can express only my own opinion, although for brevity I shall omit the usual phrase "it seems to me."

ЛЕВ ДАВИДОВИЧ
ЛАНДАУ

Scientific trends and specialites differ in essentially two attributes. In the former, the subdivision is based on the object of the research. Magnetism, optics, semiconductor physics, nuclear physics - these serve simultaneously both as names of fields of physics and names of specialities. In the second case the subdivision into specialities is dictated more by the approach to the problem or by the research method; vis., physics of oscillations, radioastronomy, theoretical physics.

Any classification is to a considerable degree arbitrary, and different scientific trends are not separated from one another by some wall. But nevertheless one can see that for specialities that are separated by the "methodological" attribute, the form is determined to a considerable degree by those who shape this specialty, and is therefore particularly significant. At the same time, the methodological subdivision is more ephemeral and unstable than the subdivision based on the "essence" of the subject. A good example is the theory of oscillations, which has by now has practically wholly incorporated in optics, radiophysics, and acoustics. The common "oscillational" ideas, which are so brightly reflected in the works of Lord Rayleigh and L. I. Mandel'shtam, are now practically ingrained in our present-day physicists, and have assumed a general educational significance. The situation with theoretical physics, of course, is more complicated. An analysis of results of experiments and observations, both qualitative and quantitative (mathematical), has always been going on in physics to one degree or another. In modern language, Newton, Maxwell, and many others, were theoretical physicists. But they were not only theoretical physicists alone, and were more readily universal physicists. Only with the colossal increase of the volume of knowledge in the 19th century did the "division of labor" become clearly pronounced in physics, and modern theoretical physics, and modern theoretical physics appeared as a separate specialty. This has found reflection in the titles, in the terminology (names of books, departments in universities, etc.) and, principally, it affected the content and the style of the work.

The foregoing remarks are quite obvious, but they are appropriate in order to emphasize our main thesis: L. D. Landau is the brightest representative of theoretical physics as a specialty. It was he who introduced an inestimable contribution to the formation and establishment of the style of modern theoretical physics.

Although there is much written and said concerning style and form in art and in literature, the problem of style in science remains in the shadow, and sometimes in complete obscurity. This is to some degree understandable, since style and form, compared with content, play after all a much less significant role in science than in art. Nonetheless, the importance of style, particularly in science, can be readily verified by opening a book or a physics journal of 100 years ago or even of the beginning of our present century. Sometimes such books and articles are almost illegible, incomprehensible, similar say to the Russian literature of the pre-Pushkin period. The modern form of theoretical physics is distinguished by a characteristic laconism, by rationalization of the calculations, by an extensive use of vector and tensor analysis, and by a large number of specific devices. We are so used to it that we hardly notice it; this is not the place to describe them in detail. This entire modern style is a regular fruit of development; it grew primarily as a result of attempts to cope in some manner with the powerful flow of new information, with the broadening front of physical research. Only by mastering the modern style, which finds such a bright and finished expression in the papers and in the textbooks of L. D. Landau, can one remain the master of the situation in all of theoretical physics. It is possible to engage today in the theory of superfluidity, tomorrow in quantum field theory, and the day after in the theory of metals. Such a master of the situation is indeed L. D. Landau, and it was he who helped his direct and indirect student to follow this path.

I can hear here the question: does not such a change in the direction and the objects of research lead first of all to superficiality? Of course, such a change can lead to negative consequences, but in theoretical physics it has so far led essentially only to benefits. The reason lies, in particular, in a certain unity of the methods, in the possibility of transferring progress from one branch to another. A bright example of this is quantum field theory. The methods developed primarily in research in the field of quantum electrodynamics turned out, after suitable modification, to be exceedingly effective in solid-state theory. And there is no doubt that the rapid and unceasing forward progress was due only to the fact that both were done by the same theoretical physicists or by their colleagues and students. Will such a situation continue in the future? It is difficult to be assured of it. Even now, a far reaching differentiation already takes place within theoretical physics itself. Theoretical physicists frequently cease to understand one another, this is how far the paths of many of them have diverged. At one pole are the mathematicians who are engaged in physics, for whom modern theoretical physics has replaced mathematical physics in the old sense of this term. At the other pole are theoretical physicists, who are close to the experiment or, more accurately, to "general physics" and are practically indistinguishable from the experimental physicists, who in our day are no longer obligated to be able to solder well, to work with glass, or to repair electrometers. The division of labor continues, and one can doubt whether even the title "theoretical physicists" will remain in use some fifty or a hundred years from now. And even if it does remain, it will possible be used only for those who engage in theory of fundamental physical problems, such as the theory of elementary particles is regarded at present. But we shall not guess. In the twentieth century theoretical physics has played, still plays, and will continue to play an outstanding role. L. D. Landau's life and work are inseparable from this very period.

Those who know Landau essentially from his books, can understand the foregoing as a statement that he is first of all a pedagogue, an author of textbooks, a systemmatizer of the known and not a creater of the new. Such a conclusion, however, would be greatly in error. The entire point lies precisely in the fact that Landau is an outstanding physicist, who is at the same time a born teacher. Even while still very young he was attracted not only by the intense and exciting research work also by the idea of training youth and teaching theoretical physics and physics as a whole. One rarely encounters a truly outstanding scientist, and even more rarely teachers with a capital T. The combination of both aspects in one person, just as the product of probabilities of two rare events, is an even more incomparably rare phenomenon. I would think that this is precisely the key to the estimate of Landau's place in theoretical physics.

Landau is severe, and without some familiarity with him he can be misunderstood and his guiding motives can be incorrectly interpreted. This has given rise to misunderstandings that have become fixed and distorted in legends. I would like, for example, to discuss the opinion that "Landau considers himself cleverer than all others" etc. This is utterly false and one can attest many times to the modesty and humility with which Landau estimates his place in science. His love for systematization and clarity has found expression, many years ago, in his essentially jocular classification of physicists on a logarithmic scale. This means, that a physicist, say, of the second class has accomplished (precisely accomplished, we are dealing only with accomplishments) one-tenth as much as a first-class physicist. In this scale, Einstein was of class one-half, and Bohr, Schrödinger, Heisenberg, Dirac, Fermi, and a few others were first-class. Landau placed himself in a two-and-a-half, and only some ten years ago, satisfied with some of his work (I recall this conversation, but forget the work involved), he stated that he worked his way up to second-class.

Landau values greatly his younger contemporaries and rates some of them, for example R. Feynman, above himself. In 1963 I met Feynman at a conference in Poland; he asked about Landau's health and about Landau himself. During the course of the conversation I mentioned how highly Landau values Feynman's results and rates them above his own. Insofar as I recall, Feynman became somewhat embarassed and stated decisively that Landau was not right. This of course, is not the point, and no proof is necessary that comparison and assessment of the scientific accomplishments of different persons is not very interesting and certainly not very important. I mention this only in order to emphasize how highly esteemed Landau is by even such highly critical outstanding theoretical physicists as Feynman. Incidently, of all those I met, no one comes closer to Landau than Feynman. This applies to everything - both the scientific style, and the pedagogical ideas, and many personal traits.

Landau's talent is so great and his technique is so polished that one might think he could do even more, solve even more difficult problems.

In this connection, I mentioned this once to Landau, and he, as if he thought about it earlier, answered very distinctly: "No. this is untrue, I did what I could." Landau also stated many times that he is not an inventor and invented nothing. These remarks must be taken, of course, with a grain of salt. Landau is very inventive when it comes to solving problems and to searching for new methods. He is not inventive only in the sense of having a designer's bent and having some traits common to certain inventors. There is a rather popular statement that a good poet should be slightly stupid. One can say with about the same degree of conviction that a good inventor should not be too well educated. After all, inventions are frequently the fuit of tedious guessing, sudden insight, trials, and errors. The sober mind of a highly educated theoretical physicist is so to speak orthogonal to such an inventive style, or to searches in the darkness. Landau's highly critical nature, and his tendency to classify as "pathology" many ideas or, more accurately, hints at ideas, are to a considerable degree the result of just his clarity and sobriety. This, of course, is not always good, but it should be not judged but understood. Landau happened to be mistaken many times in estimates of various ideas, results, and suggestions. But I think that he was much less frequently in error than any other (if, of course, we speak percentagewise, meaning the ratio of the number of misses to the number of hits). What is more instructive is another aspect: Landau's errors, as a rule, are interesting and have educational value. I allow myself to present what may not be the best example, but one which I know thoroughly. In the only paper I wrote jointly with Landau, we derived an equation for a certain effective wave function of superconducting electrons. One term of this equation contains a vector potential **A** and is of the form $1/2m(-i\hbar\nabla - e^*/e\mathbf{A})\Psi$; this obviously is quite similar to the corresponding term in the Schrödinger equation. But what is the meaning of the charge e^* in this phenomenological theory? It seemed to me from the very beginning that the charge e^* need not be equal to the charge of the free electron e, that is, it is possible to introduce into the theory a new parameter, namely the effective charge e^*. But Landau decisively rejected this thought, and in our joint article, published in 1950, there is a typical Landau phrase that "there are no grounds for assuming the charge e^* to be different from the charge of the electron." When I subsequently worked on superconductivity theory, I became convinced, however, that comparison with experiment still offers evidence that more likely $e^* = (2-3)e$. Obtaining such a corroboration, I returned to the idea of the effective charge, and, as usual, brought it to Landau to judge. It was then necessary for him not merely to shrug away this question, but express his opinion in more detail (probably Landau already had this in mind earlier, but did not consider it necessary to go into details, since I had no real argument in favor of the inequality of the charges e^* and e). Specifically, Landau noted that the effective charge (similar, say, to the effective mass) is not universal and should depend on different factors - the composition of the superconducting alloy, the pressure, etc. This means that it is possible to create conditions under which the effective charge will depend on the coordinates, and this violates the gauge invariance of the theory. In response, I attempted to construct a gauge-invariant theory with a coordinate-dependent charge e^*, but did not succeed. In my article of 1955 I wrote it "as it was," that is, I indicated the possibility of improving the agreement between theory and experiment with $e^* = (2-3)e$, and with Landau's permission and of course with reference to him, I cited his objections to the possibility of introducing an effective charge. After the microscopic theory of superconductivity was developed in 1957 it became clear, as is now well known, that the charge e^* in our equations is strictly equal to 2e, since this is the charge of the electron pairs, the unique Bose-Einstein condensation of which causes the appearance of superconductivity. Reasoning formally, one can say that Landau made a mistake in insisting that $e^* = e$. Actually, however, he was perfectly correct in negating the possibility of introducing an effective charge as a parameter. My own guess concerning the inequality of e^* and e was shallow, based on a lack of understanding. Such a guess can be useful, it might lead to an idea, if it were raised to the level of understanding the possibility of pairing electrons and the compatibility of the universal charge e^* with its failure to equal the charge of the electron e. However, as already stated, this was done only after the microscopic theory of superconductivity was developed. This example is typical in the sense that Landau's criticism, his negation of certain premises, always had a scientific basis, a certain thought-out argument. An entirely different matter is that Landau does not always like to explain and clarify his remarks, and frequently answers "think it over yourself." But if Landau is not always ready to answer and explain, this has nothing in common with conceit or lordliness. Landau is deeply democratic, and both pomposity and servility are completely strange to him. Any student could readily discuss with him scientific problems but under only one condition, common to all: if he was at the required scientific level, if he had already thought out the question, and did not want Landau to think for him or do for him what the questioner could do himself.

The last remark is a statement of a fact which does not appear to me accidental, but is connected with Landau's entire operating style. Namely, according to the general opinion, Landau did not show his age. At 54 his eye was just as sharp, the level of his work did not drop, mastery of a new computation technique did not pose any problem to him, he remained at the very forefront of theoretical physics. Only being hit by a truck, an unfortunate accident occurring on 7 January 1962, interrupted his work. Catastrophes and their consequences are almost always absurd. But when one speaks of the accident that occurred to Landau, it seems to be especially absurd and monstrously unjust. But what can you do...

In the name of the tremendous number of physicists in our country and in the entire world I wish to thank Landau for what he did for all of us and to wish him with all our heart health and happiness and the very best.

BIBLIOGRAPHIC NOTE

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