Letters to the Editor.

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Atmospheric Refraction.

I PERCEIVE on further consideration that my suggestion (NATURE, January 5, p. 8) of a spherical wave-front in place of a plane one to account for the discrepancy between the coefficient of terrestrial refraction as derived by Dr. Hunter from Mr. Mallock's proposition and the coefficient deduced from trigonometric levelling operations under ordinary conditions is inapplicable, for the reason that in practice we have to deal, not with a single point-source of light, but with an assemblage of point-sources the wave-front from which is sensibly plane.

There must, therefore, be some other explanation for the disagreement between the two values of the coefficient. I find on examining Dr. Hunter's figures (NATURE, August 11, 1921, p. 745) that the almost exact two-to-one ratio between the values, which was suggestive of a simple geometrical explanation, is illusory owing to an unfortunate slip of Dr. Hunter in confusing sea miles with statute miles. The earth's radius in sea miles is not 3960, but 3437, and the resulting value of k from Mr. Mallock's proposition is not 0.133, as Dr. Hunter states, but 0.116. Using the Continental definition of k , this gives $k = 0.232$, which agrees with the value found by Jordan's formula (quoted in my former letter) for isothermal conditions at 0°C . As to the remaining discrepancy between 0.116 or 0.232 and the values of k (on the two definitions of it) which are usually found to hold in practice, it is clear both from Comdr. Baker's letter (NATURE, January 5, p. 9) and from Jordan's formula that this is readily accounted for by temperature considerations. It is only under isothermal conditions, such as seldom or never occur in practice, that Mr. Mallock's result can be even approximately true; it was evidently incorrect to consider, as Mr. Mallock did in his reply to Dr. Hunter, that temperature effects could produce merely a difference in the result of 1 or 2 per cent. per 1000 ft. The mistake of assigning an insignificant part to temperature considerations is one which is very easily fallen into by any one who first considers the isothermal condition with its accompanying relation between density and pressure, because of the small effect which the existence of a temperature-gradient has on the rate of decrement of pressure as distinguished from density.

The great interest of Mr. Mallock's demonstration lies in its deriving the refractive index under certain conditions in a very simple and elegant manner from the velocities of light in air and *in vacuo* and the height of the homogeneous atmosphere. What has led to some confusion is the omission from the enunciation of the proposition of the qualification that it holds true only for isothermal conditions and for air at 0°C .

Putting p for pressure, ρ for density, and h for height, we may take the refractive index as depending only on dp/dh , as Mr. Mallock does, so long as we keep to isothermal conditions. But once we depart from these conditions, as is inevitably the case in practice, we must take it as depending on dp/dh , which no longer corresponds to dp/dh . We can, however, extend the simplicity of Mr. Mallock's reasoning to the condition of a linear temperature-

gradient provided we replace the height of the homogeneous atmosphere, $-pdh/dp$ (or "pressure height," as Prof. Everett preferred to call it in his "C.G.S. System of Units"), by the "density height," $-\rho dh/d\rho$, which latter may be much greater than the "pressure height" under ordinary conditions.

Comdr. Baker lays great stress on the fact that the path of the refracted ray cannot be a circular arc unless the temperature-gradient is linear. This stress is justifiable, especially from the seaman's point of view of the problem; for the temperature-gradient in the air immediately over the sea is frequently far from linear, and in navigation horizontal sights must always be taken fairly close to the surface of the sea; moreover, it will seldom happen that the most favourable time of day can be chosen for observations at sea.

The moral is that in navigation too much reliance should never be placed on the results of observations made on a single bearing whenever the accuracy of the tabular value of the dip has to be assumed. But the land-surveyor is much less limited by conditions than the seaman; he can generally keep his lines well above the ground by observing between points of considerable elevation, he can choose that time for his observations when refraction is least likely to be abnormal, and he can usually get an adequate check on his results for the elevation of a point by observing it from a number of others at different distances and comparing the results. As a matter of experience, it is found by surveyors in many countries that during the afternoon hours, when refraction is steadiest, the assumption that the temperature-gradient is linear and the path of a nearly horizontal ray consequently a circular arc is tolerably near to the truth, at any rate for lines which do not run very close to the ground for any considerable part of their length. This follows from the close concordance between the trigonometric levels obtainable for the same point from stations at very different distances, when the observations have been taken under proper conditions and worked out by the usual formulae.

JOHN BALL.

Survey of Egypt, Cairo, February 11.

Diffraction by Molecular Clusters and the Quantum Structure of Light.

THE investigations on the molecular scattering of light now in progress under the writer's direction (regarding which previous communications have been published in NATURE) have brought to light some very remarkable cases in which the observed facts are in sharp contradiction with the theories of light-scattering based upon Maxwell's electromagnetic equations. According to the Einstein-Smoluchowski formula for the scattering power of a fluid, viz.

$$\frac{\pi^2 RT\beta}{18 N\lambda^4} (\mu^2 - 1)^2 (\mu^2 + 2)^2,$$

the intensity of the diffracted beam should be proportional to the compressibility β of the fluid and should thus be very large near the critical temperature as the compressibility is there great. Experiments by Keesom and Kammerlingh Onnes have confirmed this result in the case of ethylene vapour over a range of a few degrees above the critical temperature. The scattering powers of liquid carbon dioxide and vapour for a considerable range of temperatures below the critical point have been determined in the writer's laboratory by Mr. K. R. Ramanathan, who has discovered that the formula is approximately valid only for a range of a few degrees below the critical temperature, and then

falls off much more rapidly than according to the formula. These observations are significant in view of the observation by the present Lord Rayleigh that the scattering power of saturated carbon di-oxide vapour at 21° C. is only 102 times that of the gas at atmospheric pressure, whereas according to the Einstein-Smoluchowski formula, it should have been 855 times as great.

The failure of the formula indicated above is especially surprising in view of its successes in other directions, namely, in the case of gases obeying Boyle's law, in the case of liquids under ordinary conditions, and, with certain restrictions, even in the case of solids. In attempting to find an explanation of the failure, at first sight one naturally seeks to find some flaw in Einstein's theory, or in the application of it, but the very successes of the formula in other cases would tend to discourage such an attempt. The formula was deduced by Einstein by applying Boltzmann's principle of entropy-probability in order to find the magnitude of the fluctuations of density of the fluid arising from thermal agitation and deducing the light-scattering due to these fluctuations by application of Maxwell's electromagnetic equations. It is clear that density fluctuations due to thermal agitation must occur; that their magnitude is proportional to the square root of the compressibility of the medium as contemplated in the theory may be confirmed independently by identifying the thermal energy of the molecules with the energy of sound-waves of all possible wave-lengths in an enclosed volume of the fluid and equating the energies. Further, the idea that the non-uniformity of the density of the medium is the factor determining light-scattering, at least according to the wave-theory, is confirmed by the very complete analysis of the problem given by the late Lord Rayleigh in one of his final papers (*Phil. Mag.*, Dec. 1918, p. 449). How, then, are we to escape the difficulty?

A very luminous suggestion made by Jeans in his "Dynamical Theory of Gases" (page 203) is here of great help. Jeans distinguishes between two kinds of clustering in fluid media, *mass-clustering* and *molecular-clustering*, and points out that they tend to become identical at the critical temperature. Einstein's theory is based on the idea that the fluctuations of density and the resulting scattering of light are both due to *mass-clustering*. If, however, we assume that it is *molecular-clustering* that is of importance and results in an increased scattering of light, it is easy to see that in the case of molecules such as carbon di-oxide, which are ordinarily non-associated, the clustering of molecules would only be appreciable near the critical temperature, and that at lower temperatures the clusters would rapidly break up and resolve themselves into single molecules. A double molecule would scatter four times as strongly as a single molecule, a triple molecule nine times as strongly, and so on, and if we assume that the energy-effects of separate molecules or groups are additive, and calculate the number of associated molecules from thermodynamic principles, it is easy to give the theory quantitative expression and explain the increased scattering near the critical point, and the rapid fall at lower temperatures.

But the fundamental difficulty remains, why the *mass-clustering* considered by Einstein does not, as it should, according to the classical wave-theory of light, give rise to an increased scattering of light?

To the present writer, at any rate, it appears that this contradiction of the electromagnetic theory by experience may have to be classed with its other known failures in the theory of photo-electricity and other modern fields of inquiry. We may, in fact, have to adopt the quantum theory of the

structure of light as propagated in space (and not only when it is absorbed or emitted) in order to explain the facts of molecular diffraction. Fuller experimental data which are now being obtained in the writer's laboratory may pave the way towards the clearing up of this fundamental question.

C. V. RAMAN.

210 Bowbazar Street, Calcutta, March 2, 1922.

The Radiant Spectrum.

PROF. RAMAN in his reply of February 9 to my criticism of his first letter of September 1, does not refer to the fundamental difference of opinion between us. For it was the statement "the phenomenon is due to diffraction by the corneal corpuscles," to which I took exception, because I could not find in his letter, or in Brewster's paper, any evidence on which such a conclusion could be based.

With regard to the corneal corpuscles, Schafer writes in his "Essentials of Histology" (p. 363, edition 6), "Between the laminae (of the cornea) lie flattened connective tissue corpuscles, which are branched and united by their processes into a continuous network; there is, of course, a corresponding network of cell spaces." Since, then, the corneal corpuscles lie within the substance of the cornea, their optical effect will depend on their opacity to light, or on the difference between their refractive index and that of their surroundings. Now if there was opacity, or a difference in refractive index, they should be visible under the microscope. But such is apparently not the case. Staining with hæmatoxylin or some other suitable reagent, is necessary in order that they may be visible, and therefore their opacity, or difference in R.I., must be slight. We conclude, therefore, that they will cause but slight diffraction in a ray of light passing through the cornea. In shape the cells themselves are *highly* irregular, and they average in man 20-30 μ in diameter. Their nuclei in man are roughly oval in shape, about 16 μ in diameter. In order that these structures should produce the type of diffraction pattern described by Prof. Raman, there should be two sets of them, nearly circular in outline, with diameters of 13 μ and 7 μ respectively. But a further point arises: Prof. Raman describes slight relative movements on the part of the diffraction pattern, which he compares with those which occur when a film of milk on glass is held in front of the eye. This movement, he states, ceases if the eyelids and eyeball be kept motionless for a short time. Could the corneal corpuscles undergo this movement lying as they do in lacunae in the substance of the cornea? And even if they could, why should their motion cease when the lids and eyes are kept still?

Not only has no evidence been advanced by Prof. Raman in support of his statement that the corneal corpuscles are responsible for the diffraction phenomena, but also the shape, size, situation, and optical properties of these structures would appear to be antagonistic to the view.

With regard to the scattering of light by a prism, the following experiment will be found to demonstrate the effect. On the bed of a spectrometer are placed, base to apex, two glass prisms of equal dispersion, with optically good and clean surfaces (see Fig. 1).

FIG. 1.—L.S., Arc or Pointolite. C., Condenser. S., Slit. T., Lens of Collimator. P. & P., Prisms. T., Lens of Telescope. M., Metal Strip. E., Eye of Observer.

The telescope eye-piece having been removed, a