Letters to the Editor

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Electron Diffraction by Films of Grease

IN the course of some experiments on the diffraction of electrons by thin films of spluttered platinum, a curious pattern of straight lines with a few diffuse spots (see Fig. 1) was observed by one of us, quite unlike the usual circles of a Debye-Scherrer pattern.

Further investigation has recently shown that it was due to the accidental presence of a layer of tap grease on the specimen. A number of waxes and greases give similar effects when smeared on to a solid and used to reflect a beam of electrons.

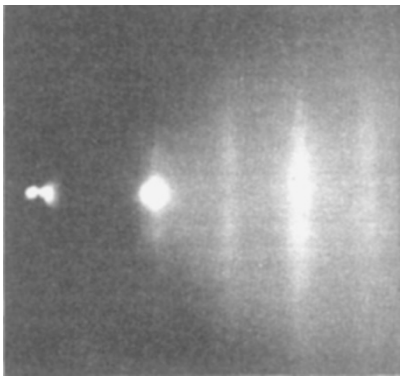

FIG. 1.

The patterns observed can be explained by supposing that the diffracting atoms occur in regular layers parallel to the surface, the layers being equally spaced, but the atoms in each layer being almost at random. Such a distribution would be produced by long-chain molecules normal, or equally inclined, to the surface, like those studied by N. K. Adam. The successive layers are the planes through successive carbon atoms of the chain. It will be noticed that alternate lines are darker than the others; this may be due to the zig-zag nature of the carbon chains which makes alternate atoms different from their neighbours. The spacing corresponding to the distance between a weak and strong line is 2·54 A. which agrees well with Müller's value of 2·537 A. for the distance between alternate carbon atoms.

Sometimes spots appear on the lines. These can be explained as due to a quasi-regularity of arrangement of the chains such as would occur if they were closely packed together.

The distance apart calculated on this assumption agrees fairly well with the cross section found by Müller.

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Convention and Fact

THE work of electrical commissions was one of the first, and remains one of the most successful, examples of international co-operation. The main reason for their success, where so many analogous bodies have failed, is that they confined themselves strictly to the establishment of conventions concerning matters (such as units and symbols) to which conventions are appropriate. Dr. Ezer Griffiths's article in NATURE of December 31, p. 987, shows that the International Union of Pure and Applied Physics does not intend to be bound by any such limitation. Although the title of his article suggests that its inquiries concern only units, it is plain that the Union proposes to discuss, if not to decide, matters of an entirely different nature, to which the conceptions of truth and error are applicable. This new policy demands far more attention than he has given to it.

Dr. Griffiths's second issue does, indeed, involve a matter of pure convention. It is agreed that there are at least two distinguishable magnitudes to which the name *permeability* and the symbol μ have been attached by various writers in the past. Everyone would welcome a decision to which of these magnitudes the name and the symbol are to be confined in the future, and a suggestion for the name and the symbol to be used for the others. But Dr. Griffiths, in stating this issue, asserts by implication two propositions that are not universally accepted and the validity of which is not a matter of convention. They are (1) that every quantity either has dimensions in length, mass and time or is a pure number, (2) that quantities not of the same kind have different dimensions. If it decided that these propositions were true or false, the Union would go far outside the sphere to which its predecessors have confined themselves. But I will not insist greatly on this matter, because it is not clear that the Union is really proposing to make such a decision; the propositions may have been introduced without authority by Dr. Griffiths in an attempt to state the issue concisely.

But there is no doubt of this kind about the first issue. The Union most certainly is trying to establish "a basis on which a connected account of electromagnetic phenomena should rest". For what purpose is this connected account required? Is it education, the statement of a logically complete theory, the formulation of 'consistent' units, or the experimental identification of the units formulated? Is it certain that a single connected account will serve adequately each and all of these purposes? Must the lecturer in electrical engineering work to the same syllabus as his colleague who is instructing mathematicians; and are the Diracs and Eddingtons of the future (or even of the present) to be forbidden to choose a 'starting point' for a theory of the universe inappropriate to immature minds? And what is a connected account? An answer to that question can only be based on some view of the logical structure of science, of the relation between fact and theory and between experiment and calculation. It must involve a choice between the views of this difficult matter taken by (say) Jeffreys, Reichenbach and Bridgman; none of these writers could possibly accept a connected account drawn up by another.

I submit that an International Union can do nothing but harm in even discussing questions that require a decision on such matters. An authoritative