

## Letters to the Editor.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, nor to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

### Ether Drift Experiments.

THE editor has kindly shown me the proof of a letter from Sir Oliver Lodge, relating to my ether drift experiments, and in particular to the account of this work recently given at the Royal Institution. Sir Oliver has in a very courteous manner described the principal argument put forth, and, as he says, the matter is now before the scientific world, *sub judice*. Notwithstanding this, it may not be out of order to make further explanation of some of the points raised by Sir Oliver.

It seems necessary to direct attention again to the fact that this experiment has never given a true zero or 'null' effect in general; it has only given a 'negative' result, that is, it has answered 'no' to a specific question. Following the report by Morley and Miller, in 1905, Prof. Einstein made the hypothesis that the motion of the observer produces no effect upon the apparent velocity of light. This hypothesis has been given in hundreds of books as the correct interpretation of the experiments, and has been accepted, perhaps without fully examining the original papers. Michelson and Morley, in 1887, said: "Considering the motion of the earth in its orbit only . . . the relative velocity of the earth and the ether is . . . certainly less than one-fourth of the orbital velocity. . . . In what precedes only the orbital motion of the earth is considered. If this is combined with the motion of the solar system, concerning which but little is known with certainty, the result would have to be modified." In 1905 Morley and Miller, using a much larger interferometer, included in the calculations with the orbital motion of the earth a presumed motion of the solar system of twenty kilometres per second towards the constellation Hercules (still leaving the orbital motion predominant), and came to practically the same conclusion. But, in each of these reports the effect to be expected was calculated in advance, both as to direction and magnitude, and then the observations at two different times of day (as determined from the expected result) were combined so as to add together the expected components, and so as to cancel effects other than those being looked for.

In all of these observations there was a definite positive result, which is observed as a periodic displacement of the interference fringes; but, for the two calculated times of day, the phases of the positive periods differed in such a way that when the two sets of readings were added, they neutralised each other and left only the very small result which was correctly reported as that obtained for the orbital component of any existing ether drift. It is this positive effect, then eliminated, that is now being examined. While it is not large, it is by no means insignificant; the present amount of observed relative motion is 10 kilometres per second with a probable error of  $\pm 0.5$  kilometres per second. As this effect was not 'expected' it was not easy to interpret. The history of the experiment has been given in *Science* for June 19, 1925, and in a more concise report in *NATURE* for July 11, 1925, p. 49.

The re-examination of all of the observations from 1887 to 1926, is the subject of a paper presented to the National Academy of Sciences in Washington on April 26, 1926, and not yet published. It is hoped to give a summary of this paper shortly in *NATURE*.

Sir Oliver Lodge asks what would appear if the results were plotted on the hypothesis that the south side of the housing is warmer than the north side, or with regard to other conditions. It is exactly for answering these questions, and others, that the experiments have been continued over a period of six years, in which time the thousands of readings have been made. Every disturbing cause that could be thought of has been exhaustively studied; among these are: daily and annual variations in temperature, meteorological conditions, radiant heat, magnetism, magnetostriction, differential gravitation, gyrostatic action, influence of method of illumination, transparent and opaque coverings of the light path, speed and direction of rotation, lack of balance in the rotating parts, position of the observer, and other conditions. One after another, these disturbances have been shown *not* to produce the observed effects. Finally, it has been possible to combine into one logical solution *all* the readings made to test all these varying conditions, without any omissions (excepting a few readings made under abnormal conditions, such as artificial heating), without any corrections, and without assigning any weights. This solution is entirely consistent with the observations of Michelson and Morley of 1887, and with those of Morley and Miller of 1902-1906.

The observations with the interferometer are made to detect a periodic shift of the fringes, periodic in each half-turn of the instrument; it is not the absolute position of the fringes that is important, but rather the periodic, vibratory change in the position of the fringes, as the interferometer turns through  $180^\circ$ . This can be detected best by having the instrument in slow and uniform movement, while the observer watches the fringes continuously. The instrument turns once in about a minute, and floats so freely that when once started it will continue in rotation for more than two hours without being touched. A series of readings is made in less than fifteen minutes.

The important part of the argument is that the reported effect has always been present, and is evident in every single observation and not merely in the mean; it is not related to the instrument, or to its surroundings; neither is it dependent upon day and night, or summer and winter; it is clearly shown to be directly related to sidereal time, that is, to a cosmical cause. This is explained, with numerous curves showing the results of the 1925 observations, in *Science* for May 1, 1926. The final arguments are based upon four very complete series of observations made at Mount Wilson for the epochs April 1, August 1, and September 15, 1925, and for February 5, 1926, thus covering various seasonal conditions, and various orbital positions of the earth.

In making the observations, two independent quantities are noted, the direction in which the interferometer points when the effect is a maximum, and the amount of the periodic displacement of the interference fringes. Each of these two sets of readings leads to an independent determination of the right ascension and declination of the apex of the supposed motion of the earth in space. It is very significant that these two determinations are wholly concordant.

DAYTON C. MILLER

### Note on the Law of Radiation.

IN connexion with the distribution of black-body temperature-radiation among different wave-lengths, the law of that distribution suggested by the late Lord Rayleigh for long wave-lengths is now well known to require the interesting and important correction factor  $\frac{x}{e^x - 1}$  (where  $x$  is  $\frac{hc}{RT\lambda}$  or, as Dr.

Jeans has virtually pointed out, very nearly  $32\pi^3 \frac{\epsilon^2/K}{RT\lambda}$ , the electronic charge being  $\epsilon$ ), in order to adapt it to all wave-lengths. This factor could not have been deduced from dynamical reasoning, even with the help of the electron and the laws of probability, without the postulate of a previously unsuspected discontinuity in the interaction between ether and matter. Not because the expression for  $x$  (which in itself is full of speculative interest) contains any discontinuity, but because the denominator of the correction-factor ( $e^x - 1$ ) suggests, as my brother has pointed out (NATURE, May 30, 1925), a continuous accumulation at compound interest with the periodic emission of a dividend. The steady growth of a tree through a summer, with occasional spasmodic drop of an apple to a lower energy level, may be in some respects a permissible analogy.

There seems some risk that in order to emphasise the novel character of this discovery of Max Planck's, supplemented as it was by Einstein, teachers of senior physics may convey the impression that Lord Rayleigh had enunciated his law as complete; whereas he was under no such mistaken impression. He knew that his law was a partial one, and could apply only to long waves. I had thought of quoting from his original 1900 Memoir on the subject, together with a footnote added in 1902, in support of this contention (see vol. 4 of Rayleigh's "Collected Papers," p. 485), since there seemed a risk of its being overlooked by readers of Dr. Jeans's admirable Report to the Physical Society on "Radiation and the Quantum Theory." But I have found that the true position is clearly admitted and stated by Dr. Jeans in his book on "The Dynamical Theory of Gases" (p. 359, third edition).

Rayleigh's law, I may remind readers, is that if  $E$  is the total radiated energy at the temperature  $T$  (which energy may be plotted as an area with a wave-length base) its distribution among different wave-lengths (or the ordinates of the continuous-spectrum energy curve) will be :

$$dE/d\lambda = 8\pi RT\lambda^{-4}.$$

Rayleigh threw out his suggestion as the simplest definite form of Wien's thermodynamically-derived and well-established so-called "displacement law" connecting energy at each wave-length with absolute temperature :

$$\lambda^{-5}\phi(\lambda T).$$

The new form represented a special case; and the replacement of  $\lambda^{-5}$  by  $\lambda^{-4}T$  was convenient for several reasons: (1) Because it emphasised the fact that atomic energy in each mode must be proportional to  $T$  (which was disguised in Wien's form); (2) because it brought in the known gas constant instead of an unknown function; and (3) because it was in accordance with the Maxwell-Boltzmann law of equipartition.

Rayleigh expected that his expression might represent the true law of radiation when  $\lambda T$  was great; and so it has turned out. The law is true so far as *matter* is concerned; that is, it is true if an atom radiated, as a sounding body does, into a material medium. That something more was re-

quired to bring in non-matter, like the ether, was clear, because for some unknown reason equipartition would not apply to that; even proportionality to temperature need not hold in the ether. No one could deduce the complete law without further discovery. Each step from matter to ether, or back again, demanded the quantum factor, and does demand it wherever such interaction occurs. The gradual Keplerian manner in which the true law was guessed was a triumph of genius. Bohr's subsequent pictorial representation of the quantum at work, in the structure and behaviour of a radiating atom, was another. In this connexion I find that an ancient address of Clerk Maxwell's to the Chemical Society in 1875 (which I had the privilege of hearing, and which is reported in NATURE, vol. II, pp. 357, 374) contains the following now rather amusing anticipatory paragraph (see also p. 435, vol. 2, of Maxwell's "Collected Papers"):

"The theory of the possible vibrations of a molecule has not yet been studied as it ought, with the help of a continual comparison between the dynamical theory and the evidence of the spectroscope. An intelligent student, armed with the calculus and the spectroscope, can hardly fail to discover some important fact about the internal constitution of a molecule."

We are living in a period of remarkable discoveries, which rather tend to put in the shade some of the older physics. Nevertheless, as an intermediate stage, complete so far as matter is concerned, Lord Rayleigh's partial statement was most useful. It never pretended to be more complete than it was, and it is only fair to his well-known care and scrupulous caution to emphasise this.

OLIVER J. LODGE.

Normanton House,  
Lake, Salisbury.

### Plastic Deformation of Single Metallic Crystals.

IN a letter to NATURE (May 22, p. 720), Messrs. Millington and Thompson discussed the wedge-shaped fracture which ordinarily results from a tensile test on a single metallic crystal. On the assumption of uniform slip on a number of parallel planes they calculate the magnitude of an angle which they assume to be the angle of a wedge-shaped fracture produced by the slip. May I point out that uniform slip of the type they consider, on one or more sets of parallel planes, would result in uniform extension together with a uniform change in the cross-section over the whole of the portion of the specimen which is slipping, and consequently no wedge-shaped fracture could result? The angle which was calculated in the letter referred to is the inclination of the axis of the 'slipped' portion of the specimen to the axis of the portion which has not slipped, and this has no connexion whatever with the angle of a wedge fracture.

In the earlier stages of a tensile test on a single crystal the act of slipping on any plane increases the resistance of that plane to further slip. This has the effect of distributing the slip more or less evenly over all the planes of the same type so that uniform extension results. At some stage, however, this strengthening action appears to become exhausted and a zone of weakness is set up. The final wedge fracture results from non-uniform slip in this region, the extent of the slip on the set or sets of planes involved becoming progressively greater as the weakest point is approached from either side. No calculation of the angle of the final fracture can be made without some information as to the nature and extent of the zone of weakness referred to, and there appears to be