

My years with Landau

The discoverer of "type-II" superconductivity lets us in on the excitement of an important time for low-temperature physics

A. A. Abrikosov

During a decade or so of my life I had the opportunity to talk with Lev Davidovich Landau almost every day, and to profit from his advice. This may be why the period from about 1950 to 1960 was so successful for me, and I would like to share my memories of it with you.

In 1950 Vitali L. Ginzburg and Landau wrote their well known paper¹ on superconductivity. Without the microscopic theory, developed later by John Bardeen, Leon Cooper and J. Robert Schrieffer,² the meaning of several quantities entering the Ginzburg-Landau work remained unclear, above all the meaning of the "superconducting electron wave function" itself. Nevertheless this theory was the first to explain such phenomena as the surface energy of electrons at the superconducting-normal phase boundary and the dependence of the critical field and current in thin films on temperature and thickness.

A new type of superconductor

Experimental verification of the Ginzburg-Landau predictions concerning the critical fields of thin films

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was undertaken by my friend N. V. Zavaritski, who was then a young research student of A. I. Shalnikov. I often discussed the matter with Zavaritski. Generally his results fitted the theoretical predictions well, and he even managed to observe the change in the order of the phase transition with decreasing effective thickness (ratio of thickness to penetration depth at a given temperature). For that experiment he used the hysteresis of the dependence of the resistance $\rho(H)$ on the field. One day, Zavaritski slightly altered his technique of sample preparation. Usually he evaporated a metal drop on a glass plate and put the resulting mirror into a Dewar flask. This time instead, he began to do the evaporation inside the Dewar flask, with the glass plate at helium temperature.

We know now that in this case atoms reaching the plate are trapped at the place where they hit and are unable to move to form a regular structure. Therefore an amorphous substance is produced, which at every effective thickness will be a "type-II" superconductor. But at that time this was, of course, not known.

The dependence of critical field on thickness, as measured by Zavaritski,³ did not follow the Ginzburg-Landau formulas. This seemed to be a para-

dox: Apart from its beauty, the theory really explained many things, and we were surprised to see that suddenly it had failed.

When Zavaritski and I discussed the possible origin of this discrepancy, we came to the idea that the approximation $\kappa \ll 1$ based on the surface-tension data (where κ is the adjustable Ginzburg-Landau parameter), could be incorrect for such new objects as the low-temperature films. In particular one could suppose that $\kappa > 1/2^{1/2}$. According to Ginzburg and Landau, the surface energy should be negative under these conditions. Intuitively, they felt that in this case the phase transition in the magnetic field would always be of second order, and this was in fact what Zavaritski observed.

When I calculated the dependence of critical field on effective thickness with $\kappa > 1/2^{1/2}$, the theory appeared to correspond to the experimental data. This gave me the courage, in a 1952 paper⁴ containing this calculation, to state that apart from ordinary superconductors whose properties were familiar to everybody working on the subject, there exist in nature superconducting substances of another type, which I propose to call "superconductors of the second group" (now called type-II superconductors). The division between the first and the second group



The theoretical group at the Institute for Physical Problems in 1955: (seated, left to right) L. Prozhorova, A. A. Abrikosov, I. M. Khalatnikov, L. D. Landau, E. M. Lifshitz, (standing, left to right) S. Gerstein, L. P. Pitaevski, L. Veinstein, R. Arkhipov, I. Dzialoshinski.

was defined by the relation between the quantity κ and its critical value $1/2^{1/2}$.

After that I tried to investigate the magnetic behavior of bulk type-II superconductors. The solution of the Ginzburg-Landau equation in the form of an infinitesimal superconducting layer in a normal sea of electrons was already contained in their paper. Starting from this solution I found that below the limiting critical field, which is the stability limit of every superconducting nucleation, a new and very peculiar phase arose, with a periodic distribution of the ψ function, magnetic field and current. I called it the "mixed state."

Landau showed a notable interest in this work and wanted me to publish my results for the vicinity of the upper critical field, which I named H_{c2} . But I wanted to understand how the new mixed state looked in the total range of fields.

Landau has his doubts

At this time I became ill and had to stay in bed for almost three months. One day Landau visited me. The conversation, as in most cases, concerned everything but physics, and Landau sipped with great pleasure from a glass of *glühwein*, which was not at all like him. And then suddenly

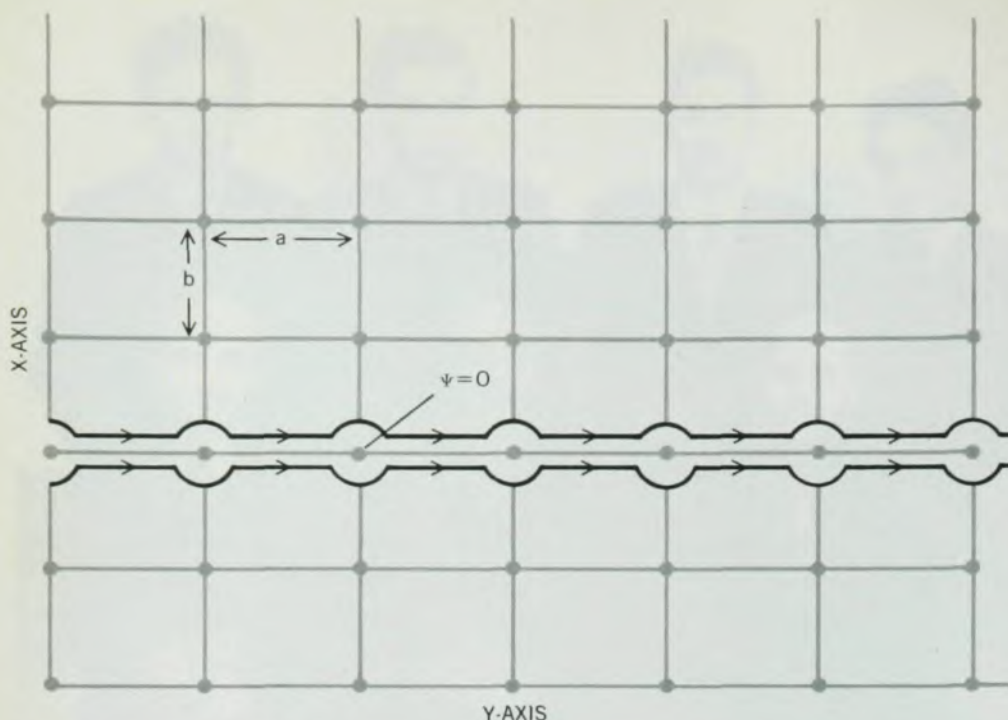
I destroyed all this paradise by telling him what I had wanted for the mixed state, namely the elementary vortices. As Landau's eyes fell on the London equation with a delta function on the right-hand side, he became furious. But then, remembering that a sick person should not be bothered, he took possession of himself and said: "When you recover we shall discuss it more thoroughly." He hastily bade me farewell and disappeared.

He did not come to see me any more. When I felt better and appeared at the Institute and tried to tell him again about the vortices, he swore rather ingeniously. At that time I was still very young and did not know the temper of my teacher well enough. He had seen many kinds of pseudoscience in his life, and this made him suspicious toward unusual statements. However, by making some effort and disregarding the noise he made, one could always "drag through" him any reasonable idea. But at that time I sadly put my calculations in my table drawer until better times.

But in fact the idea was not so bad. Analyzing the solution that I got close to H_{c2} , I saw that in the plane perpendicular to the field there are points where ψ becomes zero. The phase of the ψ -function changes by 2π along a path around such a point. I thought

about why such singularities should appear, and saw that it could not be otherwise. Indeed the Ginzburg-Landau equation contained not the magnetic field but the vector potential. If the magnetic field does not vary in sign over the whole sample, then the vector potential must increase with the coordinate. But the physical state in a uniform field (as is the situation close to H_{c2}) must be either uniform or, at most, vary periodically in space. So the increase of the vector potential must be compensated for by a change in the phase of the ψ -function.

The figure on page 58 helps us to see this. Let the field be along the z -axis and let us choose the vector potential $\mathbf{A}$ such that $A_y = Hx$. Imagine the xy plane. The gray points are those I have mentioned, where ψ becomes zero. If we want to have a unique determination of the phase we must draw cuts in the plane. We draw them through the gray points parallel to the y -axis. From the figure it is evident that when we go around the points the phase increases by $(\Delta\psi)_1 = \pi y/a$ if we move along the lower path, and by $(\Delta\psi)_2 = -\pi y/a$ if we move along the upper one. That means that at every cut the gradient of the phase $\partial\psi/\partial y$ undergoes a jump $2\pi/a$. If we use ordinary units (at that time I used the dimensionless Ginzburg-Landau units)



Vortex lattice of a type-II superconductor is seen in a schematic diagram with magnetic field along the z -axis and unit cell dimensions a and b . To determine the phase of the ψ -function uniquely, draw a cut through the gray points parallel to the y -axis. Around each point, the phase changes by π if we follow the lower (clockwise) path and by $-\pi$ along the upper path, so that, with y/a the number of points, $(\Delta\psi)_{\text{lower}}$ is $\pi y/a$ and $(\Delta\psi)_{\text{upper}}$ is $-\pi y/a$. At every cut then, $\partial\psi/\partial y$ jumps by $2\pi y/a$. In the limit, with decreasing magnetic field, one vortex exists, so that ψ must equal zero along the z -axis.

we see that compensation for the increase in A_y demands

$$\frac{2e}{c} H b = \frac{2\pi\hbar}{a}$$

or

$$H a b = \frac{\pi\hbar c}{e} = \Phi_0$$

which is the flux quantum. Because I used dimensionless quantities I did not mention the flux quantum on the right, but I understood that with a decreasing magnetic field the cell dimensions " ab " must increase, and as a limit one vortex must be considered, in which case the phase of ψ changes by 2π in going around it. On the z -axis ψ must equal zero. Otherwise the ψ -function is not uniquely defined. Such a picture allowed me to find the lower critical field H_{c1} and the magnetization curve $M(H)$.

A worthwhile diversion

But, as I have noted (and this was in 1953), after the fuss my professor made I dropped the matter. There was also another reason. Interesting news appeared in a completely different field—quantum electrodynamics.

After the wonderful work of Julian Schwinger, Richard Feynman and Freeman Dyson, many people were interested in knowing whether it would be possible to sum up all the higher-order corrections and to find formulas for the Green's functions and physical phenomena without developing in pow-

ers of the fine-structure constant $e^2/\hbar c$. My friend Isaac Khalatnikov as well as myself had an old interest in the problem. At this time a paper⁵ by S. F. Edwards was published, in which an attempt was made to sum up a ladder sequence of Feynman graphs for the electron-photon vertex part. We studied this paper and finally came to the conclusion that Edwards had done what he could but had not done what was really necessary, since he had no reason to choose this particular sequence. We tried to do something better and finally wrote some relation between the electron Green's function and the vertex part, which, as it soon became clear, was completely wrong. However when we substituted it into Dyson's equations we began to obtain various interesting consequences, as for example expressions for the electron mass and for the renormalized interaction.

Landau became extremely interested, but being busy with other problems he had no time to study the new technique of the quantum field theory. So he asked Khalat and me to teach him. I must confess that at that moment we were able to do calculations but had no true understanding of the fundamentals of the theory. Landau swore heavily, but after a month he said that he understood everything. He explained to us that we needed to find the main sequence of graphs having the highest power of the big logarithm with a given power of the inter-

action constant. This simple idea put everything in its place. We were indeed successful in calculating the asymptotic expressions for the Green's functions and various physical phenomena at high energies.⁶ Moreover the principle of summing the main Feynman graphs proved afterwards to be extremely useful in various statistical problems.

Vortices from Feynman

Being occupied with such interesting things I of course did not turn my mind back to the work on type-II superconductors. But Landau had a long-standing interest in the state of He II in a rotating vessel. On the one hand, the helium should not be dragged along by the wall, but on the other hand, this was energetically favorable. In 1955 Landau and E. M. Lifshitz published a paper⁷ in which they proposed a layer-type structure with velocity jumps on the layer boundaries. After a year they discovered Feynman's paper⁸ in which it was shown that elementary vortices appear in rotating helium. Landau immediately said that Feynman was right, and that he and Lifshitz were wrong. Of course it was true. In terms of our superconducting notation, the He II could be considered as an extreme case of a type-II superconductor, with a correlation length of the order of interatomic distances and with an infinite penetration depth. But at that time this was not so evident.

When Landau began to praise Feynman's work I asked him: "Dau, why are you ready to accept the vortices from Feynman and flatly rejected the same idea from me." Landau answered: "You had something different." "Well then look, please," I said, and produced my calculations from the drawer. This time no objections followed. We discussed the subject very thoroughly and Landau's remarks were very useful.

When everything was put in order I remembered that I had already seen very similar magnetization curves with two critical fields, namely those of superconducting alloys. Digging for the corresponding experimental data I found the old work of 1937 by L. V. Shubnikov, W. I. Khotkevich, J. D. Shepeliov and J. N. Riabinin⁹ on the magnetization curves of Pb-Tl alloys. They had prepared their samples very carefully, annealing them for a long time close to the melting temperature. So their samples were probably sufficiently uniform, and this was also confirmed by a rather small hysteresis. But at that time, and during the subsequent 25 years, everybody explained this form of the magnetization curve in terms of the formation of a "Mendelssohn sponge," that is, a nonuniform

structure with a distribution of critical parameters.¹⁰ It is worth mentioning that even many very good experimentalists finally believed in the mixed state only after they saw the powder figures of a vortex lattice obtained in 1966 by Uwe Essmann and Hermann Träuble.¹¹

So my work was published in 1957 in *JETP*.¹² In the same year I reported it at a low temperature conference in Moscow in which some physicists from Oxford and Cambridge also took part. Nobody understood a single word. This could be explained however, by the fact that I had a terrible cold with high temperature and had hardly any idea myself of what I talked about. The translation of the paper was then published in the *Journal of Physics and Chemistry of Solids*,¹³ but with more than one hundred errors in the formulas and text, and this of course did not improve the situation.

As you know, in the same year—1957—the famous paper by John Bardeen, Leon Cooper and J. Robert Schrieffer appeared. Everybody became enthusiastic about its ideas—we ourselves among the others. Therefore my paper did not attract attention. Of course it would be unjust for me to complain, because by using BCS theo-

ry we managed to get a lot of interesting results and were able to develop and improve greatly the methods of statistical calculations.

Then, in 1961 John Kunzler and his colleagues discovered that the alloy Nb_3Sn possesses a critical field of about 100 kOe.¹⁴ Shortly after that, alloys with high critical fields began to be used for constructing superconducting coils. This drew attention to the theory of superconducting alloys.

Type-II rediscovered

In my work of 1957 I noted the connection between the quantity κ and the free path length; as I have said, nobody knew about this paper. However there did exist a series of papers by Brian Pippard¹⁵ in which he qualitatively established the connection between the sign of the surface tension on the one hand and the ratio of the correlation length to the penetration depth on the other. He also mentioned the decrease of the correlation length with the free path length. In 1961, on the basis of Pippard's ideas, Bruce Goodman rediscovered that alloys with high critical fields have a negative surface tension.¹⁶ Goodman calculated the magnetic properties of such alloys supposing a simple layer model for the distri-

bution of the normal and superconducting phases. The results were in qualitative agreement with experiment.

I don't know how it happened, but probably somebody told Goodman about my work. What followed was completely incredible. In 1962 Goodman published another paper¹⁷ in which he gave a short presentation of my theory and analyzed the experimental data for type-II superconductors, comparing them with the predictions of both theories, mine and his own. He came to the conclusion that the vortex model fits experiment much better than the laminar one. So the aim of Goodman's paper was to prove that his theory was worse than mine! I have never in my life seen another example of this kind, and took the first opportunity to express my admiration to Goodman.

After this paper by Goodman, physicists working on superconductivity finally developed an interest in my work, and I suppose that this was to a considerable extent the cause for the favor done to me by the London Award Committee. Therefore I would like to express once more my gratitude to Bruce Goodman.

Of course the fact that the award

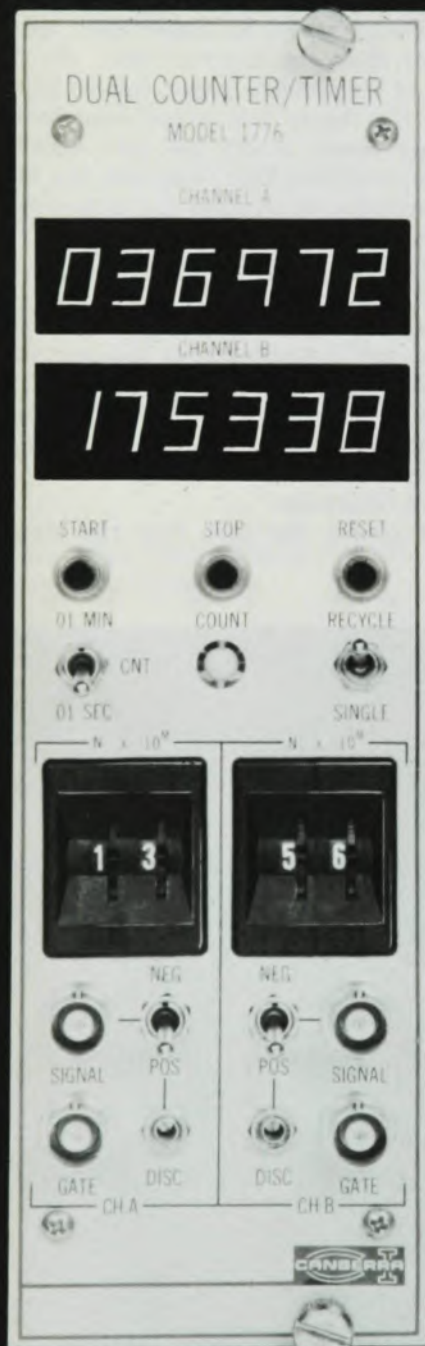


Alexei Alexeivitch Abrikosov is seen here in a moment of relaxation. This photograph was taken in 1967 during a visit to the Kamchatka peninsula in the northeastern USSR.

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A more formal portrait of Abrikosov is seen in this photograph, taken in 1970.

given me has the name of Fritz London is particularly pleasant for me, because he, together with Heinz London, invented the first phenomenological equations of superconductivity. As became apparent much later, they had described the electrodynamics of just the type-II superconductors. Also it was Fritz London who introduced the notion of the magnetic flux quantum, which has a direct relation to the subject.

Finally I would like to mention some of my other activity in the low-temperature field. With one exception it had a much quicker reception. My work with L. P. Gor'kov on the microscopic theory of superconductivity (high frequency behavior,¹⁸ Knight shift¹⁹ and the influence of impurities, particularly magnetic impurities,²⁰ on the properties of superconductors) was accepted by readers rather soon after publication, and has been developed further. It appeared, by the way, that the gapless superconductivity that we predicted for magnetic alloys could also exist under rather different conditions. My work on the Kondo effect²¹ and the studies with Khalatnikov²² of the properties of liquid He³ had a lucky fate too.

The exception I have mentioned is the theory of semimetals of the bismuth type that L. A. Falkovski and I constructed ten years ago.²³ This theory explained such peculiarities as the strange crystal structure, the small number of free carriers and the large dielectric constant and gave formulas for the energy spectrum of bismuth that fitted experiment very well.

Recently I got some new results based on this theory and these have been published in the *Journal of Low Temperature Physics*.²⁴ I hope that they will be interesting to those who work on semimetals and on the metal-

insulator transition. In conclusion I would like to express my gratitude to the Fritz London Award Committee for the honor it has given to me.

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The author is the recipient of the 8th Fritz London Award for Distinguished Contributions in Low Temperature Physics. This article is adapted from his acceptance speech, delivered in his absence by Pierre Hohenberg at the 13th International Conference on Low Temperature Physics in Boulder, Colorado last August.

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