

LANDAU'S ATTITUDE TOWARD PHYSICS AND PHYSICISTS

Lev Davidovich Landau was a unique physicist and teacher of physicists.

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On 22 January 1988 in Moscow, we celebrated Lev Davidovich Landau's 80th birthday in the same hall at the Institute of Physical Problems where Landau held his seminars and where I used to see him. Perhaps that is why the thought haunted me at that memorable meeting that Landau might have been among us that day, sitting there in the front row as he used to, and I expressed it in my opening remarks. But, alas, more than 27 years have already passed since Landau conducted his last seminar: On 7 January 1962, Landau met with a car accident and was disabled for the rest of his life. He died on 1 April 1968.

Today only those older than 45 or so retain any personal impressions of Landau as physicist. Younger physicists and, more generally, those who had no personal contact with Landau know him mostly from his papers, many of which have not lost their freshness or become out of date with the passage of time. The large number of references to Landau's publications that one encounters in the current literature confirms this. For this reason and because of the remarkable graduate-level course in theoretical physics that Landau developed with Evgenii M. Lifshitz (a new edition is now being prepared by Lev P. Pitaevsky) Landau's contributions to physics continue to be of interest. But Landau's legacy cannot be summed up merely by recording his publications. Understandably, there is considerable interest in both Landau as a teacher and person and in his peculiarities as a physicist. One can get acquainted with these aspects of the "Landau phenomenon" from a wonderful paper by E. M. Lifshitz^{1,2,3} and also from the volume of memoirs that at last is expected to

appear.³ That volume contains my reminiscences as well. In addition I wrote a paper in connection with Landau's 60th-birthday celebration, but it appeared instead as an obituary because of Landau's death.⁴ In this article I would like to concentrate on some examples that illustrate Landau's attitude towards physics. But I make no claim whatsoever to have developed detailed analyses and generalizations.

I should note that several times in the past when I wrote reminiscences of outstanding physicists, I was criticized for focusing the discussion partly on myself. Such criticism is quite understandable and, in principle, absolutely right. The reader or hearer of reminiscences of, say, Landau is interested in Landau himself, not in the author of those reminiscences. Unfortunately, this clear understanding of the matter does not help much. Hard as I may try, I cannot put myself aside altogether and in my reminiscences get rid of the "I." The situation is made somewhat comprehensible, however, by noting that one remembers better those things that are important to oneself. In addition, I have a peculiar memory, sometimes very poor or with a high threshold, which exercises a kind of selectivity that I cannot help. So, I ask you, dear reader, to please regard any shortcomings you find in this article as not arising from any desire to use this opportunity to say something about myself.

Theory of superconductivity

I will start with an example that is interesting in many respects. In the only paper I wrote with Landau⁵ we constructed a phenomenological, or macroscopic, theory of superconductivity. (Perhaps it would be better to refer to this theory as quasimacroscopic, for I think Landau preferred that term.) The crucial point in our paper is the equation for a certain order parameter Ψ , "the effective wavefunction of superconducting electrons." The term of

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Lev Davidovich Landau. Photo taken in 1960.

this equation that depends on the vector potential $\mathbf{A}$ takes the form

$$\frac{1}{2m^*} \left(-i\hbar\nabla - \frac{e^*}{c} \mathbf{A} \right)^2 \Psi$$

which is obviously quite similar to the corresponding term in the Schrödinger equation for a particle of charge e^* and mass m^* . The mass m^* can be chosen arbitrarily because the quantity $|\Psi|^2$ cannot be measured directly. So we can put—this is supported by the idea of pairs—that $m^* = 2m$, where m is the free-electron mass. But what is the meaning of the charge e^* ? Since Landau and I were dealing with a phenomenological theory, it seemed to me from the very beginning that e^* was some effective charge that might well be different from the free-electron charge e . But Landau rejected that idea, and in our 1950 paper there is a statement, typical of Landau, that “there is no reason to believe that $[e^*]$ differs from the electron charge.” At the time we wrote that paper I had no concrete reasons for insisting on the introduction of an effective charge e^* , but several years later I came to the conclusion that the introduction of an effective charge $e^* = (2-3)e$ made the agreement between theory and experiment much closer. The possibility of determining e^* experimentally arises because our theory involves a dimensionless parameter

$$\kappa^2 = \frac{2e^{*2}}{\hbar^2 c^2} H_c^2 \delta_0^4$$

where H_c is a critical magnetic field and δ_0 the penetration depth of the external magnetic field into the superconductor. The quantities H_c and δ_0 can be measured directly (in reference 5 we were concerned with what are now called type I superconductors, for which $\kappa < 1/\sqrt{2}$). The parameter κ can also be determined directly from the data on the surface energy between the superconducting and the normal phases and from the limiting field for “supercooling.” Thus, knowing H_c , δ_0 and κ , one can determine the value of e^* .

Naturally, I informed Landau of my result, but he once again objected to the introduction of the effective charge e^* . He argued—he probably had the argument formulated even at the time we wrote our paper—that the effective charge, like the effective mass, might depend on pressure, temperature, metal composition and so on. This meant that e^* could depend also on the coordinates (by virtue, say, of sample inhomogeneity or the dependence of temperature on the coordinates). But such a behavior of e^* would break gauge invariance. I tried to somehow avoid this difficulty but did not succeed, and it was clear that Landau's thoughts were fairly set. So I wrote a paper⁶ in which I pointed out the possibility of substantially improving agreement between theory and experiment by introducing the charge $e^* = (2-3)e$, and I also mentioned in it, with Landau's permission and of course with a reference to him, Landau's arguments against an effective charge different from the free-electron charge. As is well known, the microscopic theory of superconductivity of John Bardeen, Leon Cooper, and J. Robert Schrieffer, in which the charge $e^* = 2e$ arises due to pair formation, appeared soon thereafter. Even now I feel sorry, and to some extent ashamed, to think that I did not consider the possibility of pair formation. Landau's objection to introducing an effective charge no longer arises if the effective charge is universal and equals $2e$ irrespective of temperature, composition and so on. But neither Landau nor anyone else who knew of my paper⁶ thought of the pos-



Evgenii M. Lifshitz (left) and Landau on holiday, Borzhomi, 1960.

sibility of introducing a universal charge of $e^* = 2e$. There are reasons why the idea of pairing, which seems trivial now, was not at all obvious at that time, and I will come back to them later.

Publishing papers under Landau

It has often been said—I have heard it myself—that Landau's sharp criticism of the work of others prevented them from creating something great or at least from obtaining or publishing exceedingly important results. Indeed, Landau did criticize others, hotly and not always politely, but that was his style, and those who knew him were convinced that even his sharpest expressions usually did not imply any personal hostility. As for Landau's preventing others from carrying on their work or from publishing papers, it was out of the question, at least in the cases I know of. The story of the effective charge is not atypical: Though Landau was resolutely opposed to the introduction of the effective charge, not only did he not put obstacles in the way of my publishing my paper, but as I have already mentioned, he allowed me to present our respective arguments.

Important scientific achievements and discoveries do not appear from nothing. More than one person, and sometimes even many people, may think of the same thing—at times, they may even attain the same objective. On the other hand, if someone leaves something out or does not understand a problem to the bottom, what is to blame for that? First, chance plays an exceedingly large role. Of course, I do not mean that the theory of relativity or, more generally, great and profound ideas arise merely by random occurrence. But when we speak of some particular effect, phenomenon or theorem, there may be an infinity of reasons why one or another physicist did not think it through to the bottom, did not "hit" it and appreciate and publish the results. Second, an author's own underestimation of results he obtained at the time he obtained them is the best

evidence that he made the discovery accidentally or that he didn't really make it at all but only claimed later that he had. (When this happens, as it does sometimes, it is in some cases not a deliberate deception but rather a not uncommon psychological effect.) Let me relate a story in this connection: Physicist A mentioned to physicist B that he had derived the Schrödinger equation before Schrödinger but had not published this result because he had not thought it to be of sufficient importance. To this B replied, "I advise you not to tell anybody else about this, for it is no shame to fail to derive the Schrödinger equation, but to obtain such a wonderful result and fail to appreciate its importance really is a shame." I think I heard this story from Landau, and I believe it was he who had played the role of physicist B; at any rate, Landau was of precisely the same opinion as B. In short, Landau might have failed to comprehend or support some obscure idea brought to him for his opinion, or he might have criticized it, but it would simply be foolish to make him responsible for the fact that afterwards, in someone else's hands, that idea proved very fruitful.

On the whole Landau was very tolerant. (Some exceptions cannot change this conclusion because most people have some things about which they are anomalously sensitive.) I will give some examples below, but for the moment I will note only that Landau was rather liberal regarding publications. He objected to publication only of those papers that contained no new results but presented, say, another way of obtaining known results. Generally, Landau spoke with contempt and irritation about "giving new grounds" (from the German word *Neubegründung*, which Landau liked to use in this connection). I am not sure, however, that he interfered actively with publication even of such papers. Of course his approval was not required for publication of most papers as long as the paper did not contain blatant errors. However, not to interfere and to approve are two different things. I don't remember whether or

not Landau approved the publication of my paper⁶ suggesting the value of $(2-3)e$ for e^* , but in general he used to reproach me, sometimes even with irritation, for "publishing everything [you] know." He didn't like one's being too casual in publishing papers, and he, obviously, didn't publish or rush into print everything he did.

It is my opinion that the question of whether to be casual or restrained about what one allows oneself to publish cannot be decided unanimously, for the answer depends on the author's taste and style. I think that Landau's restraint was in some measure determined by, among other things, the fact that he didn't like to write; it is common knowledge that even papers of which he was given as the sole author were usually written by someone else. It also seems to me that Landau was affected by the thought that a physicist of his rank should not publish trifles. I, for one, write rather easily, and moreover, until I write a paper I don't feel that the work is done even in the rough. And once a paper or a note is written and seems to be of some interest, why not try to publish it? This is what I have customarily done and still do, although I long ago came to realize that publishing small notes in no way raises my renown but gives rise to ill-disposed criticism. But I repeat that in my opinion this is a matter of taste and that to me not to publish something only out of fear of the "clamor of Boeotians" does not look like valor at all.

Landau's scientific publications

The two volumes in Russian of Landau's "collected works" contain 98 papers. (The English edition is somewhat different, but not in any essential way.) Seventeen papers and notes are not reprinted in the collected works, but are listed in it. Of these 17 publications, some are short reports on the papers included in the collection. Those papers that Landau himself regarded as erroneous are not included in the collection (see reference 1). Among these is a paper that attempts to explain superconductivity on the basis of the spontaneous current hypothesis.⁷ That paper may seem erroneous, but in fact it contains a very interesting idea.

Landau considered in that paper a phase transition into a state with a nonzero spontaneous current density j .

This breaks the gauge invariance, which must be maintained, but we must remember that the paper appeared before Landau developed the general theory of phase transitions in 1937. Instead of j , however, one can take for the order parameter the density of the toroidal dipole moment T , which possesses the same transformation properties as the current density. This order parameter leads us to a new type of magnetics—toroidal magnetics. I turned to the paper⁸ in 1978, almost 45 years after it was published. I was glad I knew it existed.

The fate of this paper of Landau's seems to me to be in some respects typical of his activities. Like any other man, Landau was in the wrong sometimes. But the number of mistakes he made, and especially their proportion relative to his correct statements, was very small. And even Landau's mistakes often were very instructive about physics.

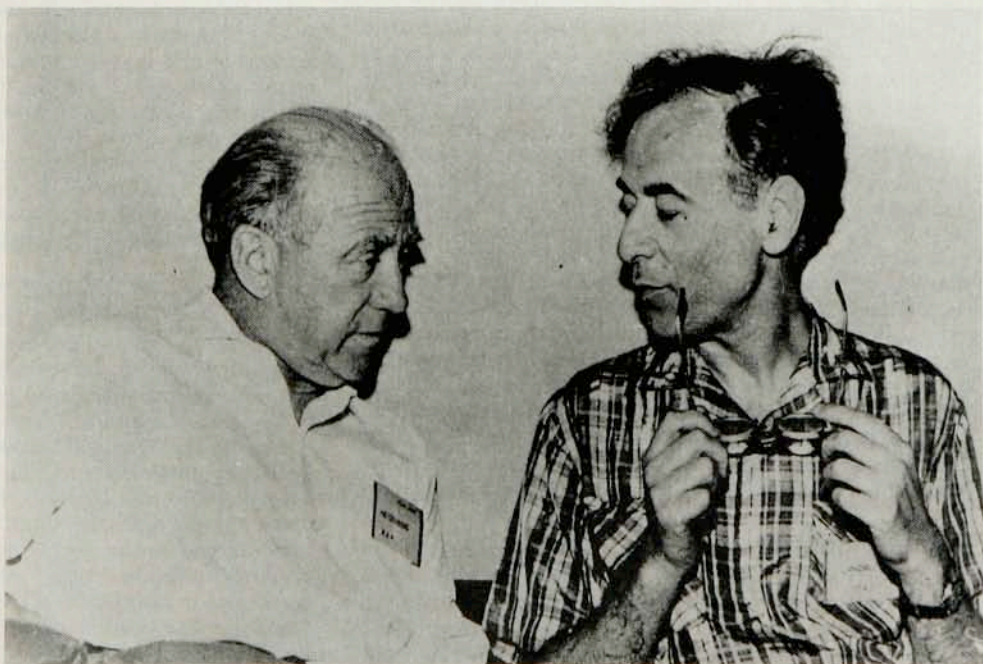
The number of papers Landau published could have been much larger. As I already mentioned, Landau did not publish everything he did; he obviously did not in the least try to publish papers just to lengthen his publication list. Also many results reported in papers by other authors in fact belong partly to Landau. I am not saying that these were instances of plagiarism, but rather that without Landau's inestimable advice and criticism, some of the papers his students and colleagues wrote would not have appeared at all or would have been of much less value. Sometimes Landau simply renounced co-authorship, refusing to be included even when he had contributed substantially to the work reported.

In 1943, for example, I was working on the problem of the field acting in a rarefied plasma. Some authors at that time believed that the effective field E_{eff} was equal to the mean macroscopic electric field E .² Others supposed that E_{eff} did not equal E but, rather, for instance, that

$$E_{\text{eff}} = E + \frac{4\pi}{3} P = \frac{\epsilon + 2}{3} E$$

where

$$P = \frac{\epsilon - 1}{4\pi} E$$



Werner Heisenberg (left) and Landau at Kiev, 1960.

is the polarization and ϵ is the dielectric permittivity. To sort out the right answer was not an easy task, given the state of plasma theory at that time. I got confused, didn't know how to achieve consistency in my analysis of the problem and asked Landau for advice. He was convinced from the very start that $\mathbf{E}_{\text{eff}} = \mathbf{E}$, but he did not think that this was obvious and could go without proof. With Landau's help I proved that $\mathbf{E}_{\text{eff}} = \mathbf{E}$. I then wrote a paper reporting the proof and brought it to Landau, whom I had included as my coauthor. But he did not accept the offer, and the paper appeared under my sole authorship.⁹ In the paper I acknowledged Landau for "a detailed discussion of the problem" and for instructive advice on how to take close collisions into account. (In reference 3 I give another example of Landau's renouncing coauthorship. But I would not like to dwell here on that case, which is unpleasant for me, although it is less trivial than the one narrated above.)

I do not know exactly why Landau renounced authorship when he did; I suppose that in these cases Landau simply did not consider the results interesting or valuable enough. By the way, some who sought Landau's advice or help could not appreciate it (which was indeed not always easy to do), and for this reason published the results under their name only. I know of results, some of them popular in the literature, that in all fairness should bear Landau's name and not the names of his colleagues only.

Superfluidity

I now return to the theory of superconductivity and superfluidity. It was no accident—or so it seems to me—that Landau did not surmise the existence of pair formation following my suggestion that the effective charge in our phenomenological theory might be twice the free-electron charge. Landau had long been of the opinion that Bose statistics and Bose-Einstein condensation had nothing in common with the superfluidity of He II (the superfluid phase of He⁴). He based his assertion on the fact that an ideal Bose gas is not superfluid.¹⁰ Besides, it seemed to Landau that one did not need to invoke the Bose statistics of He⁴ atoms to explain the superfluidity of helium II. But it is essential for the superfluidity of He II that atoms of He⁴ obey the Bose statistics. As far as I know, this was first clearly shown by Richard P. Feynman.¹¹ Unfortunately, I don't remember how Landau reacted to the fact that liquid He³, first obtained in 1948, is not superfluid down to very low temperatures $T > 0.1$ K. (In the early 1970s it was discovered that He³ is superfluid at temperature $T \leq 0.1$ K.) In any case, before the formulation in 1957 of the BCS theory of superconductivity, the idea of electron pairing was alien to Landau, as well as to very many other people. I have written this in more detail elsewhere.¹² Here I wish merely to note that if reproaches are admissible in such cases, then I, perhaps more than Landau, am the one who deserves them.

Fermi-liquid theory

For some time Landau believed that plasmons in solids cannot be "good" quasiparticles because their damping time must be small, that is, of the same order of magnitude as the period of oscillations. (See reference 3.) This view

reflected one of Landau's favorite theses, namely that electrons cannot form an almost ideal Fermi gas in a normal (nonsuperconducting) metal "because nobody has yet repealed Coulomb's law." It was, of course, none other than Landau who explained all this later in the theory of the Fermi liquid.¹³ As for plasmons, they do exist in simple metals—their damping is relatively small.¹⁴

General relativity

Landau was a great admirer of general relativity theory, which he called "the most beautiful of the existing physical theories." But he categorically denied the possibility of introducing the Λ term into Einstein's equation, as well as of somehow changing or generalizing general relativity theory, even when such changes did not violate the good agreement between the theory and the known observational data. Though I fully share his admiration, I could not understand why it should rule out the existence of the Λ term. I don't remember that Landau ever put forward any physical arguments against the Λ term, but at that time there undoubtedly were no realistic arguments for its existence, and Einstein himself believed, I think, that his introducing this term had been a mistake. I haven't been able to find in Einstein's papers the place where he expressed this sentiment, but in the appendix to his book *The Meaning of Relativity* we read: "The introduction of this additional term made the theory more complicated and thus did much harm to logical simplicity."¹⁵ Wolfgang Pauli, too, expressed a negative attitude toward the use of the Λ term.¹⁶ It is now common knowledge that introducing the term Λ is equivalent to using the equation of state $P = -E$, and the term is now the subject of much discussion in the theory of the early universe.

What do I wish to illustrate by this discussion of the Λ term in general relativity? Landau, like his great older contemporaries Einstein and Pauli, attached much importance to logical simplicity and beauty in a fundamental theory. He realized that emphasizing logic and beauty is inevitable and necessary when considering problems for which there is not enough experimental data, and for which the theoretical possibilities are numerous. This attitude was above all a manifestation of Landau's pragmatism: In those days research in general relativity and especially in relativistic cosmology was not yet in a position to justify the introduction of the Λ term or to extend the general theory of relativity.

First physics encounter with Landau

For a short time starting toward the end of 1939, two groups of theorists—one headed by Landau, at IFP, the Institute of Physical Problems of the USSR Academy of Sciences, the other headed by Igor Tamm at FIAN, the P. N. Lebedev Physical Institute of the academy—met for joint seminars, held alternately at the two institutes. Both groups were very small; of Landau's coworkers at the time I now remember only Evgenii Lifshitz. I recall in particular two joint meetings. At one of those, held at IFP, Landau reported on the theory of superfluidity and Tamm proposed the term "roton," as mentioned in reference 10. At the other meeting, at FIAN, Tamm started discussing one of my first papers, devoted to the quantum theory of Vavilov-Cerenkov radiation. I had shown that the condi-

tions for emission of this radiation follow when one applies the laws of conservation of energy and momentum to a particle radiating a photon of energy $\hbar\omega$ and momentum $\hbar\omega n/c$ in a medium of refractive index $n(\omega)$. I had also calculated the intensity of the VC radiation, but we did not come up to the intensity in that seminar.

Landau immediately expressed skepticism about my results, saying that they were of no interest since the effect was classical and there was no point in treating it quantum mechanically. He was right in some sense, because quantum corrections to VC radiation are of the order of $\hbar\omega/mc^2$ (m is the particle mass) and therefore are small in the optical region. But very often a new interpretation, approach or conclusion turns out to be fruitful. This was the case in my work on the VC radiation: The quantum approach and the use of the conservation laws appeared to yield new results in, for example, studies of the Doppler effect in a dispersive medium. All this is discussed in reference 17 and in the literature cited there, so I will leave out the details.

I have dwelt on this experience with Landau, first, to demonstrate once again his pragmatism and his dislike for *Neubegrundung*. Second, this example is a striking illustration of the role of tastes and feeling in science. I literally love problems dealing with the VC radiation and, generally, with radiation from uniformly moving sources.¹⁷ Landau was quite indifferent to this range of questions and did not consider the VC effect beautiful. The above incident is not the only illustration



Portrait by Mogilevsky.

of this predilection: I remember telling Landau about a paper—I think it was the paper by David Bohm and David Pines—in which the plasma (longitudinal) wave damping discovered by Landau¹⁸ was interpreted as the inverse VC effect. But Landau remained quite indifferent to that interpretation.

I have already discussed the “accusations” that Landau’s bitter criticism might have been an obstacle in someone’s way. The claims that Landau was a conservative and that he “considered himself the most clever man” are groundless as well. I am not inclined to idealize Landau and to pass in silence over his weak points. Landau himself was not beyond criticizing even those he regarded as great, and he was critical of himself. The latter trait showed up in many ways. For example, Landau considered himself inferior “in class” to a number of other physicists and his contemporaries.^{1,3,4} This was particularly true of his attitude toward Feynman, who was 10 years younger than Landau. In 1962 I met Feynman at a conference in Poland. Feynman was concerned about Landau’s health and inquired after him. (The two had never met.) I mentioned how highly Landau thought of Feynman’s results, and that he ranked them higher than his own. Feynman was somewhat embarrassed by this and resolutely declared that Landau was wrong in so judging him. Such comparisons are not my main point, however. Even Landau mentioned his “classification” less often as the years passed, and he started to take a more sober view of classifying people. By the way, of all the physicists I have known personally, nobody resembled Landau more than Feynman. The resemblance extends to many things, scientific style, aspects of their personalities

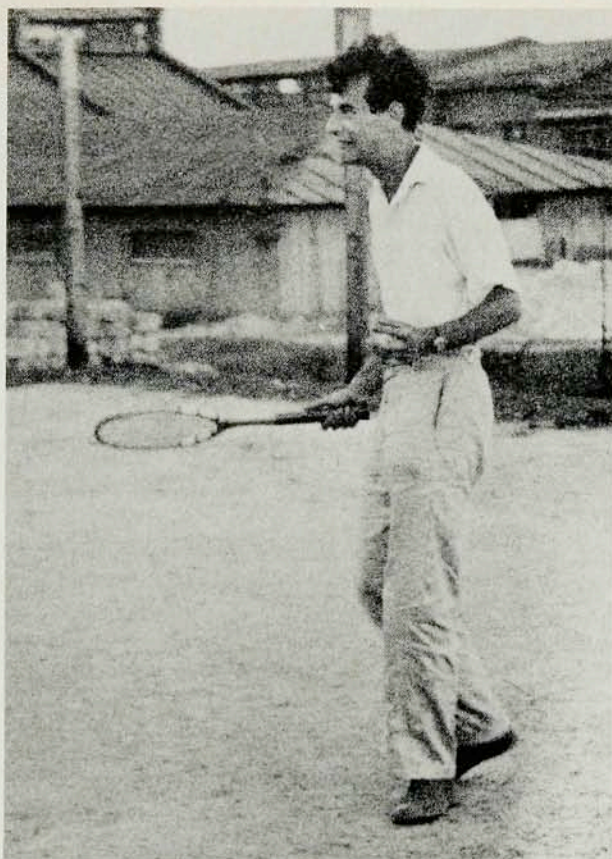
and behavior, and interest in pedagogical ideas. Even among famous physicists there is a diversity of talents. Niels Bohr and Landau, for instance, were on opposite extremes. Landau and Feynman I see as having had very similar talents. The similarity seems to me to be genetic. The differences, which one could attribute to their different surroundings and different upbringing, are certainly also very large. What a pity it is that these two remarkable physicists never met. It really pains me to think of this “product” of our past.

Landau’s talent was so bright, and he commanded the techniques of theoretical physics so skillfully, that one is inclined to think he could have done a lot more and solved still more difficult problems had he lived longer. Once, in a conversation with him I expressed my feeling that he could achieve more than he had. He replied immediately and very definitely, as if he had already thought about it, “No, this is wrong; I have done everything I could.” I believe he was right. He generally worked very hard and tried to solve very difficult problems. For example, he spent much effort trying to formulate a theory of second-order phase transitions beyond the scope of the self-consistent field approximation. He once told me that no other problem had taken so much of his strength as this one, with which he still had not made much progress.

Landau often also asserted that he was not an inventor, that he had invented nothing. As far as the invention of devices is concerned, it is true that he had no talents as a designer. The sober mind of a highly educated physics theorist and analyst works in a way that is somehow orthogonal to the search-in-the-dark, trial-and-error methods of the inventor. But Landau was fairly



Landau at the low-temperature physics conference, Kiev, 1955, with (from left to right) unidentified, S. Kapitza, E. M. Lifshitz, A. Akhiezer and I. Akhiezer.



inventive when it came to solving complicated problems and searching for new methods.

Landau's highly critical attitude stemmed from his sober mind and his comprehension and profound knowledge of physics. Besides, Landau didn't care a cent about presenting his remarks and opinions disingenuously. That often made him look opinionated and unwilling to adopt new ideas. But that would have been a misimpression, because Landau very often agreed, maybe not immediately, with disputable hypotheses and, in general, with new trends. So I do not support the opinion about Landau's conservatism that I sometimes hear. It is certainly difficult to weigh the degree of conservatism on a precision balance. It is also difficult to decide where the boundary lies between the true conservatism and the "healthy" conservatism—by the latter I mean, for example, the recognition that the old order should not be broken without good reason.

Landau's last published paper "On the Fundamental Problems," is to me vivid proof that he was not a conservative. It appeared in 1960, in a collection in commemoration of Pauli.¹⁹ In this paper Landau expressed the opinion that the "Hamiltonian method for strong interactions is dead and must be buried." So Landau was ready to face the breakdown of fundamental theory. (The Hamiltonian method was later on found to be far from exhausted and now forms the basis of quantum chromodynamics.)

In this article, I realize, I could promote the understanding of Landau's style and his scientific image only to a very small degree. But I find consolation in the thought that to provide real insight into the nature of this remarkable physicist is extremely difficult. I would, however, like to make one final remark. Few are those to whom I have returned in my memory as often as I have to Landau, even though he left us many years ago. I cannot explain this fact only by my friendly feelings toward him and by his tragic and bitter demise. Another fact might provide a clue to this phenomenon: Landau was a unique physicist and a teacher of physicists. So my attitude toward him is inseparably linked with our attitude toward physics, which is so dear to all of us.

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This article is adapted from a talk the author gave at the Landau Birthday Symposium at NORDITA, Copenhagen, 13–17 June 1988. The talk also will be published in the book *Landau: The Physicist and the Man*, edited by I. M. Khalatnikov (Pergamon, Oxford, 1989).

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