the deviations of which differ by only 1·5 or 2 2 degrees, and by making an accurate geometrical diagram it will be seen that these beams never entirely separate out from each other, but continue to overlap no matter how far one passes away from the prism. Thus under the conditions of the experiment it would hardly be possible to detect the existence of separate beams at all. Blondlot does not mention the use of a lens to focus the rays, and if one were used it would be necessary to re-focus it separately for each beam, according to the different values of the indices of refraction.

In measuring wave-lengths of light by a diffraction grating, everyone knows how enormously the intensity of the incident light is reduced in the different diffracted images, yet Blondlot was able, apparently with the greatest ease, to split up a divergent pencil of n-rays, coming through a slit 5 mm. wide, into eight divergent homogeneous beams by passing it through a prism, then to take only as much of one single beam as would pass through a second slit 1·5 mm. wide, having perhaps 1/50 the intensity of the original beam, and after allowing this small fraction of the whole radiation to fall on a grating, to detect the existence of, and measure up accurately, a central image and no less than twenty diffracted images, the intensity of each of which must have been considerably less than 1/1000 of the original beam. All this was done with a radiation so feeble that no observer outside of France has been able to detect it at all.

But it is questionable from another point of view whether the different diffracted images could be observed at ·all, at least in certain cases, under the conditions of the experiment, for the slit was quite broad, 1·5 mm., and apparently no lens at all was used to bring the spectra to a focus. The central beam and the various diffracted beams would thus continue to broaden out and become more and more diffuse. Now using the ordinary formula for a plane grating and calculating back from one of Blondlot's wavelengths, 0·0081μ, it follows that for radiation of this wavelength the distance apart of adjacent spectral images at a distance, say, of 50 cm. from the grating would be only 0 8 mm., or considerably less than half the breadth of the central beam itself. This is with the grating mentioned as having 200 lines to the millimetre. With the grating containing 50 lines to the millimetre, the distance between adjacent spectral images would be only 0·2 mm., or less than 1/8 the width of the central beam. In other words, there would be no definition, and the broad central band, together with the broad diffracted bands, would hardly separate out at all, even using as large an angle of incidence as 75 degrees.

In measuring wave-lengths by means of Newton's rings, it is well known that the rings produced by a fairly bright source of light, such as a sodium flame, are quite faint, and a dark background is necessary in order to see them at all. Yet if we accept one of Blondlot's wave-lengths, 0 0085μ, as correct, he must have succeeded in counting up no less than 70 n-ray rings in the space between two adjacent sodium rings, and this by the use of a source of radiation only 1/8 the intensity of the original source, as the latter must have been split up into homogeneous beams before the rings were formed. It would be interesting to know just where the phosphorescent screen was placed in this experiment, as the rings are formed in the thin air gap between the lenses, and the eye must be focused on that point to see them sharply. But of course, the screen could not be put between the lenses, as the latter could not then be brought into close contact, and if it were placed anywhere else the rings would be somewhat blurred.

C. C. SCHENCK.

McGill University, March 10.

Escape of Gases from Atmospheres.

IN a recent number of NATURE (January 14) there appears an article on the above subject by Dr. G. Johnstone Stoney, in which he corrects a statement in the literary supplement of the *Times* of December 25, 1903, in regard to the escape of helium from the earth's atmosphere. The permanence of planetary atmosphere is of so much importance to science that I trust I may be permitted through your columns to add a word to what Dr. Stoney stated in his letter of January 14.

The problem of the escape of gases from planetary atmo-

spheres has, as Dr. Stoney remarked, been approached by two distinct methods :—

(1) The inductive method, by taking the conditions as they appear in nature and arguing upward to results concerning our atmosphere which may then be applied to other planetary atmospheres.

(2) The deductive method, by using the laws which are acknowledged to appertain to gases under known conditions, and by assuming conditions under which these laws are known to apply for the outer stratum of our atmosphere, and to apply these laws to the escape of molecules from the atmosphere.

The first of these methods was made use of by Dr. Stoney in his memoir on "Atmosphere upon Planets and Satellites" in the *Astrophysical Journal*, 1898. In this paper Dr. Stoney argues that since helium is coming into the atmosphere at a greater rate than it is being removed from the atmosphere by natural carriers, and since it has not been proved to be increasing as a constituent of the atmosphere, it must be escaping from the outer stratum of the atmosphere, and in doing so must attain a speed of 9 27 times its mean velocity at a temperature of $-66°$ C. —the velocity that would carry it beyond the earth's attraction.

In the *Astrophysical Journal*, January, 1900, I have shown by the Maxwell-Boltzmann distribution of velocity that if we assume the outer stratum of the atmosphere to be at a temperature of $5°$ C. with a density equal to that at the earth's surface, and to be composed entirely of helium, only $10·34 \times 10^{-4}$ c.c. of helium would be favourably situated, and would attain a velocity sufficient to escape in 10^7 years—the computed age of the earth ; and also that if we assume a temperature of $66°$ C., the number of c.c. that would attain to that velocity would be $22·10 \times 10^{-24}$, or less than a single molecule in the same length of time ; while if we assume a temperature of $-180°$ C., which I believe to be much more probable for the temperature of the ultimate stratum, only $91·6 \times 10^{-86}$ c.c. will escape, which, of course, means that an atmosphere of helium at normal pressure and at the average yearly temperature could not escape from the earth.

If these results, deduced from the kinetic theory under conditions to which it is generally acknowledged that the kinetic theory does apply, have any value whatever, it seems to me that they completely refute the assumption made by Dr. Stoney that helium is escaping from our atmosphere. But these results do not stand alone as evidence of the permanency of our atmosphere. Prof. Bryan by an entirely different method (see *Transactions* of the Royal Society, London, 1901) reaches the same conclusion, both in regard to hydrogen and helium.

In the *Monthly Weather Review* for August, 1902, I further discussed the probability of molecules, in a highly attenuated atmosphere, reaching velocities much greater than under normal conditions, and it is there shown that no conceivable effect could influence the results sufficiently to allow the escape of helium from the atmosphere. In further evidence of the fact that the latter view has been accepted by other writers, I may cite the work of M. E. Rogovsky (see *Astrophysical Journal*, November, 1901), who, after having published the above article, published a note in NATURE (July 3, 1902) in which he stated that his results would have to be modified in accordance with the results obtained for the escape of gases according to the kinetic theory.

In conclusion, permit me to say that although I fully recognise the imperfection of the kinetic theory in dealing with problems of attenuated atmospheres, yet I believe that the results arrived at under the special assumptions made will have to stand until it can be shown by other *a priori* reasoning that these conclusions are not within the limits of the probable results, *i.e.* that the escape of helium from our atmosphere is practically *nil*.   S. R. COOK.

Case School of Applied Science, Cleveland, O.,
February 22.

Demonstration of Magnetostriction by Means of Capillary Ripples.

IN his experiments on the change of length by magnetisation, Joule ("Papers," vol. i. p. 50) mentions that "the expansion, though very minute, is indeed so very rapid that it may be felt by the touch." If everybody were endowed