the centres of an iodine molecule and an air molecule in collision would be $(\sigma_{I_2}+\sigma_{air}) = 5 \cdot 516 \times 10^{-8}$ as calculated from viscosity data. On the other hand, the diffusion coefficient $D = 0 \cdot 0815$ enables the quantity $(\sigma_{I_2}+\sigma_{air})$ to be calculated from Chapman's formula for diffusion:

$$(\sigma_{I_2}+\sigma_{air})^2 = \frac{3\sqrt{c^2{}_{I_2}+c^2{}_{air}}}{32\,\pi\,\nu\,D}.$$

The value of $(\sigma_{I_2}+\sigma_{air})$ at 25° is found to be $4 \cdot 783 \times 10^{-8}$. If we subtract $1 \cdot 86 \times 10^{-8} = \sigma_{air}$, we have $\sigma_{I_2} = 2 \cdot 923 \times 10^{-8}$ at 25°, as calculated from the diffusion coefficient. This is considerably smaller than the value $3 \cdot 656 \times 10^{-8}$ calculated above from Rankine's viscosity data.

The discrepancy is in the same direction as that found with gases, but is rather greater in magnitude, as is to be expected for a vapour.

Calculation of the actual molecular radius (*i.e.* of the attracting elastic sphere model) from diffusion data is admittedly unsafe, even for comparatively permanent gases. In the case of iodine the corresponding calculation from viscosity data would seem to be open to some question; the specific heat of iodine vapour is changing rapidly at the temperature of Rankine's experiments, presumably owing to a progressive increase of vibrational energy; this might be expected to affect somewhat the value of the Sutherland constant obtained from the temperature coefficient of viscosity.

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The Oogenesis of Lumbricus.

I TRUST that I may be allowed a small space in order to reply to certain criticisms of my work on *Lumbricus terrestris*, which have been made in a recent paper by Prof. J. B. Gatenby and Dr. V. Nath (*Q. J. Mic. Sci.*, **70**, pp. 371-389, 1926).

In the first place I am accused of not referring to much of the literature on the subject. But much of the work which they cite as having been omitted was published after my paper had gone to press in the early summer of 1924, and Gatenby and Nath still ignore what Dr. Cannon pointed out in his contribution to the previous discussion in these pages (NATURE, **116**, 1925, p. 97), that my work was more in the nature of a critique of the methods of argument used by this school, rather than of their observed results.

In the second place, my technique and powers of observation are called in question. The thread-like mitochondria in my preparations are stated to be "artefacts due to long exposure of the fresh ovaries to water of unsuitable tonicity, or to unsuitable fixation." The ovaries used for the work were not brought into contact with any fluid other than those of the worm itself before being transferred immediately into fixative. Further, Prof. Cowdry states ("General Cytology," p. 317, l. 11) that in cell injury "first we often observe a breaking up of filaments into granules (this may also be induced by faulty technique). . . ." The point need not, I think, be laboured.

Again, my critics "are at a loss to understand Harvey's difficulties in giving a clear account of the behaviour of the Golgi apparatus . . .," because of the favourable nature of the material used. In this connexion it is interesting to recall Nath's reference (*Pro. Roy. Soc. Lond.*, B., **98**, 1925; footnote to p. 54) to this organ as being remarkable and representing Calkin's yolk nucleus. The latter structure has now been shown to consist of mitochondria.

To take one more criticism, they state that there is no yolk in the egg of Lumbricus at all, that I have mistaken degenerating Golgi bodies for yolk droplets and also for Calkin's yolk plates, which I found in all my preparations. I have re-examined my slides, and find that I cannot modify my former statements. The yolk plates, yolk droplets, and Golgi elements are present side by side in many preparations, and there can be no mistaking one for the other. There is, however, little yolk present compared with the amount found in many molluscs, ascidians, and arthropods, but perfectly distinct, yellowish droplets are present in many young oocytes, and these, in the older oocytes, become colourless.

Finally, Prof. Gatenby and his school are content to accept the view that yolk is a general term covering anything in the cell which cannot otherwise be identified, and originating from all possible primordia. Is it not time that cytologists made some attempt to bring the observations on oogenesis to as orderly a state as are those on spermatogenesis? Possibly there will be found to be much minor variation, but the materials from which yolk is derived will, it is to be hoped, be reduced to two or at most three definite sources in the ovary.

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Magnetic Storms and Wireless Communication.

IN the issue of NATURE of November 6, p. 662, Sir Joseph Larmor directs attention to the fact that during the magnetic storms of October 14 and 15, the Canadian beam signals were greatly reduced in strength. The explanation that appeals to me is connected with the fact that ionic refraction is not the only factor which determines long distance short wave ($\lambda < 60$ m.) transmission. Together with this there is the effect of energy absorption by collisions of electrons with molecules. That this absorption plays an important part in transmission seems to me to be upheld by a considerable body of evidence, not the least of which is that afforded by the action of magnetic storms.

The ionic absorption factor for the ray which traverses the ionised medium is well known to be $4\pi c/\rho$, where ρ is the effective resistivity (at the frequency concerned). This quantity can be expressed in the form:

$$\frac{1}{\rho} \propto \frac{Ne^2}{m} \frac{T^2}{\tau},$$

where T is the time period of the waves and τ the average time between successive collisions of an electron with a molecule or positive ion.

The essential point is that the absorption factor is proportional to N, the number of electrons per c.c., a fact which is obvious from the physical consideration that the total energy drawn from the waves is proportional to the number of collisions, *i.e.* N.

We have only to suppose that there is an appreciable increase in N, due for example to the injection of charged particles or increased ionisation as a result of the solar activity, to account for the increased absorption and consequent decrease of signals during sunspot activity. That absorption plays a very considerable part in long distance short wave transmission is evidenced by the fact that the range of a station in summer and in low latitudes is less than that in winter and high latitudes.

On the ionisation theory the numerical value of N will be greater in summer and at low latitudes than