Letters to the Editor.

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Atmospheric Refraction.

THE correspondence on terrestrial refraction from Mr. Mallock and Dr. de Graaff Hunter in NATURE of June 9, p. 456, and August 11, p. 745, raises a paradox which I think must have puzzled many readers of NATURE besides myself. Mr. Mallock is, of course, quite correct in stating that the diminution of density of the air observable under ordinary conditions is practically linear for moderate increases of height above the earth's surface, and that, consequently, the refractive index of the air may for moderate increases of altitude be taken as diminishing linearly at such a rate that it would reach vacuum value at the height H of the homogeneous atmosphere. Dr. Hunter is equally correct in pointing out that Mr. Mallock's reasoning, based on the above-mentioned observational fact, leads to a value of k , the coefficient of terrestrial refraction, which is almost exactly twice as great as that found by observation under ordinary conditions. Dr. Hunter does not, however, point out what I think is the real fallacy in Mr. Mallock's argument.

The difficulty is not to be got over by any consideration of temperature-gradient in the air, although it is well known that variations in the temperature-gradient constitute the chief cause of variations in terrestrial refraction. The only way in which temperature-gradient could affect Mr. Mallock's result would be by its requiring a change in the value (4.32 sea-miles) which he adopts for the height H of the homogeneous atmosphere. Whether we calculate H for air at a uniform temperature, as is usually done, or on the assumption of a diminution of temperature with increase of height at the rate ordinarily observable (say 1°C. for each 200 metres), we obtain a value of H which is nearly the same as that used by Mr. Mallock in his argument.

May I suggest that the solution of the riddle is to be found in Mr. Mallock's supposition of a "plane vertical wave-surface starting from P," whereas the rays of light from a terrestrial point must give rise to an approximately spherical wave-surface? In the diagram (Fig. 1), which represents a vertical section through the homogeneous atmosphere with the curvature of the earth neglected for simplicity, a plane wave-surface HPO would change its position to BAC in the time t , where HB and OC are proportional to the velocities of light at H and O. But in that time rays from a point P would reach points D and E, such that $PD - PA = \frac{1}{2}(PB - PA)$ and $PA - PE = \frac{1}{2}(PA - PC)$, because the average velocity along PD would be the mean of the velocities at P and H, and, similarly, the average velocity along PC would be the mean of the velocities at P and O. It is easy to see that this gives a radius of refractive curvature exactly twice as great as that found by Mr. Mallock, and consequently leads to a value for the coefficient of terrestrial refraction which is in agreement with observation and with the tables ordinarily employed by navigators for the dip and distance of the sea horizon.

It may be worth while to mention here a very likely source of confusion in comparing the values of the coefficient of terrestrial refraction k found by different observers under different conditions, and especially by observers in different countries. There are two definitions of k in use by surveyors, one of

which makes its numerical value double that given by the other. An assistant of mine who read Dr. Hunter's letter was greatly surprised at his statement that $k=0.133$ "is not a value ordinarily met with in practice," because in Egypt we ordinarily use $k=0.13$, and our trigonometric levels derived from observations made in the afternoon hours when refraction is at its minimum and steadiest value are found to agree surprisingly well over great distances with those found by spirit-levelling. The explanation of the apparent discrepancy between Dr. Hunter's statement and our experience is that we follow the Continental practice in defining k as the ratio of the curvature of the refracted ray to the curvature of the earth, while Dr. Hunter and most English writers define it as half this quantity.

It does not seem to be very generally known that a rational formula for calculating the coefficient of terrestrial refraction at any point where the barometric pressure, air-temperature, and temperature-gradient are known was advanced by Jordan so long ago as 1876. This formula, which is given, together with an account of the theory on which it rests, in Jordan's "Handbuch der Vermessungskunde," Band 2, is

$$k = 0.2325 \frac{B}{760(1 + \alpha t)^2} (1 - 29.35n),$$

where k is the coefficient of terrestrial refraction, defined as being the ratio of the curvature of the ray to that of the earth, B the barometric pressure in mm., α the coefficient of expansion of air at constant pressure, t the air-temperature in degrees C., and n the temperature-gradient in degrees C. per metre of height.

Jordan's theory is probably not quite complete, in that it omits any consideration of variations in the humidity of the air; but it does take account of variations of pressure, temperature, and temperature-gradient, and these are probably the principal factors affecting the value of k . The resulting formula is very simple and easy of application, and, so far as I have been able to test it in the Egyptian deserts, I have found it to give results which are in good agreement with those of observation. It appears also to accord very satisfactorily with Indian experience; for when applied in the two examples given by Dr. Hunter in his letter, one at sea-level and the other at an altitude of 19,000 ft., it yields (allowing for the difference in the definition of k) results identical with those which were found by Dr. Hunter to agree well with numerous observations.

JOHN BALL.

Survey of Egypt, Cairo, December 14.

IN NATURE of June 9, p. 456, a letter appeared from Mr. A. Mallock giving a proof that the path of a nearly horizontal ray through the earth's atmosphere is a circle of about 14,900 miles' radius, and later (August 11, p. 745) Dr. de Graaff Hunter, of the Indian Survey, wrote controverting Mr. Mallock's statement, and asserting in effect that the radius of

FIG. 1.