to that of propagation of the primary rays as experimentally determined is within 5 per cent. of that calculated on the ether pulse theory (see *Phil. Mag.*, February, 1908). If Prof. Bragg can suggest a distribution of ejected pairs that will produce such close agreement between the calculated and experimentally determined intensities, it will be time to consider the theory further.

My argument has not been concerned with γ rays, but with the type of radiation with which I am experimentally more familiar—X-rays of ordinary penetrating power.

University of Liverpool. CHARLES G. BARKLA.

The Wave-length of Röntgen Rays.

IN his theory of thermodynamical radiation, Planck has found the simple law $e=h_0 n=h_0\frac{c}{\lambda}$, where e is an element of energy, $h_0=6.55\cdot10^{-27}$ a constant, n the frequency, λ the wave-length of an electromagnetic resonator, c the velocity of light; according to this "elementary law" the energy of an electromagnetic resonator changes during a period by a multiple of e.

Applying Planck's elementary law on the emission of Röntgen rays by stopped kathode ray particles, I have found the following (*Physik. Zeitschr.*, viii., 882, 1907). Let $e_k=\frac{m_0 v^2}{2}$ be the kinetic energy of a kathode ray, ϵ its electric charge, V the freely traversed potential difference, the total kinetic energy may be, by stopping, transformed into energy of radiation. The smallest wave-length of the emitted Röntgen radiation is then $\lambda_k=\frac{2h_0 c}{e_k}=\frac{2h_0 c}{\epsilon V}$; for a working potential difference of 60,000 volts on a Röntgen bulb λ_k becomes $6\cdot10^{-9}$ cm. Haga and Wind (*Ann. d. Phys.*, x., 305, 1903) have found by their experiments on diffraction for the wave-length of the used Röntgen rays the value $\lambda=5\cdot10^{-9}$ cm.

It is clear that the reversed phenomenon—the transformation of Röntgen rays into kinetic energy of electrons—gives the emission of secondary kathode rays by Röntgen rays, or more generally by light. I have deduced from Planck's elementary law that the maximum of the velocity of secondary kathode rays is independent of the nature and temperature of the radiating body, but inversely proportional to the square root of the absorbed wave-length. This statement is in agreement with the observations of Innes (Proc. Roy. Soc., lxxix., 442, 1907); the observations cannot be explained by the hypothesis of J. J. Thomson and W. Wien that the emission of secondary kathode rays is produced by some radio-active process.

It may be added that Planck's elementary law is also confirmed by my observations on the Doppler effect on Kanalstrahlen; the simple or two-fold minimum of the intensity in this effect is explained by that law (*Physik. Zeitschr.*, viii., 913, 1907). Applying the law to a hypothesis of the origin of banded spectra, it is possible to calculate an inferior limit for the spectral position of the banded spectra of the saturated and "loosed" valencies in chemical compounds (*Physik. Zeitschr.*, ix., 85, 1908).

J. STARK.

The Orientation of the Avebury Circles.

IN Sir Norman Lockyer's notes on the orientation of stone avenues printed in NATURE, January 16, pp. 249–257, in dealing with Avebury, he founds his argument as to the existence and direction of the Beckhampton avenue upon Stukeley's statement as to the remains of it visible when he wrote in 1724. He then passes to the Kennet avenue, and says:—

"As will be seen from the map, this avenue apparently was connected with the southern circle as the Beckhampton one was with the northern one. If this were so, certainly the enormous bank, erected apparently for spectacular purposes, which is such a striking feature of Avebury, was not made until after the Kennet avenue had fallen out of any astronomical use."

In accordance with this statement, Sir Norman Lockyer marks on the map reproduced to illustrate his notes the course of the south-eastern or Kennet avenue as a straight line making directly for the centre of the southern circle *across the existing bank and ditch* well to the left of the present road leading to Kennet. In this he entirely ignores the fact that Stukeley (in the map given by Long, "The Temple at Abury surveyed by Dr. Stukeley in 1724") marks two prostrate stones of the avenue actually in the existing gap in the earthworks by which the Kennet road enters Avebury, and furthermore notes that they were "broke 1722." Aubrey, too, in his plan taken in 1663 (reproduced in Jackson's "Aubrey," p. 319), shows seven stones of the avenue as lining the sides of the existing road *immediately* on its leaving the gap in the mound. Lastly, there is standing at this moment a few yards on the right-hand side of the Kennet road a large stone which is the only one now remaining of those seen by Aubrey and Stukeley at the point where the avenue struck the earthwork circle. This stone was apparently not noticed by Sir Norman Lockyer.

Surely if anything can be said to be certain at all about Avebury, it is that the Kennet avenue joined the outer circle through the existing gap in the rampart by which the Kennet road enters it to-day, and did not make straight for the centre of the southern circle over the bank and ditch as shown in Sir Norman Lockyer's plan. Theoretically, perhaps, it ought to have done so, but as a matter of fact, if any weight is to be attached to the statements and plans of Aubrey and of Stukeley, and to the position of the one existing remnant of the avenue on the spot to-day, it did not. In the interests of accuracy it seems desirable to point this out. ED. H. GODDARD.

Stability in Flight.

MR. MALLOCK (January 30, p. 293) seems to presume, as a great many others do, that an apparatus on the aëroplane principle "demands constant attention on the part of the aëronaut" to maintain its stability in the air. We are apt to get ideas from watching the behaviour of little bits of paper floating in the gusts of wind, and to forget that the flying machine of the future may run into tons of weight. Though a frail canoe may easily capsize, the big ship seldom turns over even in the roughest of seas. Even so primitive a contrivance as we may presume that of Mr. Farman to be is some 33 feet across and weighs, complete, half a ton. Such a structure is not easily upset by mere puffs of wind. But it is also evident that a machine can be designed possessing nearly perfect automatic stability. Langley's model, away from all human control, flew steadily on over the billows of the air for a minute and a half. A well-designed and well-balanced machine is automatically stable without any pendulums or other appliances; in fact, it forms a pendulum of itself. B. BADEN-POWELL.

32 Princes Gate, S.W., February 1.

REFERRING to the letter which appeared under the above heading in NATURE of January 30, I have given some little attention to this subject for the past few years, and thoroughly endorse your correspondent's views.

Any balancing apparatus must be automatic in its action if it is to respond to the changes in the relative motion of the air without delay. It would seem to me that any such apparatus must, as is suggested in the letter referred to, depend on the conservation of angular momentum in a pendulum or fly-wheel. Such a pendulum (or system of pendula) or fly-wheel may operate directly or indirectly, *i.e.* the torque of resistance opposing change of angular momentum may be employed to right the aëroplane, or may operate mechanism to control the position of guide planes or jockey weights, or rotate the main planes in a suitable manner. The first case is analogous to the Brennan mono-rail system, the second to the Obry torpedo balance. HERBERT CHATLEY.

32 Britannia Road, S., Southsea, February 1.

The Stresses in Masonry Dams.

I DO not think that Prof. Pearson proves his point. Is it not an axiom of practical mathematics that nearly identical functions (within certain limits) may have widely different second differentials? Between o and π, for