ON SUPRACONDUCTIVITY AND THE HALL EFFECT

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Read before the Academy Monday, April 24, 1933

I venture to raise two questions, each perhaps heretical, concerning metals in the supraconductive state. The first relates to the Hall effect; the second will be stated somewhat later.

Is the experimental evidence from which Ounes inferred the non-existence of the Hall effect in the supraconductive state of metals satisfactory?

Supraconduction shows itself in two ways. The first way, the one by which the phenomenon was discovered in solid mercury, is that of a vanishingly small difference of potential between two points a considerable distance apart on a piece of metal forming part of a circuit through which a current is maintained by an applied e. m. f., the circuit as a whole not being supraconductive. The second, and even more astonishing, way is by the magnetic moment of currents, set up within the metal by induction, which persist for hours, apparently without diminution, after the inducing influence has ceased to act. Ounes used both of these manifestations of supraconduction in his test for the Hall effect, the metal used being in each case lead.

In his first test, by what may be called the direct method, he used the ordinary arrangement for observing the Hall effect; i.e., placing the plate of metal, carrying an electric current, between the poles of an electromagnet and looking for a transverse difference of potential due to the action of the magnetic field. He worked at two temperatures, 4.°25K. and 2.°8K., each well below the critical temperature of supraconductivity in lead. Using a magnetic field too weak to destroy supraconduction, he found merely that the Hall coefficient, R, was at the higher temperature less than 2×10^{-5}, and at the lower temperature less than 6×10^{-5}. By using a sufficiently powerful magnetic field to abolish the supraconductive state he found a measurable Hall effect, R being 18×10^{-5} at 4.°25K. and 13×10^{-5} at 2.°8K. The most, then, that can be claimed for this test, even if the logic of the method employed is accepted, is that it shows the value of the Hall effect in the supraconductive state of lead to be several times smaller than the value in the normal conductive state.

The second method used by Ounes in looking for a Hall effect in the supraconductive state may be described as follows: A hollow sphere of lead in the supraconductive state was suspended by a torsion spring in a horizontal magnetic field and by variation of the intensity of this field a system of persistent currents was set up within the lead, the plane of these currents being perpendicular to the direction of the magnetic field. By twisting the suspending spring the sphere was turned so that the axis of these currents made an angle, in the horizontal plane, with the direction of the imposed magnetic field. This angle, the amount of torsion in the suspension being undiminished, remained constant, so far as could be seen, for several hours.

Ounes interpreted this experiment as showing that in supraconductive lead there is no Hall effect, no tendency for an electric current to move sidewise through the metal under the pull of the ponderomotive force of the magnetic field, whereas in the normal conductive state of a metal there is, apparently, such a tendency. This conclusion is paradoxical, for it seems to imply that electric currents have in one respect less freedom of motion in the supraconductive state than in the normal state. Accordingly Ounes was led to conceive of the persistent currents as flowing in channels or tubes, the walls of which precluded transverse motion, though the idea of such tubes as existing within a supraconductive metal was no less paradoxical, perhaps, than the phenomenon it was invoked to explain. In this view of the matter he had the support of Lorentz.

The logic of both these tests made by Ounes seems to be open to serious question, for in both cases it is, apparently, assumed that the lines of induction of an external magnetic field can penetrate fully metals in the supraconductive state. We know that in any ordinary good conductor penetration by magnetic flux is sensibly resisted and retarded by the induced Foucault currents which the changing state of induction sets up within the metal. The greater the conductivity, the greater is this resistance to or retardation of the magnetic penetration. Should we not, then, expect this penetration to be very slow, perhaps indefinitely retarded, in the supraconductive state?

Clerk Maxwell in Art. 654 of his "Electricity and Magnetism," 2nd edition (1881), begins a discussion "on the induction of electric currents in a sheet of infinite conductivity," and in Art. 655 he says, "The sheet may therefore be regarded as impervious to magnetic induction, and the lines of magnetic induction will be deflected by the sheet exactly in the same way as the lines of an electric current in an infinite and uniform conducting mass would be deflected by the introduction of a sheet of the same form made of a substance of infinite resistance. If the sheet forms a closed or an infinite surface, no magnetic actions which may take place on one side of the sheet will produce any magnetic effect on the other side."

This statement of Maxwell's seems to apply both to the flat sheet of lead used by Ounes in his first Hall effect test and to the leaden shell used in his second test. To be sure, we are not justified in assuming for actual metals in the supraconducting state a really infinite conductivity such as Maxwell assumes for his imagined sheet or surface. The currents which are set up by induction within, or "on the surface of," real metal in the supraconductive state would not be set up if no change whatever occurred in the amount of magnetic flux within the material. But, as we cannot at present put any definite limit to supraconductivity, we cannot put any definite limit to the slowness with which lines of magnetic induction would penetrate supraconductive metal. If the conductivity of the metal is great enough to allow currents, once started, to persist for hours without perceptible diminution, may it not be great enough to prevent for an equal time perceptible penetration of the metal by magnetic flux?

We have had, then, I think, no satisfactory test for the existence of a Hall effect in the supraconductive state of metals. But apparently such a test could be made. Set up the Hall effect in a metal at a temperature just above the conductive critical temperature and note whether the effect continues when the metal is cooled below this temperature, the magnetic field continuing in force.

Such an experiment would be of considerable theoretical interest. If the Hall effect is immediately dependent on the longitudinal potential-gradient within the metal in the magnetic field, because of some slight distortion produced by this field, the effect should vanish when this potential-gradient vanishes, as it does in the supraconductive state. If, on the other hand, the effect is immediately dependent on the intensity of the longitudinal current, and so on the magnitude of the ponderomotive force exerted on the current by the magnetic field, we should expect it to persist in the supraconductive state.

It may be well to remark here that neither the existence of a ponderomotive force acting on a supraconductor carrying a current through a magnetic field nor such a torque as Onnes found with his leaden shell subjected to in a magnetic field requires us to suppose penetration of the conducting material by the lines of magnetic induction.

According to my conception of a dual electric conduction, only a fraction of the total normal conduction being attributed to the free electrons, the net Hall effect is the resultant of highly complicated conditions and actions in the normal state of metals. I am, however, of the opinion that supraconduction is a much simpler process than ordinary conduction, being effected by means of electrons permanently free so long as the supraconductive state continues. I think that in all probability a Hall effect exists in supraconducting metals and I should not be much surprised to find it of the same sign in all metals in the supraconductive states. I have no confident expectation, however, as to this matter of sign. We know that electrons moving in free space through a magnetic field must drift in the direction of the ponderomotive force exerted on them by this field. But to assume from this that free electrons moving between the atoms of a metal must drift in the direction of the magnetic ponderomotive force acting upon them seems to me a fallacy. Any object merely floating in the air must move in the direction of the wind, but a boat under the action of wind and the reaction of water does not necessarily move in the direction of the wind.

I come now to my second heretical question. Is there conclusive evidence that the persistent currents which Ounes and others have observed are anything more than the aggregate of microscopic electric whirls within the metal? Is there conclusive evidence that the persistent current which is ordinarily assumed to be circumferential within a supraconductive ring or shell is really circumferential?

As bearing on this question I will quote two passages from the 1924 Solvay Conference paper of Ounes. On p. 251 of the Conference Report he speaks of an experiment of Mr. Langevin "où une bobine à circuit ouvert montrait un courant persistant." On pp. 263 and 264 he describes an experiment of his own made with a ring consisting of 24 alternative sectors of tin and lead. These metal sectors, which were soldered together, were thin layers covering a ring of ivory, the junctions between them being on radii of the ring. "Le courant [persistent] fut établi avec un champ perpendiculaire au plan de l'anneau, puis celui-ci fut tourné d'un angle de 30°. Nous avions pensé que nous trouverions un courant qui s'étendrait au bout d'un certain temps [because the soldered junctions were not supposed to be supraconductive] mais l'expérience a montré que des courants continuaient à circuler dans l'anneau et, lorsque l'expérience fut répétée avec l'anneau coupé, celui-ci montra le même moment magnétique."

A chain being no stronger than its weakest link, it seems probable that local currents of very limited radius would be more likely to persist than currents having a long cyclic path. Currents of the latter description may well be induced in a supraconductor when the original penetrating magnetic field is varied but they are likely to die out sooner than the local whirls of current. Is it not reasonable to suppose that we have here an explanation of the fact noted by Ounes, on p. 255 of the Solvay Report, that just after a change of the imposed magnetic field the induced current "varie encore un peu?"

I must, of course, speak very cautiously of this matter, for I have never even seen an experiment on supraconductivity. It seems to me, however, that, for example, the conclusions reached by McLennan[1] and his co-workers as to the existence and strength of circumferential persistent currents in small rings of tantalum, lead and tin, respectively, are open to question. These investigators make no mention of the possibility that the currents are not circumferential. They assume them to be circumferential and on this assumption estimate their strength, from the observed magnetic torque between each supraconductive ring and a neighboring coil of copper wire carrying a current. I believe, however, that all of the phenomena they describe are quite consistent with the supposition that the persistent currents were local whirls within the metal rings, not circumferential currents at all.

Apparently a test of the question here raised could be made by determining the direction and intensity of the magnetic field along an axis common to the ring and the surrounding coil of current-bearing wire. The investigators assumed, I believe, that the magnetic flux along the axis was zero after the persistent current in the ring was established. If my idea of the matter is correct, there should be along this axis a permanent flux corresponding to the direction of the current circulating in the copper coil.

[1] *Phil. Mag.*, 168–180, July (1932).

THE KATABOLISM OF THE NON-VOLATILE ORGANIC ACIDS OF TOBACCO LEAVES DURING CURING[1]

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Communicated May 4, 1933

In a recent study[2] of the changes that take place in the non-volatile organic acids during the process of curing of the leaves of Connecticut shade-grown tobacco, we showed that the proportion of malic acid altered very little, but that the proportion of citric acid increased materially. In those experiments, however, an unsatisfactorily low proportion of the total organic acidity of the leaves was recovered in the form of identified organic acids and only traces of oxalic acid were detected. Further study of the methods of analysis previously employed has now revealed several unsuspected sources of error and a revision of these methods is to be presented in a forthcoming publication from this Station.[3]

The newer modifications of these methods have been employed in a further study of the metabolism of the non-volatile organic acids of tobacco leaves during curing. The material employed consisted of three 50 kilo lots of leaves (8th to 11th leaf) picked the same day (August 1, 1929). One lot was immediately extracted with boiling water, the other two were cured for 12 and for 51 days, respectively, before extraction. The polybasic organic acids were precipitated as barium salts from the concentrated hot water extracts of the tissue by the addition of two volumes of alcohol. The barium salts were decomposed with sulphuric acid, the liberated acids were extracted by ether, esterified and the esters distilled, the individual acids being identified as hydrazides or other suitable derivatives.