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IN MEMORY OF VITALY LAZAREVICH GINZBURG

About Ginzburg – Landau, and a bit about others

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Abstract. **This note is a brief history of how the theory of Ginzburg and Landau came to be. Early publications on the macroscopic theory of superconductivity are reviewed in detail. Discussions that the two co-authors had with their colleagues and between themselves are described. The 1952 review by V L Ginzburg is discussed, in which a number of well-defined requirements on the yet-to-be-developed microscopic theory of superconductivity were formulated, constituting what J Bardeen called the 'Ginzburg energy gap model'.**

The intention of this note is to present my historical research ('historical' not in the sense of significance, of course) into V L Ginzburg's work in the 1950s on the physics — and particularly the macroscopic theory — of superconductivity. This memorial issue of *Physics–Uspekhi* highlighting Vitaly Lazarevich's brilliant achievements may seem to be an odd place for such a note, given that the work to be reviewed was done fifty plus years ago and that its scientific results are long and well known. I am not, however, on the point of going into detail on questions of pure science, which are the subject of numerous monographs and reviews, and will instead briefly concentrate on the role of VL in the development of the macroscopic theory of superconductivity (I take the liberty of using the abbreviation VL throughout because this was our, his colleagues', nickname for him among ourselves). I would like to recall here how the macroscopic theory developed and came into being in serious discussions between its authors, and to touch in this connection on some aspects of research psychology. I saw in the course of my historical research that the seminal papers on macroscopic theory (as well as the unpublished memoirs of its contributors and those around) are thought provoking, not only regarding pure science, but also with regard to the authors themselves and the time they lived in.

It is natural to start with the 1950 work of V L Ginzburg and L D Landau [1] which, notably, brought VL the Nobel Prize in Physics 2003, and more than deservedly so, because the implications of this paper extend far beyond its subject, the theory of superconductivity, and the ideas it formulated are still at work in many areas of physics, chemistry, and even mathematics. Along with Ref. [1], my sources are two reviews by Ginzburg [2, 3] published soon afterwards and (the Nobel Prize winning) paper by A A Abrikosov [4]. Besides, some other studies of the same period of the 1950s will be briefly mentioned.

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To begin, then, in Ref. [1] Ginzburg and Landau used the theory of second-order phase transitions developed earlier by Lev Davidovich to write down an expression for the free energy of a superconductor, and next utilized this expression to derive the following equation for a certain wave function Ψ, whose absolute value squared $|\Psi|^2$ was considered as the density of superconducting electrons:

$$\frac{1}{2m^*}\left(-\mathrm{i}\hbar\nabla - \frac{e^*}{c}\,\mathbf{A}\right)^2\Psi + \alpha\Psi + \beta\Psi|\Psi|^2 = 0\,. \tag{1}$$

The boundary condition for equation (1) is as follows:

$$\left(-\mathrm{i}\hbar\nabla - \frac{e^*}{c}\,\mathbf{A}\right)\mathbf{n} = 0\,. \tag{2}$$

Here, $\mathbf{n}$ is the unit vector normal to the surface of the superconductor, $\mathbf{A}$ is the vector potential of the magnetic field occurring in the system, $\hbar$ is the Planck constant, c is the speed of light, and α and β are coefficients in the free energy expansion in a power series of the order parameter $|\Psi|^2$ about the phase transition point. Notice that $\alpha = \alpha_0(T - T_c)$, where T_c is the superconducting transition temperature. Along with the equation for Ψ, other results of Ref. [1] were equations for the vector potential and an expression for the superconducting current $\mathbf{j}$:

$$\Delta\mathbf{A} = -\frac{4\pi}{c}\,\mathbf{j}\,, \tag{3}$$

$$\mathbf{j} = -\frac{\mathrm{i}e^*\hbar}{2m^*}\,(\Psi^*\nabla\Psi - \Psi\nabla\Psi^*) - \frac{e^{*2}}{m^*c}\,|\Psi|^2\mathbf{A}\,. \tag{4}$$

The Ginzburg–Landau equation is, in a sense, the standard quantum-mechanical nonlinear Schrödinger equation for a system of particles with charge e^* and mass m^*. Putting aside for the moment the question of the magnitude and physical meaning of these parameters, we only quote here from Ref. [1]: "*there is no reason to consider them to be different from the electron charge and mass* (e, m)." It should be noted that using quantum-mechanical equation (1) and the quantum-mechanical expression (3) for the current in constructing the phenomenological theory of superconductivity was highly nontrivial. Although by the 1950s there was a clear realization among many physicists that superconductivity had to do with the quantum nature of the metallic state, it was in Ref. [1] that this association was clearly and unambiguously expressed.

Equations (1) and (3) can be used to consider a superconductor placed in an external magnetic field H_0. Assuming that the wave function Ψ remains unchanged by the magnetic field, Eqn (4) yields the following expression for the current:

$$\mathbf{j} = -\frac{e^2 N_s}{mc}\,\mathbf{A} = -\frac{c}{4\pi\lambda^2}\,\mathbf{A}\,, \tag{5}$$

where $N_s = |\Psi_0|^2$, and

$$\lambda^2 = \frac{mc^2}{4\pi N_s e^2}\,. \tag{6}$$

Equation (5) is identical to the equation describing the ideal diamagnetism of superconductors, which two English physicists, the London brothers, suggested on a purely intuitive basis back in the 1930s. Solving Eqns (3) and (5) for a system placed in an external magnetic field yields the behavior of a superconductor in this field. As shown by the Londons, the external field is expelled from the bulk of a superconductor and decays into the depths from the surface exponentially over a distance on the order of the penetration depth λ determined by relation (6).

Within the framework of the Ginzburg–Landau equation, we can self-consistently solve equations for the vector potential $\mathbf{A}$ and the wave function Ψ. It was shown in Ref. [1] that the number of superconducting electrons becomes zero at the surface and exponentially rises to its equilibrium value in the bulk of the sample over the distances of order $\sqrt{2}\,\xi$, where

$$\xi^2 = \frac{\hbar^2}{2m|\alpha|}\,. \tag{7}$$

Also introduced was the important problem parameter $\varkappa$, the ratio of the penetration depth λ to the coherence length ξ:

$$\varkappa = \frac{\lambda}{\xi}\,. \tag{8}$$

From formulas (6)–(8) it is clear that the characteristic distances for changes in the magnetic field H and the superconducting order parameter $|\Psi(r)|^2$ become equal at $\varkappa = 1/\sqrt{2}$. The text of Ref. [1] implies a clear understanding by the authors that the value $1/\sqrt{2}$ of the parameter $\varkappa$ is in a certain sense a critical value, and that superconductors with $\varkappa < 1/\sqrt{2}$ and $\varkappa > 1/\sqrt{2}$ can differ considerably in their properties, especially in external magnetic fields.

It is well known that in a certain magnetic field H_{c1} the homogeneous superconducting state breaks down and the metal makes a transition to the normal state. Reference [1] examined, in particular, the behavior of a metal near this critical field. The magnetic field H_{c2} at which a superconducting nucleus can develop in the metal was calculated to be

$$H_{c2} = H_{c1}\varkappa\sqrt{2}\,. \tag{9}$$

According to formula (9), superconductors with $\varkappa \geqslant 1/\sqrt{2}$ can exhibit superconducting nucleation in magnetic fields above H_{c1}. This means that some kind of inhomogeneous superconducting state might also exist in magnetic fields $H > H_{c1}$.

The subsequent text of Ref. [1] shows signs of, shall we say, a struggle between the co-authors over how the $\varkappa \geqslant 1/\sqrt{2}$ system should be dealt with. I allow myself to quote from Ref. [1]: "*Since from the experimental data it follows that* $\varkappa \ll 1$ *... the solution ... possible for* $\varkappa \to \infty$ *does not offer any intrinsic interest... and we shall not discuss it here.*" But I know for certain from my talks with one of the co-authors exactly whose hand inserted this phrase into the typed text after first deleting what was there before. It is true, however, that these talks did not reveal exactly what was deleted: although usually communicative and ready to talk, the only answer my counterpart gave was that he did not remember. To the extent that I knew him — which was quite well, in particular through co-authorship of some (admittedly few) publications — I can imagine only too well how difficult it was to convince him to omit what he considered interesting from his work.

It is likely for this reason that shortly after the quoted passage the question of the value of $\varkappa$ is addressed again [1]: "*Let us now note that for* $\varkappa \geqslant 1/\sqrt{2}$ *a peculiar instability of the normal phase of the metal develops. ... It can be seen that for* $\varkappa \geqslant 1/\sqrt{2}$ *an opportunity appears, with respect to the formation of thin layers of the superconducting phase, in the sense that the solutions of our equations appear with the order parameter* $\Psi \neq 0$. *... This instability is connected with the fact that when* $\varkappa \geqslant 1/\sqrt{2}$ *the surface energy between normal and superconducting phases becomes negative.*" Moreover, the same paragraph containing this last quotation features the Ginzburg–Landau equation for the function Ψ describing a superconducting nucleus, written under the assumption that the magnetic field $H > H_{c1}$ allows the existence of a homogeneous normal state. We will need this equation below, so I reproduce it here (for those who are knowledgeable):

$$\frac{d^2\Psi}{dz^2} = -\varkappa^2\left(1 - \frac{H}{H_{c1}}\,z^2\right)\Psi\,. \tag{10}$$

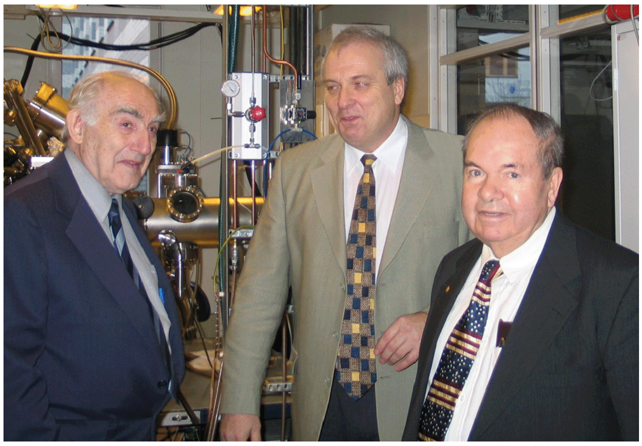
This is the harmonic oscillator equation familiar from quantum mechanics. Its solution, though, is not attained — the second co-author is on guard too — and the following statement appears in the paper: "*It has not been necessary to investigate the nature of the state which occurs when* $\varkappa \geqslant 1/\sqrt{2}$, *since from the experimental data ... it follows that* $\varkappa \ll 1$." Ginzburg's review [2], published in the same year in two issues of *Soviet Physics Uspekhi*, addressed again the question of the magnitude of $\varkappa$ and whether inhomogeneous superconducting states can exist in superconductors with $\varkappa \geqslant 1/\sqrt{2}$ in magnetic fields $H > H_{c1}$. To quote now from the review [2], "*As an examination of the starting equations shows, a peculiar instability develops in the normal phase of the metal for* $\varkappa \geqslant 1/\sqrt{2}$." VL returns to this question also in the review [3]: "*Negative surface energy values would not lead to the destruction of superconductivity in a bulk sample at* $H = H_{c1}$ *but the sample would break instead into alternating superconducting and normal layers.*"

This was how the battle was fought over the question of whose solution was later published by A A Abrikosov in his famous work [4]. It should be emphasized at once that formula (10) is in fact where Ref. [4] starts. The essential point to note here is that, rather than calculating the appearance of a superconducting nucleus against the background of a homogeneous normal state, Ref. [4] was concerned with quite the opposite, the appearance of a nucleus of the normal phase against the background of the homogeneous superconducting state. In his study Abrikosov showed that, for $H > H_{c1}$, a magnetic field penetrates superconductors with $\varkappa > 1/\sqrt{2}$ (or type II superconductors in current terminology) in the form of vortices, peculiar structures with core in the normal state and wherein the magnetic field is maintained by circular currents.

Why L D Landau was so unwilling to clear up the problem of superconductors with $\varkappa > 1/\sqrt{2}$ is now difficult to understand but of considerable interest for the history of physics, especially considering the fact that it was work on these superconductors which brought Abrikosov the Nobel Prize in Physics. For me, Landau's reluctance to be seriously engaged in considering the case of $\varkappa > 1/\sqrt{2}$ is due primarily to his well-known dislike for abstract problems. He used to say that life was finite, all problems cannot be solved, and only those of real-world relevance should be selected. As things were at the time, though, the bulk of experimental data favored $\varkappa < 1/\sqrt{2}$. Besides, it was known that superconductors with $\varkappa < 1/\sqrt{2}$ can also exhibit a state which separates into alternating superconducting and normal phases. Such an intermediate states arise, for example, in a ball placed in a uniform magnetic field.

It should be noted that back in 1937, well before the development of the macroscopic theory of superconductivity, L D Landau made a substantial contribution to solving the problem of the intermediate state. It will also be recalled that by the time of the publication of Ref. [1], Landau had proposed an even more sophisticated version of the intermediate state, one with multiply branching superconducting and normal phases, so an inhomogeneous superconducting state might have been too much hypothesizing for him. It was also clear that superconductors with $\varkappa > 1/\sqrt{2}$ cannot exhibit an intermediate state in the form which had been proposed by Landau and which was examined in experiments. Abrikosov, in his Nobel lecture [5], points to this as a possible reason for Landau's lack of interest in systems with $\varkappa > 1/\sqrt{2}$. Here is a quotation from this lecture: "*It was also established that with increasing value of $\varkappa$ the surface energy between the superconducting and normal layers would become negative, and since this contradicted the existence of the intermediate state, such a case was not considered.*" Anyway, I think that Landau dispelled VL's interest in superconductors with $\varkappa > 1/\sqrt{2}$ for a long time. True, at that time VL was faced with — and solved with efficiency and enthusiasm — an excessively large number of other problems (including highly classified work on thermonuclear fusion); but the after-taste remained. It is therefore quite understandable that VL says, not without bitterness, in his paper "On superconductivity and superfluidity (what I have and have not managed to do)": "*So Landau and I, in fact, overlooked the possibility of the existence of type II superconductors.*" (The paper was first published in Refs [6, 7], there is a mention of it in Ref. [8], and its extended version is presented in book [9]). Exactly who is to blame is too late to find out, nor is this important. What is important is that all the three characters of this story became Nobel laureates.

How long was it that Landau felt dislike for superconductors with $\varkappa > 1/\sqrt{2}$? Judging from Aleksei Alekseevich Abrikosov's statements in his Nobel lecture and repeatedly elsewhere, all the way up to 1955. According to Abrikosov, he did his work on vortices as early as 1952, but was prevented by Landau from publishing it — a ban which was not lifted until the publication of Feynman's paper [10] on vortices in superfluid helium. Abrikosov's statements to this effect were sharply refuted by E M Lifshitz, who claimed that Landau never, in principle, forbade anybody from publishing anything. It is difficult for me personally to tell whether Landau ever forbade publishing something (even if he considered this something not entirely sound), but what I can say is that some publications *were* made contrary to his negative opinion. One such case will be discussed below. Thus, in 1950, when writing the paper [1] jointly with VL, Landau was quite pessimistic about the existence of superconductors with $\varkappa > 1/\sqrt{2}$. The question now arises of whether there is any evidence, even if indirect, that his position in respect to the systems with $\varkappa > 1/\sqrt{2}$ underwent some changes in the period between 1950 and 1955? The answer is: yes, there is, and not just indirect, but by all means direct. Here is what we read in Ginzburg's 1952 *Sov. Phys. Usp.* review [3]: "*L D Landau's idea is — and this is what A A Abrikosov is currently working on — that the value of $\varkappa > 1/\sqrt{2}$ is realized in alloys, whose behavior, as is well known,*[1] *differs from that of 'ideal' superconductors.*" Two 1952 papers in *Sov. Phys. Dokl.* on this subject, written by Abrikosov [12] and Zavaritskii [13] and both presented by none other than Academician L D Landau, are even more telling on the position of Landau.



Nobel laureates in Physics 2003 V L Ginzburg (left) and A A Abrikosov (right) visiting the Department of Condensed Matter Physics (headed by A M Grishin, center) at the KTH Royal Institute of Technology in Stockholm on 12 December 2003.

Experiments by N V Zavaritskii [13] on as-deposited (and hence highly disordered) thallium superconducting films demonstrated a phenomenon which did not fit the framework of Ginzburg–Landau's theory for $\varkappa < 1/\sqrt{2}$. According to this theory, the destruction of superconductivity in strong magnetic fields has to occur in a jump upon reaching the first critical field H_{c1}, i.e., via a first-order phase transition, and the second-order transition is possible only in very thin films. In Zavaritskii's experiments, however, thallium *thick* superconducting films exhibited a second-order phase transition. Abrikosov, on the other hand, showed in Ref. [12] that such a transition is possible in systems with $\varkappa > 1/\sqrt{2}$. This study does not say a single word about the magnetic structure of these superconductors in fields $H > H_{c1}$, nor about the existence of vortices in them. Nor, indeed, could any of those few people who were in frequent contact with Abrikosov in the early 1950s and whom I asked on the matter remember having heard anything about any vortices at that time.

[1] Here VL makes reference to his book *Superconductivity* that was published in 1946 [11] (see Ref. [7, p. 576] for the history of how this book was written).

What this means is that even as early as 1952 Landau not only did not ignore the possibility of existing superconductors with $\varkappa > 1/\sqrt{2}$ but, on the contrary, understood well that they can exist in alloys and dirty metals and actively encouraged work on their study. Whether that year Aleksei Alekseevich presented to Lev Davidovich one more paper on the properties of superconductors with $\varkappa > 1/\sqrt{2}$, specifically on vortices in type II superconductors or on something else, and whether Landau forbade Abrikosov to publish this work, all this is something we are unlikely to ever learn.

A careful textual comparison of papers [3], [4], and [12] gives rise to an interesting and, in a sense, politically loaded question. As we have seen above, it is unequivocal from VL's 1952 review [3] that already in 1952 Landau understood that it is in alloys where the condition $\varkappa > 1/\sqrt{2}$ can be satisfied. Abrikosov's 1952 paper [12] makes no mention whatsoever of alloys. And it is only in his 1957 study [4] that Aleksei Alekseevich points out (in a footnote) that L D Landau was the first to identify alloys as possible candidates for $\varkappa > 1/\sqrt{2}$. As far as the idea of the condition $\varkappa > 1/\sqrt{2}$ being fulfilled in alloys is concerned, it is unclear whether in 1952 Landau communicated it to Ginzburg alone and concealed it from Abrikosov or simply gave Aleksei Alekseevich advice not to mention alloys in paper [12]. Was there perhaps something hidden (or even dark) behind this idea, something which had been important in 1952 and lost its importance in 1957? Sadly speaking, there was. Abrikosov's 1957 paper [4] compares in detail the theoretically calculated properties of type II superconductors with the experimental data that had been obtained back in 1937 by a Kharkov team guided by L V Shubnikov [14]. Given that L V Shubnikov was executed (in the same 1937) as an 'enemy of the people', in 1952 one still could not get away easily after such a comparison — unlike 1957. Because Landau was in Kharkov in 1937, he could not have been unaware of Shubnikov's work but, according to VL, the work was not something Landau thought much of when their joint paper [1] was being prepared. However, Zavaritskii's work [13], whose results were very similar to those obtained on thallium films, caused Landau soon to recognize the value of Shubnikov's work [14]. Whether he mentioned this work to and discussed it with those around him before 1954 is unknown. "Such were the times."

Broadly speaking, it is not that important today whether Landau forbade or did not forbid Abrikosov to publish the paper on vortices. Even though published after Feynman's work on vortices in superfluid helium, Abrikosov's work is surely worth the Nobel Prize in Physics as a work which laid the theoretical foundations of the physics of type II superconductors which are currently widely used in science and technology. Of course, having published the work on vortices before Feynman would have been something to be personally proud of, but, on the other hand, Feynman won his Nobel Prize in Physics 1965 not for vortices in liquid helium but for work in quantum electrodynamics.

There was another problem over which the opinions of the two authors of the work [1] diverged: the magnitude of the effective charge e^* (see the mention above). A comparative analysis by VL showed that the experimental data available at the time and predictions of the Ginzburg–Landau equations agreed much better if e^* was taken to be somewhat larger than, rather than equal to, the electron charge. To my knowledge, this idea was rejected by Landau outright, without giving any serious reasons. As already noted, though, it was hard to distract VL from his ideas, and it was more than once that he revisited this problem.

In particular, in the review [3] VL points out that the question of the magnitude of e^* could be settled by comparing theory with experiment. Let me quote him here: "*The field strength dependence of the magnetic field penetration depth for a massive metal sample is clearly a question for further study — a study which is especially important in that it can provide data for determining the magnitude of $\varkappa$ and comparing it with its theoretical value. A point not to be forgotten is that the transition to the numerical coefficient in the relevant formulas was done under the assumption that the charge occurring in the theory is equal to that of a free electron. This assumption fits naturally into the framework of the theory of Ref.* [1] *and appears to be totally unobjectionable. And yet we do not consider this condition to be absolutely necessary. If the formula with* $e = 4.8 \times 10^{-10}$ cgs *proved to be inconsistent with the data on the penetration depth in a strong magnetic field* (*which, as noted, is not so far the case*), *then it would be necessary to determine from measurements the theory parameter $\varkappa$ and the verification of the theory would consist in measuring the surface energy which is determined by the same parameter*."

VL did implement this program, and the reader is referred to his paper [15] for the results. The analysis of experimental data on the magnetic field penetration depth into a superconductor led VL to conclude that calculated results in the Ginzburg–Landau equation framework are brought into better agreement with experimental data by assuming that the effective charge e^* is not equal to but is 2–3 times larger than the electron charge. In the book [9] (p. 46) it is argued that, in discussing the work of Ref. [15], Landau raised a strong objection to introducing the effective charge. And on the same page we read: "*I could not find arguments against this remark and, with the consent of Landau, I included it in paper* [15]." The objection lodged by Landau was quite serious and had to do with the fact that the introduced effective charge e^* could be dependent on the composition of the material, temperature, etc., and could thereby be a function of coordinates, possibly — and inadmissibly — violating the gradient invariance of the theory. In VL's view, the analysis of experimental data was also a very serious thing. As we see, even a serious objection on the part of Landau did not, by any means, prevent Ginzburg from publishing his work, nor did Landau use his authority to employ any bans or sanctions; even though his authority on the *Sov. Phys. JETP* editorial board was quite sufficient (to say the least) for him to prevent any material from being published.

The development by J Bardeen, L N Cooper, and J R Schrieffer (BCS) of the microscopic theory of superconductivity and the derivation of the Ginzburg–Landau equations from this theory by L P Gor'kov [16] seemed to have resolved the question of the effective charge. According to Gor'kov, the function Ψ entering into the Ginzburg–Landau equations is the wave function of the superconducting electron pairs, and hence the effective charge in these equations is twice the electron charge: $e^* = 2e$. It would seem, then, that VL's prediction was fully confirmed and that there were no problems with gradient invariance violation. In reality, though, things were somewhat more complex than that. VL obtained the value of e^* from experimental data on the magnetic field penetration depth λ. Let us rewrite the expression for this quantity in a somewhat different form from that in formula (6) above:

$$\lambda^2 = \frac{m^* c^2}{4\pi |\Psi_0|^2 e^{*2}} \, . \tag{11}$$

Aside from $e^* = 2e$, it also follows from Gor'kov's work that $m^* = 2m$. There is also another point to note, which remained underemphasized in Gor'kov's work, namely, that the wave function should be normalized to the number of superconducting pairs rather than to the number of superconducting electrons, i.e., $|\Psi_0|^2 = N_s/2$. On substituting these values into formula (11), we see that the value of λ remains the same as for the case of $e^* = e$, $m^* = m$, and $|\Psi_0|^2 = N_s$. In this sense, the values of e^*, m^*, and N_s introduced in Ref. [1] are adequate for describing the diamagnetic properties of superconductors in the context of Ginzburg–Landau equations.

Thus, unfortunately, it is due to inaccurate experimental data that VL's guess $e^* \neq e$ was confirmed. Now the question is raised, is there an experimental evidence directly confirming that charge carriers in superconductors are not electrons but electron pairs with a double charge? The answer is yes, there is, and it is, primarily, the quantization of magnetic flux, an effect first predicted by F London in the late 1940s.

Insight into this effect can be gained by considering a massive superconductor with a cylindrical cavity inside. Let the temperature T be initially above the critical value and let there be an external magnetic field imposed parallel to the generatrix of the cylindrical cavity. If we next cool the sample and make it superconducting, the magnetic field will be expelled from the bulk of the superconductor, but the cavity will retain in it a frozen magnetic flux $\Phi = HS$, where H is the magnetic field strength, and S is the cross sectional area of a cylinder. This flux will be maintained by currents flowing in the superconductor near the cavity walls. Consider a contour C within the sample, drawn at a distance much greater than the magnetic field penetration depth from the cavity walls. Clearly, there is no superconducting current flowing along this contour. Take also into account that the wave function of the superconducting electrons has its absolute value constant along this contour and that the phase $\theta(r)$ of the wave function $\Psi_0 \exp[i\theta(r)]$ can change alone. Using expression (4) for the current then yields

$$\mathbf{j} = \frac{e^{*2} \Psi_0^2}{mc} \left[\frac{\hbar c}{e^*} \nabla\theta(r) - \mathbf{A} \right] = 0 \, . \tag{12}$$

In this equation we are interested only in the expression

$$\frac{\Phi_0}{2\pi} \nabla\theta(r) - \mathbf{A} = 0 \, . \tag{13}$$

Here, Φ_0 is the elementary magnetic flux quantum defined as

$$\Phi_0 = \frac{2\pi\hbar c}{e^*} \, . \tag{14}$$

Integrating equation (13) around the contour C and taking into consideration that

$$\oint_C \mathbf{A} \, d\mathbf{l} = \Phi \tag{15}$$

and that the closed-contour integration of a single-valued wave function gives

$$\oint_C \nabla\theta(\mathbf{r}) \, d\mathbf{l} = 2\pi n \, , \tag{16}$$

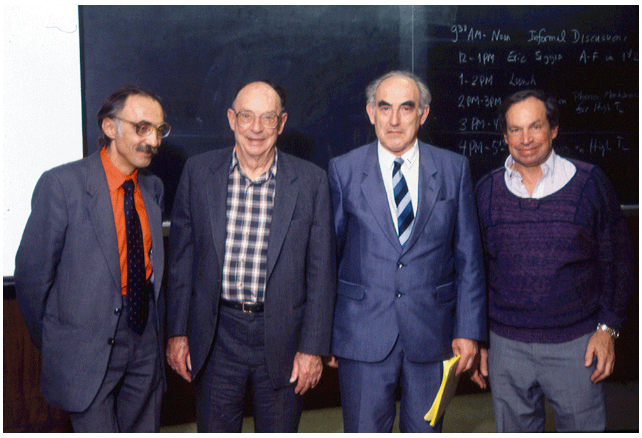


Photo taken at the University of Illinois at Urbana-Champaign in 1987. From left to right: E G Maksimov, J Bardeen, V L Ginzburg, and J Pines.

where n is an integer, we finally obtain $\Phi = n\Phi_0$. This means that the magnetic flux frozen in the cavity inside the superconductor is quantized with Φ_0 as a quantum. The above derivation using the Ginzburg–Landau equation is in fact taken from Ginzburg's work [17].

F London, who was the first to predict the quantization of magnetic flux, found for the flux quantum the expression $\Phi_0 = 2\pi\hbar c/e$, which corresponds to the effective charge $e^* = e$ in Eqn (14). Experimental detection of magnetic flux quantization, which occurred only in 1961, showed that the observed flux quantum was half the value suggested by London, a result which follows automatically if it is taken into account in formula (14) that $e^* = 2e$. Interestingly, VL's idea that the charge of the superfluid electronic component should be of order 2–3 times the electron charge is given respectful mention in Schriffer's book *Theory of Superconductivity* in the section on magnetic flux quantization. For fairness sake, a theoretical explanation of the experimental magnitude of the magnetic flux quantum was published by C N Yang, L Onsager, and J Bardeen in 1961 in the same *Physical Review Letters* issue containing two experimental studies. Underlying their theory was the idea of electron pairs existing in superconductors. Thus, while the diamagnetic properties of superconductors are described correctly by the Ginzburg–Landau equation for the values of charge and mass chosen and the wave function normalized in a standard way, this is not always the case for other properties not directly related to diamagnetism — as the example of the magnetic flux quantum has demonstrated.

As already noted, vortices in type II superconductors also carry a magnetic flux in them. The calculation of the magnetic flux carried by a single vortex also requires taking into account that $e^* = 2e$. A slight inaccuracy in this respect can even be found in, for example, Abrikosov's Nobel lecture. In it, Abrikosov gives a correct ($e^* = 2e$) expression for the magnetic flux quantum $\Phi_0 = \pi\hbar c/e$, but credits London with deriving the correct magnitude of the quantum. In Ref. [4] Abrikosov used the standard Ginzburg–Landau equation and obtained a correct solution for a vortex structure, but did not calculate the flux carried by a vortex. A correct calculation of the flux quantum requires introducing a double charge, but there is no mention of this in Abrikosov's Nobel lecture.

I would like to conclude by returning to Ginzburg's review [3], which primarily presents a discussion (or more precisely, a rather sharp and unflattering critique) of the microscopic theories of superconductivity that were available by 1952. Indeed, by and large, explaining superconductivity theoretically at the microscopic level proved to be a tough nut to crack for many prominent twentieth-century physicists, including Einstein, Bohr, Heisenberg, Landau, Born, and some other lower-ranking but still widely known names.

Here is one of the concluding sentences of the review [3] to illustrate VL's critique: "*All attempts in recent years* (*by, among others, Heisenberg and Koppe, Born and Sheng, Bardeen, Fröhlich, Tissa and Luttinger, Möglich and Rompe*) *to develop a microscopic theory of superconductivity are, in our view, either erroneous or devoid of any positive content. The reasons for this are multiple, including the acceptance of the hypothesis of spontaneous currents, the use of perturbation theory outside its range of applicability, ignoring the bulk of available experimental evidence and of the deep analogy between superconductivity and superfluidity*."

VL's harshest criticism was against the hypothesis of spontaneous currents existing in superconductors even in the absence of external magnetic fields. It is to be remembered that one of the proponents of this hypothesis was L D Landau, as witnessed by a paper he published in 1933. Apparently, by the early 1950s and the publication of the review [3], Lev Davidovich himself realized that this hypothesis was incorrect; otherwise why such a negative assessment of it by VL? It is perhaps not by chance that this work is not found in Landau's collected works published at the end of the last century. While Landau's 1933 work is, naturally enough, referenced in the review [3], it follows from the above quote that VL's criticism is mainly levelled at the publications of the late 1940s and early 1950s (by, among others, the future Nobel laureates Heisenberg, Born, and J Bardeen, the last of whom won this honor twice).

In particular, VL showed [3] that, instead of creating a theory of superconductivity, what Bardeen in fact did in a series of his studies was try to develop the theory of metals possessing strong but nonideal diamagnetism, which is a property real superconductors have. However, VL's work [3] was given close attention by Bardeen. In his review [18], which shortly preceded the publication of the famous BCS theory, he gives a detailed and respectful discussion of VL's paper [3]. Now what was it that interested Bardeen in that paper? Hardly the critique of Heisenberg's and Born's work, and even less so of Bardeen's own results. The most likely answer is the clearly formulated requirements which the future, then still nonexistent, microscopic theory of superconductivity should satisfy. Bardeen calls this VL concept 'Ginzburg's energy gap model'. And rightly so, as the following excerpt from VL's review [3] shows: "*Clearly, the free electron model cannot produce superconductivity, nor can the usual approach of introducing the influence of the crystal lattice* (*in the form of a periodic potential field*) *change anything here. For superconductivity to occur, a free electron approximation-based model must have some kind of gap in the energy spectrum at the Fermi boundary ...* $\Delta \sim T_c$ *in width*." VL then goes on to argue that, because of the isotope effect observed in superconductors (i.e., the dependence of T_c on the isotope mass), there is an additional necessary condition imposed on the gap, $\Delta \approx 1/\sqrt{\varpi}$, where ϖ is the average phonon frequency. A further point made in Ref. [3] was that this gap could not be obtained by perturbation theory. All this meant providing the BCS team with a clear and well-defined working program.

By all indications, both Ginzburg and Bardeen realized that $\Delta \approx 1/\sqrt{\varpi}$ can be obtained in no other way than by considering the electron–phonon interaction. The review [18] makes one further point which is absent from Ginzburg's work, namely, that the gap should vanish at the transition point at $T = T_c$. In his *On Superconductivity and Superfluidity: A Scientific Autobiography* [9, p. 202] VL recalled, somewhat self-critically: "*I realized then and see it even more clearly now that creating a microscopic theory of superconductivity was beyond my powers*." This must have been close to what Bardeen felt — a reason perhaps why Yang's former postgraduate L Cooper, well versed in mathematical techniques beyond perturbation theory, was introduced into Bardeen's team. In our country in 1955 there was no lack of specialists in the theory of electron–phonon interactions or in nonperturbative techniques, and these were highly qualified specialists — but it was not them but Bardeen, Cooper, and Schrieffer who developed the microscopic theory of superconductivity. Why this way and not another is, of course, anybody's guess. It appears to me that one of the reasons is that Bardeen clearly recognized Ginzburg's views on the underlying physics of the microscopic theory, whereas his Russian colleagues did not.

So much, then, for what I could remember and find out about the work of V L Ginzburg in the middle of the last century.

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