

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.]

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.

W. H. KEESOM.

University of Leyden.

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 equipartition of energy in matter alone, was found 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. $+68^{\circ}$) 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

fixed direction in space on the horizontal plane ought to move equally to the east and to the west during a sidereal day. What actually happens is the occurrence of an effect pointing towards the north-west quadrant of the compass in about ninety-five per cent. of all observations. This fact seems to be fatal to the assumption of an ether drift of constant direction towards a certain point of the heavens. If the effect were really genuine it would prove the existence of a north-west drift of the ether accompanying the earth's rotation. The velocity of the drift would be at least 10 km./sec., whereas the velocity of the daily motion of a point of the equator is only about five per cent. of this amount. The advocates of the ether will find it difficult to account for a whirling motion of the ether round the earth with a velocity surpassing that of the earth's rotational motion about twenty times.

Apart from the systematic deviations, there are large irregular discrepancies in the observations. The mean value of the ether velocity at a certain hour of the sidereal day taken from twenty single observations differs sometimes by more than 100 per cent. from the total average for the same hour taken from all observations of a given epoch.

It appears that on account of the extreme difficulties of the measurements, which were vividly described in Prof. Müller's presidential address before the Kansas Meeting of the American Physical Society, the results of the Michelson experiments are less trustworthy and less stringent than they have been supposed to be. The Trouton Noble experiment offers considerably less difficulty, and its negative result may be regarded as more convincing. Dr. R. Tomaschek, of the University of Heidelberg, has repeated this experiment at an altitude of 11,400 ft. at the Jungfrauoch, Switzerland (*Ann. d. Phys.*, vol. 78, p. 743, 1926). The observed effects did not exceed the errors of observation, and it was concluded that there is no relative velocity of the ether greater than 3 km./sec. at the given altitude.

This result gives rise to doubts concerning the significance of the interference shifts observed at Mt. Wilson. The doubts are augmented by the great irregularity of the measured shifts and by the predominance of the north-western direction in the diurnal variation of the effect, which is inconsistent with the assumption of an ether drift of constant 'absolute' direction.

My conclusion is, therefore, that the effect must not be attributed to any cosmic cause at all, but may be due to local disturbances.

HANS THIRRING.

Institut für theoretische Physik,
Universität, Wien,
June 19.

The Molecular Spectrum of Carbon Dioxide.

THE value of the specific heat of carbon dioxide indicates a triangular molecule. Bjerrum (*Deutsch. Phys. Ges.*, 16, 737, 1914) has made a study of this molecule and has decided that the atomic nuclei lie at the corners of an isosceles triangle the apex angle of which is either 145° or $40^\circ 6'$. Dennison (*Phil. Mag.* (7), 1, 195, 1926) decides in favour of the former angle. Such a model, according to these authors, should be characterised by three fundamental vibrational frequencies. Each tries to associate the three well-known bands of absorption at 14.66μ , 4.25μ and 2.73μ with these predicted frequencies. The model does not anticipate a fourth weak band which I found at 2.02μ (*Phys. Rev.*, 26, 469, 1925), especially since this is not harmonic with any of the other three.

Dennison associates the 4.25μ band with a motion

of the carbon nucleus perpendicular to the bisector of the apex angle, and the other two bands with motions of the oxygen nuclei along the line joining them, the carbon nucleus moving along the bisecting line in such a way as to keep the same molecular configuration. In attempting to identify the observed relative intensities of these three bands with his predicted amounts, he meets with success in the case of the two longer wave-lengths but fails in the case of the third one. For whereas this band should be only 1/180th so intense as either of the other two, it actually is found to have 1/4th their intensity.

A new explanation for the presence of the 2.73μ band as well as of the weaker 2.02μ band has been sought on the basis of combinations of the frequencies of the strongest two bands. Table I. shows that the frequencies of the two weaker bands are approximately equal to $\nu_a' + 2\nu_a$ and $\nu_a' + 4\nu_a$ respectively, where ν_a' and ν_a are the frequencies of the 4.25μ and 14.66μ bands.

TABLE I.
ABSORPTION BANDS OF CARBON DIOXIDE.

λ in μ .	ν in mm^{-1} .	Designation.	Calc. ν .	Diff. Per cent.
14.66	68.9	ν_a		
4.25	235.5	ν_a'		
2.73	366.3	$\nu_a' + 2\nu_a$	373.3	1.9
2.02	495.0	$\nu_a' + 4\nu_a$	511.1	3.2

Emission bands corresponding to each of the carbon dioxide absorption bands, including the newly observed weak one at 2.02μ , have been found in the spectrum of the Bunsen flame. Table II. shows the values of these bands, as well as the agreement of the frequencies of the weakest two with values calculated on the basis of combinations similar to those of Table I.

TABLE II.
EMISSION BANDS OF CARBON DIOXIDE.

λ in μ .	ν in mm^{-1} .	Designation.	Calc. ν .	Diff. Per cent.
14.1	70.8	ν_e		
4.4	227.0	ν_e'		
2.76	362.0	$\nu_e' + 2\nu_e$	368.6	1.8
1.99	502.0	$\nu_e' + 4\nu_e$	510.2	1.6

It will be noticed that in every case the calculated combination frequencies are somewhat larger than the observed values. This should be anticipated if we are to associate the terms $2\nu_a$, $4\nu_a$, etc., with approximate harmonic vibrations of some portion of the molecule; for the so-called harmonic vibrations arise from causes which also produce deviations from true overtone relationships, namely, the non-linearity of the force and the finite amplitudes of motion of the nuclei. Thus in the case of hydrogen chloride the first harmonic absorption frequency differs by 1.7 per cent. from a true multiple relationship (Brismade and Kemble, *Proc. Nat. Acad. Sci.*, 3, 420, 1917), while extrapolation of the frequencies which I have observed to be characteristic of the C-H bond in all organic substances (*Phys. Rev.*, 27, 298, 1926) indicates a corresponding deviation of 1.85 per cent.

Comparison of the above data shows an increase in frequency in the case of the longest wave-length band in passing from absorption to emission, while an opposite change characterises the 4.25μ band.