tiania; *Comptes rendus,* vol. clvi., pp. 450, 536, 1913). In particular he finds certain remarkable periodic trajectories in the form of circles the plane of which is perpendicular to the axis of the magnet and the centre of which is at some point on that axis. If this point coincide with the centre of the magnet we obtain circular orbits in the equatorial plane of the magnet. Further, there are other trajectories which never get outside closed toroidal spaces in the case of stability, or which approach asymptotically the circle in question in the case of instability. It appears probable that similar results would be obtained in the case of a ring of electrons, and that the outstanding problem of the stability of such a rotating ring when only electrostatic forces are considered might in this way be overcome. Experimentally such stable rings have been obtained by Birkeland by employing a magnetised sphere inside a vacuum tube.

Some of the orbits calculated by Størmer are also suggestive in connection with the wide angle scattering of α particles investigated by Rutherford and by Geiger and Marsden. If the nucleus produce a magnetic field, Rutherford's estimate of its radius may require modification.

H. S. ALLEN.

Wheatstone Laboratory, King's College, London.

I HAVE read the letters of Dr. Bohr and Mr. Moseley with great interest, and would like to make a few remarks in reply which may serve to render the meaning of my first letter more clear. Dr. Bohr says that we have no right to consider nNe^2, m, r, and h as independent variables and that we must eliminate r, in which case we find his formula. I am not convinced that this is necessary *a priori,* as Dr. Bohr would seem to consider it In some cases it leads to conclusions which are obviously erroneous. Supposing, for instance, that we calculate the period of a pendulum by this method. If we eliminate h we find $t=\text{const.}\sqrt{\frac{l}{g}}$, but if we eliminate l we find $t=\text{const.}\sqrt[3]{\frac{h}{mg^2}}$. We have just as much or just as little reason, *a priori,* to eliminate h or r, or any of the quantities involved in one case as in the other. In the case of the pendulum, h can only appear as a corrective term, perhaps of a form similar to $\sqrt{1-\frac{h\nu}{E}}$, where E is the energy. Possibly the same is true in atomic models.

I suggest that Mr. Moseley's frequencies, which can be represented by various equations, do not prove that one must necessarily adopt the formula obtained by eliminating r. But even if it be admitted that r must be eliminated *a priori,* the fact that we then always find a formula which, as Dr. Bohr admits, only differs from his in the constant, seems to me to justify my view that the fact that the frequencies agree with the formula does not necessarily confirm Dr. Bohr's special assumptions. The support to be derived from an agreement in the matter of the constant, however, is not very strong, as, according to Dr. Bohr's theory, it contains a factor of the form $(1/\tau_1^2-1/\tau_2^2)$ which obviously gives us the choice of an infinite number of values between O and $2\pi^2(N-\sigma_n)^2$.

Mr. Moseley also adduces arguments only in favour of what he calls the h hypothesis, not of Dr. Bohr's special assumptions. The reasons, however, do not appear to me absolutely convincing. Thus he says $\nu\sim(Fr^2)^2$, where F is the resultant electrostatic force on one electron, and concludes that as $M^{\frac{2}{3}}.L^2T^{-1}$ is constant, ML^2T^{-1} is constant. He thus introduces various hypotheses, such as that the same number of electrons oscillate in every atom, that there exist no other forces than electrostatic, and so on. If one liked, the fact that $\nu\sim N^2$ might just as well be interpreted as $\nu\sim Fr^2$, assuming N electrons to be attracted, whence we could deduce $ML^2.L/T=\text{const.}$, *i.e.* a universal velocity times a universal moment of inertia. Mr. Moseley says no independent natural unit of length is known. It is very easy to imagine atomic models in which one occurs, as, for instance, that proposed by Sir J. J. Thomson at the last meeting of the British Association.

There are one or two other points which do not seem to confirm Mr. Moseley's interpretation of the phenomena which he has observed. Mr. Moseley himself found, I believe, several lines in the characteristic platinum radiation, which are not where they should be according to his hypothesis, *i.e.* about in the region of wave-lengths two octaves shorter than copper. M. de Broglie has shown by means of the ingenious method for photographing X-ray spectra described by him in the *Comptes rendus de l'Académie des Sciences,* November 17, 1913, and completed December 22, 1913, and January 19, 1914, that platinum antikathodes emit at least ten independent lines. Although the whole spectrum was photographed, including the shortest wave-lengths, and although a continuous spectrum was observed in the region in which the lines were to be expected, the lines themselves were not present. Unless we ascribe all the strong lines observed to impurities and introduce a special hypothesis to account for the fact that the expected platinum lines are not observable, this seems to constitute a grave difficulty for the theory of Mr. Moseley. I have misgivings further as to the ring of four electrons being able to emit such strong lines as those observed, as the radius of the ring is about one hundred times smaller than the wave-length, but no doubt Mr. Moseley has considered this obvious objection, and satisfied himself that it is unfounded.

To recapitulate. It seems to me that Dr. Bohr postulates the h hypothesis, and that Mr. Moseley derives it by introducing a hypothetical model. That the h hypothesis does not entail Dr. Bohr's model. That Dr. Bohr's constant as applied by Mr. Moseley contains a factor which varies from 0 to 1, and that $\frac{3}{4}$ the value chosen is entirely arbitrary. Therefore my view is that all that can be said of Mr. Moseley's observations is, that they do not contradict Dr. Bohr's assumptions, not that they confirm them.

F. A. LINDEMANN.

Paris, January 25.

Systems of Rays on the Moon's Surface.

IT is a strange fact that those who have little experience of volcanoes notice a rough resemblance between the irregularities of the lunar surface and terrestrial volcanic vents. However much one juggles with diminished gravity and magnifies volcanic energy in the past history of our satellite, there are still several facts which are overlooked by many theorists. Mr. C. H. Plant points out in NATURE of January 15 (p. 550) that the "volcanic action of the moon was of enormous character"—this would need be so to produce on such a small globe craters of 80 kilometres or more in diameter.

Now all large craters are the result of explosive action, and, in explosive action, only fragmentary ejecta are thrown out by the amount of volatile constituents of the magma, which, if sufficient to excavate a crater, are also sufficient to break up all the igneous magma into scoriaceous or pumiceous materials, and not allow it to issue continuously as a lava stream. When lava rises, subsequent to an ex-