Tolansky[4]. Moreover, the fine structure schemes for the terms $6p^3D_1$ and $6s^3D_2$ reported by him are apparently incorrect, since they can interpret neither his own experimental results nor mine without ambiguity.

Full details will be published in the *Proceedings of the Physico-Mathematical Society of Japan* in the near future.

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Structure of Light Waves

SIR J. J. THOMSON has explained in his recent letter to NATURE[1] that the discontinuity in the structure of light waves (a 'singularity'), to which I directed his attention in my comments[2] upon his views on the nature of light[3], need not be provided, as I wrote, by a permanent "guiding" cylinder of different medium, but can be generated inside the atom and travel with the wave packet.

I do not wish to criticise this hypothesis here, especially as Sir J. J. Thomson apparently does not claim to find yet a complete explanation of the process of ejection of singularities from the atom, but only suggests the way of finding this explanation. However, in view of the fundamental nature of the problem, I think it useful at this stage to formulate briefly the common points and the essential differences in our methods of approaching it.

We both had a common object in view, namely, to connect light and material corpuscles. In pursuing this object, Sir J. J. Thomson in his communication to NATURE, and I in my previous paper[4], both used the same physical idea of axially symmetrical electromagnetic waves propagating *along* the axis, and up to a point followed exactly the same method of mathematical analysis, but the solution of the Maxwell equations which I rejected, mainly in view of the existence of singularities in this solution, proved to be useful to Sir J. J. Thomson just because of it. This was by no means accidental, but very logically followed from the essential difference in the approach to the problem. While Sir J. J. Thomson wanted to introduce singularities (a "core") into the photon, I endeavoured not only to avoid them in the light waves, but also to eliminate them from the "material" corpuscles (and the matter as a separate entity as well)[5].

Which method of approach may prove more fruitful can be judged only from future developments. While I am naturally inclined to regard the method on which I am working as more promising, I fully appreciate the importance of Sir J. J. Thomson's contribution in suggesting the alternative method.

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[1] NATURE, **137**, 823, May 16, 1936.
[2] NATURE, **137**, 663, April 18, 1936.
[3] NATURE, **137**, 232, February 8, 1936.
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[5] *Phil. Mag.*, **19**, 934, *ibid.*, **20**, 417 and 641 (1935).

Viscosity of Air

IN deducing the value of the electronic charge, Millikan assumed for the viscosity of air $\eta_{23} = (1822 \cdot 7 \pm 0 \cdot 9) \times 10^{-7}$. Recently Kellström[1] has obtained the value $\eta_{23} = (1834 \cdot 8 \pm 3 \cdot 0) \times 10^{-7}$, by a rotating cylinder method. This higher value, together with Millikan's data, leads[2] to $e = (4 \cdot 816 \pm 0 \cdot 013) \times 10^{-10}$ E.S.U., in agreement with the X-ray-grating-crystal estimates of e.

During the last few months, I have measured the viscosity of dry air (at atmospheric pressure) by a capillary tube method. The ends of a wide-limbed 'U' tube containing paraffin oil (of density $0 \cdot 87$) were connected by a *closed* system consisting of two capillary tubes in series. Initially the oil is displaced, and in attaining equilibrium it drives air through the capillaries. Care was taken to avoid constant and systematic errors; and two 'U' tubes, two sets of calibrated capillary tubes, and two methods of drying the air were used. The experiments were carried out at temperatures ranging from $13 \cdot 6°$ C. to $16 \cdot 9°$ C. Assuming that η increases by $4 \cdot 93 \times 10^{-7}$ per degree centigrade rise in temperature[3], I find

$$\eta_{23} = (1834 \cdot 7 \pm 0 \cdot 8) \times 10^{-7}.$$

I had expected that my result would confirm the lower estimate of the viscosity; but it is in good agreement with Kellström's value.

Using Millikan's and the more recent oil-drop data[4], together with my value for the viscosity, I deduce the two estimates :

$$e = \begin{Bmatrix} 4 \cdot 816 \pm 0 \cdot 005 \\ 4 \cdot 800 \pm 0 \cdot 005 \end{Bmatrix} \times 10^{-10} \text{ E.S.U.}$$

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May 4.

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[2] R. T. Birge, *Phys. Rev.*, **48**, 918 (1935).
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An Interesting Infra-Red Absorption Band in Fused Quartz

To check the calibration of an infra-red spectrometer, we attempted to locate a fairly sharp band of fused quartz reported by Dreisch[1] as existing at $2 \cdot 75\mu$ with an intensity of absorption of 75 per cent when a 5 mm. specimen was used, and located by Parlin[2] at $2 \cdot 71\mu$ with an intensity of absorption of 55 per cent in a 2 mm. specimen. To our surprise, no such band appeared, although we used several different plate and lens specimens, the thickest sample having a thickness of 5 mm. In the meantime, a paper by Drummond[3] has appeared, presenting a careful plotting of the spectra of crystalline and fused quartz. With 6 mm. samples of the latter, he found a $2 \cdot 73\mu$ band with a 20 per cent absorption. His paper is particularly interesting because it plots for comparison the values of the absorption coefficient K for fused quartz and for the ordinary and extraordinary rays of crystalline quartz. Throughout the $4-8\mu$ region, the graph for fused quartz assumes a kind of average position for the other two, which are qualitatively similar to each other. But in the 3μ region there is a profound difference, a fact discovered by Dreisch[1], who ascribed it to a destruction of the crystal lattice upon fusion, the inference being