between r, Nne^2, m, and h; and if we introduce this relation in Dr. Lindemann's expressions for ν, all the different expressions become identical.

By a consideration of dimensions only, we cannot calculate the numerical factors which determine the exact values for the frequencies of the spectrum of an element; in order to do this, we must introduce more detailed assumptions as to the constitution of the atom and the mechanism of emission of radiation. A discussion of the special assumptions used in my calculations will be found in a paper on the influence of electric and magnetic fields on spectral lines, which will appear shortly in the *Philosophical Magazine*.

N. BOHR.

The University, Copenhagen, January 5.

DR. F. A. LINDEMANN (NATURE, January 1) disagrees with the theoretical interpretation of my recent work on X-ray spectra (*Phil. Mag.*, December, 1913). He objects to my statement that the results so far obtained strongly support the views of Bohr, and considers that they yield no information about the structure of the atom beyond confirming the views of Rutherford and van den Broek. My work was undertaken for the express purpose of testing Broek's hypothesis, which Bohr has incorporated as a fundamental part of his theory of atomic structure, and the result of the test certainly confirms the hypothesis. In my opinion, however, further definite conclusions can be drawn from the results, and these conclusions strongly support other features of Bohr's theory. Moreover, I cannot accept the alternatives which Dr. Lindemann offers to my formula representing the values of the principal frequencies observed.

Dr. Lindemann's arguments are based on the principle of dimensions. This method of treatment is of historical interest, as we owe to it the introduction of Planck's quantum h into the discussion of atomic structure. So long as the only factors, common to all atoms, on which this structure was known to depend, were e, m, the charge and mass of an electron and Ne the charge of the nucleus, it was impossible to obtain a quantity of the dimensions of a frequency. In an electromagnetic system the introduction of c, the velocity of light, might get over this difficulty, but it has proved more profitable to treat the problem as electrostatic and make definite calculation possible by using h.

We will call the assumption that h is a fundamental factor in the atom the h hypothesis. It then follows from the principle of dimensions that the frequency of an atom, $\nu = f\frac{e^4 m}{h^3}$, where f is a numerical constant which depends on N, and also on the arrangement of the electrons in the atom.

The reason why Dr. Lindemann arrives by the same argument at an indefinite result is that he takes r, the distance of the electron from the nucleus, or else rN to be an independent factor in the calculation. No independent natural unit of length, which would apply to an electrostatic problem, is known, and the separate introduction of r or rN appears to me to be unwarranted. Bohr has pointed out that the fundamental frequency ν_0 of ordinary series spectra is obtained by putting $f = 2\pi^2$ in the formula given above, while my work shows that the frequency of the principal line in the X-ray spectrum of elements from Ca, N=20 to Zn, N=30 corresponds with

$$f = 2\pi^2 . \tfrac{3}{4}(N-1)^2.$$

The simplicity of the expression f in these two cases is itself an argument in favour of the h hypothesis. It is, however, more strongly supported by the fact that the frequencies in the X-ray spectrum are proportional to $(N-1)^2$. Two alternative explanations can be given for the occurrence of $(N-1)$ and not N. It is just possible that two of the elements which precede calcium have the same atomic number. A mistake would then have been made each time in reckoning N, and ν would really be $\propto N^2$. It is much more likely that the repulsion of the other electrons cannot be neglected compared with the attraction of the nucleus, and then N must be replaced by $(N-\sigma_n)$. In either case we conclude that as we pass from atom to atom $\nu \propto (Fr^2)^2$, where F is the resultant electrostatic force on the vibrating electron. In other words, a quantity of dimensions $T(ML^3T^{-2})^2$ remains constant, and since the mass is always the mass of an electron $M^pL^2T^{-1}$ remains constant. By putting $p=1$ a quantity is obtained of the same dimensions as h. For these reasons I conclude that the experiments support the h hypothesis, which has been put forward in three distinct forms, first by Nicholson, then by Bohr, and recently by J. J. Thomson.

I have not succeeded in obtaining agreement between my results and the vibrations considered by Nicholson. Bohr's theory, on the other hand, explains why there is a general spectroscopic constant, ν_0, given by $f=2\pi^2$, and at the same time demands that the principal X-ray frequency should be given by $f=2\pi^2.\frac{3}{4}(N-\ \ )^2$. This agrees with the experimental result if the vibrating system is a ring of four electrons, all vibrating together; since $\sigma_4 = 0{\cdot}96$. Two things, however, suggest that either Bohr's theory or my interpretation of it requires modification. In the first place, it fails to account for the second weaker line found in each spectrum. In the second place it is difficult to see how a ring of four electrons can store up enough energy to vibrate as a whole. Perhaps the examination of the spectra of other groups of elements will suggest a solution of these difficulties.

H. MOSELEY.

Oxford, January 5.

"Atmospherics" in Wireless Telegraphy.

WITH reference to Prof. Perry's letter on "atmospherics" in NATURE of January 8, a description of some experiments made by us in the summer of 1912, and continued last summer, may be of interest. A receiving station was erected near Rothbury, in Northumberland, with an antenna consisting of two horizontal wires stretched about 3 ft. from the ground. The receiving apparatus consisted of a galena-tellurium detector and telephone circuit coupled to two inductances connected to the antenna wires and having a variable condenser in circuit between them. The length of the antenna was varied during the experiments, but for most of the time was about 500 yards each way, the direction of the wires being approximately north-west and south-east. No earth connection was used.

The antenna was laid on a slight slope, the receiving hut being situated in a field, but in each direction the antenna wires passed through extensive woods, the whole district in the vicinity being thickly wooded. During the observations of 1912 the ground was nearly always very wet owing to the excessive rainfall.

According to the views put forward by Prof. Perry, it would naturally be expected that atmospherics would be either absent or greatly diminished in intensity with an antenna such as we used. So far from this being the case they were both numerous and loud, so much so that we adopted this form of antenna as being suitable for investigating the direction from which atmospherics emanate. For this purpose we used crossed horizontal wires connected to a form of radio-goniometer, the well-known directive effect of