Letters to the Editor.

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Solidification of Helium.

ON June 25 helium was compressed in a narrow brass tube forming communication between two German silver tubes. The brass tube and part of the two German silver tubes were in a liquid helium bath. At a pressure of 130 atmospheres the tube system appeared to be blocked. When the pressure was diminished by 1 or 2 atmospheres the tube system was open. The temperature of this experiment was somewhat uncertain. By diminishing the pressure of the liquid helium bath the same phenomenon was observed at a temperature of about 3·2° K. at 86 atmos., and at a temperature of about 2·2° K. at 50 atmos. From the regularity of the phenomenon it appears that we were observing the solidification curve of helium. This method of observing solidification has indeed already been used by Kamerlingh Onnes and Van Gulik in preliminary measurements on the curve of solidification of hydrogen.

A repetition of the experiment on July 1 confirmed the early observations. At 4·2° K. helium solidified at 140 atmos. The solidification curve was prolonged to 1·1° K., and the helium solidified then at 26 atmos. The exact numerical data will be given elsewhere. The solidification curve bends so that at the lower temperatures it shows a tendency to become parallel to the axis of the temperatures. So far as can be ascertained from these observations, helium is expected not to have a solid-liquid-gas triple point.

Finally, helium was compressed in a glass tube provided with a magnetic stirrer after the pattern of Kuenen. The observations on the solidification of helium were confirmed. The stirrer was seen to stick when the helium solidified. In one experiment part of the substance was liquid and part solid. One could hammer the solid block with the stirrer that was in the liquid part. A limiting surface between the solid and the liquid could not, however, be seen. Solid helium forms a homogeneous transparent mass, the refractive index of which probably differs extremely little from that of the liquid.

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Supplementary Note on Radiation.

PERMIT me to make a little correction to my letter on page 891 (June 26) about the law of radiation. It is usual to quote Rayleigh's law in the form there given, namely, $8\pi RT\lambda^{-4}$; the $8\pi R$ part is, however, due not to Rayleigh but to Dr. Jeans (see *Phil. Mag.* for July 1905). In Rayleigh's 1900 paper he left the constant undetermined. Afterwards, in NATURE for May 18, 1905, he concluded, from simple gas-theory, that the constant would be $64\pi R$; but Jeans speedily pointed out a source of error, and made the constant $8\pi R$, a correction which Rayleigh at once accepted (see his "Collected Works," vol. 5, p. 253; also p. 248).

It is interesting to note that had the possible modes of vibration been one-dimensional, as in sound, the numerical part would have been 4π; in light the transverse vibrations have two modes open to them. This makes the constant 8π; while if, as in an elastic solid, vibrations had been possible in all three directions of space, the constant would have been 12π (see Jeans's "Report," p. 14). In no case could it be 64π; but that was a slip, due to counting some integers twice over.

Another point in which my letter might be misleading is that the dynamical proof of the continuous-spectrum law of radiation had given no indication that the result would only be true for long waves. It must have been clear by common sense that it did not hold for short waves, but no reason for this was suggested by orthodox dynamics. The serious discrepancy remained a puzzle, until it was solved by the quantum.

In other words, as I express it, neither gas-theory, probability, nor dynamics was competent to express fully the interaction between matter and ether. An expression obtained by attending to continuous equi partition of energy in matter alone, was bound to be incomplete; while if the doctrine of continuous equipartition was extended to the ether, with its apparently unlimited degrees of freedom, the result was impossible. Indeed, thirty years previously, Maxwell had emphasised this outstanding difficulty of molecular theory, in his lecture on the "Molecular Constitution of Bodies," reported in NATURE, vol. 11, pp. 375-6 (or "Scientific Papers," vol. 2, pp. 433-438), and had decided that whatever the constitution of the ether might be it could not be molecular. Discontinuous partition, as represented by the quantum, enabled the true radiation law to be obtained; and the puzzle was thereby shifted to an explanation of the quantum itself—a problem which can scarcely be solved until we possess more knowledge about the intimate structure of the ether.

OLIVER LODGE.

Prof. Miller's Ether Drift Experiments.

THROUGH the courtesy of Prof. Miller I have been made acquainted with the results of his February series of observations made on Mt. Wilson, to be published in the *Proc. Nat. Acad. Sci.*, Washington. I am sorry to say that my opinion concerning the significance of the observed displacements disagrees with his so completely that I cannot attribute the effect to any cosmic cause.

The calculations of Prof. Miller and his collaborators lead to the conclusion that an ether drift directed towards a point in the constellation Draco (R.A. 17 h., Decl. +68°) would agree best with the observed effects. The drift is assumed to be caused by a motion of the solar system towards the given direction with a velocity of approximately 200 km./sec. A partial drag of the ether is supposed to reduce this velocity to 10 km./sec. at the surface of the earth, thereby annulling the influence of the orbital motion.

My objections against these assumptions are laid down in a paper published recently in the *Zeitschrift für Physik* (vol. 35, p. 723, 1926). The theoretical curves of the line displacements as plotted against the azimuth of the apparatus are given there for different directions of the ether drift. A comparison with the mean value of Prof. Miller's observations shows systematic deviations as large as the full amount of the effect occurring at certain hours of the day. The asserted good agreement between the assumed ether drift and the observations is due to the fact that Prof. Miller has arbitrarily displaced the theoretical curves, giving the azimuth of drift as a function of sidereal time to match the empirical curves. This procedure may be justified in all cases where only the *shape* of the curves is essential. In the present case, however, the absolute values of the curves play a fundamental rôle.

As Prof. Miller rightly remarks, the projection of a

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