Diamagnetism of Superconducting Bodies

In connection with London's letter on the same subject[1] I should like to point out that the striking diamagnetism exhibited by superconducting metals (the so-called Meissner effect) can be interpreted, or rather described, in a way which is both simple and natural; so natural, indeed, that I am afraid it will be considered as quite obvious. Its theoretical implications, however, do not seem to be generally recognized.

Since the induction B represents the space average of the field in the molecular theory, it is difficult to believe that the idea of a superconducting phase with $B\neq 0$ inside is devoid of any meaning whatever, or, in other words, that $B=0$ is anything but an equilibrium condition. If this view is correct, then the inevitable interpretation of the Meissner effect is, that the presence of even a very small field inside the metal must cause so great an increase of the free energy, that the lines of force are pushed out of the metal. The magnetic behavior of ordinary dia-, para- and ferromagnetic bodies can of course be discussed on the same lines, starting from the expression $f=B^2/8\pi\mu$ for the free energy per unit volume. Simply to set $\mu=0$ into this expression for superconductors, would not be very satisfactory. One can easily see, instead, that in order to obtain a perfect Meissner effect at moderate field strengths, it is sufficient to postulate that f is any function of B with $(\partial f/\partial B)>0$ at $B=0$.

The possibility of a phase with $B\neq 0$ had been already considered by Peierls[2] not, however, as an unobservable instable state, but as something which could exist under suitable conditions; he identified it, therefore, with the "transition state." Casimir and Gorter, London[3] and others maintain, on the other hand, that the transition state consists of a fine texture of the pure superconducting phase with $B=0$ and of the normal phase which carries the magnetic flux; Landau's work has greatly strengthened this opinion.[4] Although, therefore, a pure superconducting phase with $B\neq 0$ is never observed experimentally, we will retain the idea of such a phase as a useful and permissible fiction, of a kind which is not unusual in thermodynamics, the best-known example being perhaps van der Waals' instable fluid. The analogy is obvious if one considers the experimental free energy curve for the transition state (Fig. 1(a)) and the corresponding "isotherm" (Fig. 1(b)) (full lines), which one is immediately tempted to accomplish in a manner analogous to van der Waals' isotherm.

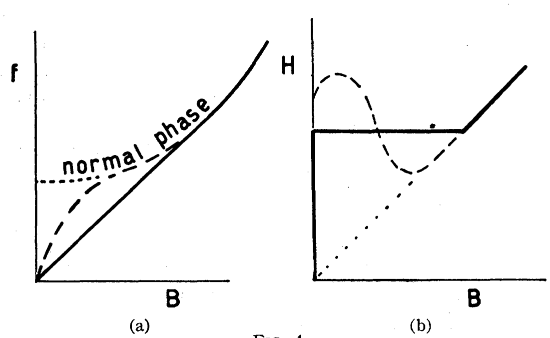

FIG. 1.

(I am much indebted to Dr. Peierls for Fig. 1(a) and the discussion of the transition state contained therein.) Thus the splitting of a superconductor with $B\neq 0$ into a network of two different phases is made to depend on a general theorem about systems with a free energy curve (free energy *vs.* some additive parameter) which is concave towards the axis of abscissae (also kindly pointed out to me by Dr. Peierls), as for instance a system of two incompletely miscible liquids.

The theory will have to explain, of course, why the electronic motions should be so strongly disturbed by even a very small field. (This difficulty, however, is not specifically dependent on our assumption; it is, indeed, inescapable.) The difficulty is best seen in Fig. 1(a); the singularity at the origin ($(\partial f/\partial B)\neq 0$ at $B=0$) is particularly striking, since one would expect here a series expansion like:

$$f=a+bB^2+cB^4+\cdots$$

to be valid. We must then assume that the validity of this expansion is confined to fields so small, as to be unimportant from a macroscopic point of view. (This means that the effect of a magnetic field cannot be treated by means of perturbation theories.) We need not, perhaps be exceedingly disturbed by this fact, such singularities being indeed a common feature of "cooperative phenomena," such as superconductivity undoubtedly is. There is in fact a rather nice parallelism between the two extreme cases of magnetic behavior, the $H(B)$ curve showing a jump at $B=0$ for superconductors and the $B(H)$ curve showing a similar jump at $H=0$, if somewhat idealized, for ferromagnetic crystals.

I finally wish to express my warmest thanks to Professor Heisenberg as well as to Dr. Peierls for much kind criticism and advice.

G. C. WICK

Istituto di Fisica,
Roma, Universita,
July 28, 1937.

[1] London, Phys. Rev. **51**, 678 (1937).
[2] Peierls, Proc. Roy. Soc. **155**, 613 (1936).
[3] London, Physica **3**, 450 (1936); see also reference 2.
[4] London, Physik. Zeits. Sowjetunion **11**, 129 (1937).

Ionization of Mercury Vapor by Positive Potassium Ions

The balance space charge method of detecting ionization, reported at various times by one of the writers[1] has been improved to adapt it to experiments involving extremely feeble ionization. The ions were detected in this method by their influence on a space charge limited current of electrons flowing between a hot tungsten filament and a surrounding metal cylinder. This electron current tended to become unstable because the filament was colder near the ends than in the middle and failed to provide a space charge limited electron current at the ends. This difficulty was eliminated by lengthening the filaments about an inch at each end and placing shielding cylinders around the unusable cooler portions. The anticipated result was obtained, and with the greater stability, greater sensitivity was possible.

The system of accelerating the positive potassium ions was also altered slightly to allow a more intense ionizing beam to enter the detector. The change amounted only to narrowing and lengthening the accelerating slits and placing the accelerators closer together.

The apparatus was tested by looking for the ionization of argon by potassium ions previously observed and reported.[1] The ionization of the argon was so intense that it was estimated conservatively that an effect one-one hundredth as large could certainly have been observed.

The argon was removed and the tube filled with mercury vapor at suitable pressure (10^{-2} to 10^{-1} mm) by closing off the tube with a mercury cut-off and heating the whole tube and cut-off to the proper temperature as given in the *International Critical Tables* for mercury vapor pressures. No ionization of the mercury was observed at all by potassium ions of energies up to 300 electron volts. The result is surprising in view of the fact that ionization of mercury by sodium ions had previously been observed.[2] It now appears as if the ionization by sodium ions if it really exists is the result of a purely fortuitous interaction which can occur between Na^+ and Hg. The work is being checked.

The senior writer wishes to express his indebtedness to Professor R. T. Birge and Professor Leonard B. Loeb for the privilege of working in the physics laboratory of the University of California.

ROBERT N. VARNEY

New York University,
University Heights.

MILTON E. GARDNER
A. C. COLE

University of California,
Berkeley, California,
August 16, 1937.

[1] R. N. Varney, Phys. Rev. **47**, 483 (1935).
[2] Varney and Cole, Phys. Rev. **50**, 261 (1936).

Disintegration of Boron by Deuterons

In a previous experiment, Bonner and Brubaker[1] investigated the neutrons from the disintegration of boron by 0.9 MV deuterons. From the measurement of the energies of recoil protons it was shown that there were neutron lines at 4.35, 6.35, 9.1 and 13.2 MV. When recoil helium nuclei were used to measure the neutron energies, the same lines were observed with the possible exception of the one at 13.2 MV. At that time it was suggested that the absence of high energy recoils might be due to the fact that 13 MV neutrons do not make elastic head-on collision with helium nuclei. The present experiment has been done to clear up the question as to whether the expected high energy helium recoils are missing.

3500 pairs of stereoscopic pictures were taken when boron was bombarded with 0.9 MV deuterons. For this series of pictures the cloud chamber was filled with helium at a pressure of 10.5 atmospheres. The energy distribution of the recoils in the forward direction ($0-10°$) indicated neutron lines with energies of about 4.3, 6.3, 9.1 and 13.2 MV, the last three having relative intensities of 1, 3, and 1, respectively. Thus it seems certain that 13 MV neutrons do make elastic collisions with helium nuclei.

The relative intensity of the 13.2 MV line (as compared to the 9.1 MV group) appears somewhat weaker when detected with recoil helium nuclei than with recoil protons. If this small effect is not due to experimental error it may be due to a difference in the variation of the collision cross section with energy for helium and hydrogen nuclei.

W. E. STEPHENS
T. W. BONNER

Kellogg Radiation Laboratory,
California Institute of Technology,
Pasadena, California,
August 4, 1937.

[1] Bonner and Brubaker, Phys. Rev. **50**, 308 (1936).

Errata: Magnetic Quadrupole Field and Energy in Cubic and Hexagonal Crystals

(Phys. Rev. **44**, 38 (1933))

and

Magnetic Interaction and Resultant Anisotropy in Strained Ferromagnetic Crystals

(Phys. Rev. **52**, 18, (1937))

In analyzing the effects of strain on magnetic interaction in crystals I have had occasion to recompute S_{40}, a coefficient fixing the magnitude of so-called quadrupole energy terms,[1] and have discovered a mistake, made in 1933, in reducing correct lattice sums of fourth-order zonal harmonics to the form chosen for publication. The mistake does not affect any conclusion yet based upon the erroneous values since the resulting discrepancies lie within the present accuracy of measurements. The following are the changes that should be made in the interest of accuracy.

In Phys. Rev. **44**, 38–42 (1933) in the last paragraph on page 40 write $A_0' = (21/2)R_c$ and $A' = -(105/4)R_c$ for $A_0 = (21/2)NR_c$ and $A = -(105/4)NR_c$. In Table III on page 41 the last two entries in the last column should be -1.77512 and -4.30040 instead of -1.77562 and -4.11336.

In Phys. Rev. **52**, 18–30 (1937), in Table II on page 21, the values of S_{40} under "Body-Centered Cubic" and "Face-Centered Cubic" should be -3.10646 and -7.52569 instead of -3.10734 and -7.19838. In Table III, part 1, and in Table IV, the "First Anisotropy Coefficient" (quadrupole part) should be multiplied by 0.99972 for body-centered crystals, by 1.04547 for face-centered crystals, and K_1 changed as necessary to correspond.

L. W. MCKEEHAN

Sloane Physics Laboratory,
Yale University,
New Haven, Connecticut,
August 5, 1937.

[1] For an explanation of this notation see Phys. Rev. **52**, 18–30 (1937).