trons, or to both, and these axes are associated different amounts of energy, it is possible for the mass of the atom to be different for the different positions of the nuclear axis, since by the Theory of Relativity energy possesses mass. That is, the model proposed predicts the existence of isotopy. If the direction of the nuclear axis as described above is determined by the structure of the outer shell of electrons, we should expect the existence of different axes in the same atom to be favoured in those atoms the outer shells of which are complete (inert gases), nearly complete (halogens), or just forming (alkali metals). An inspection of Aston's list of isotopic elements shows that it is in the neighbourhood of the inert gases that the phenomenon of isotopy chiefly occurs.

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The Intensity of X-ray Reflection from Powdered Crystals.

IN the May number of the *Philosophical Magazine* which has just reached us, Mr. C. G. Darwin has presented a most valuable discussion of the reflection of X-rays from imperfect crystals. He shows that, on account of the difficulty in determining the effective extinction coefficient of the X-rays in such crystals, it is very difficult to calculate with accuracy the intensity of the reflected beam. Hence he is unable to make a satisfactory comparison between the theoretical formulæ and the existing experiments on the intensity of X-ray reflection. This result is in general agreement with the conclusion reached by one of us (*Physical Review*, July 1917) on the basis of somewhat similar considerations.

Mr. Darwin concludes that a more satisfactory test might be made on powdered crystals, since in this case the only factor contributing appreciably to the extinction is the ordinary absorption of the X-rays in the powder, which can be measured directly. We had arrived at the same conclusion, and have made quantitative measurements of the intensity of the X-rays scattered by powdered crystals.

In our most recent experiments, the $K\alpha$ line from molybdenum ($\lambda = 0.708 \text{ \AA.}$), after reflection from a crystal of rock-salt, was allowed to fall upon a plate of powdered sodium chloride. The first order reflection from the [100] faces of the powdered crystals then entered the ionisation chamber. The method was thus similar to that employed by W. H. Bragg (*Proc. Phys. Soc., Lond.*, 33, 222, 1921) except that the primary rays were homogeneous. The ratio of the energy reflected into the ionisation chamber due to this first order line to that incident upon the plate was 2.94×10^{-4} , with a probable error of about 10 per cent. The theoretical intensity of the line was calculated from a formula identical in significance with Darwin's formula (10.4) (*loc. cit.*), except that correction was made for the absorption of the X-rays in the crystal mass. We thus obtained the value 2.7×10^{-4} , which is in satisfactory agreement with the experimental measurement. Thus, at least to a close degree of approximation, the theory of X-ray reflection based upon the classical electrical theory gives accurate results.

This comparison of theory with experiment may be viewed in another manner. Any formula for the intensity of X-ray reflection must depend upon the value of a function ψ , the magnitude of which is determined by the distribution of the electrons in the atoms. The theoretical value 2.7×10^{-4} mentioned above is based upon the value $\psi^2 = 0.59$, i.e. upon the assumption that the intensity is 59 per cent. as great as it would be if all the electrons in sodium and

chlorine were grouped together at the centres of their respective atoms. This value was estimated by one of us (*loc. cit.*) on the basis of some of W. H. Bragg's measurements of the relative intensity of the different orders of X-ray reflection from rock-salt. The corresponding value of ψ^2 as determined by the measurements of W. L. Bragg, James and Bosanquet is 0.43 (*Phil. Mag.*, July 1921). To obtain our experimental value 2.9×10^{-4} for the intensity of reflection from powdered crystals, the value of ψ^2 must, however, be 0.64. The difference between the latter two values of ψ^2 supports Darwin's suggestion that the method employed by Bragg, James and Bosanquet for studying the intensity of X-ray reflection is not wholly trustworthy.

We hope in the near future to be able to report experimental results of a considerably higher degree of accuracy than those described above.

ARTHUR H. COMPTON.

NEWELL L. FREEMAN.

Washington University, Saint Louis, May 30.

Discoveries in Tropical Medicine.

I AM much astonished to learn, for the first time, from Dr. L. W. Sambon's letter in *NATURE* of May 27, that during the whole period of my work in India (from April 1895 to February 1899) he "was almost daily at Manson's house" and was allowed to read my private letters to Manson and to "discuss every detail." Are we to understand by this that his almost daily visits to Manson's house continued for all this period, and that during it he read all my letters to Manson, numbering 110, and averaging a thousand words each in length; or merely that he read a few of the letters which Manson showed to him from time to time? Dr. Sambon would appear to claim the former interpretation of his words, because he proceeds to suggest that he is intimately acquainted, in consequence of his knowledge of these letters of mine, with all details concerning the relations between my work and the theories of Manson. If so, I can only say that I am amazed and hurt. Many of my letters to Manson were of a very private nature, and it is difficult for me to believe that he would have handed over the whole of this correspondence without reserve to a gentleman who was at the time a stranger to me and was in no way concerned with my affairs.

Moreover, when Manson sent some of my letters to Lord Lister he was, of course, careful to inform me of the fact; but he never mentioned the name of Dr. Sambon, so far as I remember, in all the fifty-six letters which he wrote me in reply to mine, as surely he would have done had he decided to submit my letters to a third person without my previous consent. Nor did Sir Patrick Manson ever mention this matter to me during the many years which have elapsed since the correspondence referred to ceased. On the other hand, if Dr. Sambon did not see all my letters to Manson, including the private letters, he cannot possibly have that close knowledge of my work which he seems to believe he possesses.

The remainder of Dr. Sambon's communication in *NATURE* referred to makes me still more doubtful regarding the interpretation which is to be placed upon his words; for it seems to me that he does not understand the said relations between my work and the theories of Sir Patrick Manson. May I also take the opportunity to state that I for one can scarcely accept as sound any of the conclusions which he has set forth in your columns in the letter referred to.

RONALD ROSS.