tinct edges; on the margin of the upper angle of the Piltdown fragment the edge or margin of this groove can be clearly recognised.

In Dr. Smith Woodward's reconstruction, therefore, it is not only necessary to move the fragments of the right side outwards; the left parietal bone has also to be moved outwards, or rather tilted upwards and outwards until it assumes a more vertical position, with the marking of the sinus in the middle line. When that is done, and the other parts correctly adjusted, the brain cast assumes the form and size represented in Fig. 2. I made many experiments to test other possible suppositions, but only when the fragments were placed as in Fig. 2 could I secure symmetry, and at the same time obtain all the anatomical markings in their normal situations. The brain cast obtained from this reconstruction displaces just over 1500 cubic centimetres of water. Dr. Smith Woodward estimated his brain cast provisionally at 1070 c.c.; the replicas of the brain cast which were distributed displace 1200 c.c. of water; even if the reconstruction carried out by Dr. Smith Woodward is accepted, and the right half is made approximately symmetrical with the left, the brain of Piltdown man will be about 200 c.c. above his original estimate.

In my reconstruction two other peculiar features of the original brain cast have disappeared. One is the sharp bending inwards or kinking of the temporal lobe of the brain; the other is the position of the foramen magnum—the opening in the base of the skull for the exit of the spinal cord. In the original reconstruction the lower margin of the occipital bone was brought forwards so far in the base of the skull that when a palate was articulated there was no room left for the soft palate and pharynx. The corresponding basal parts of the brain cast are, of course, also abbreviated.

I do not attach any high importance to actual brain mass; it is merely a rough indication of mental power when applied to human brains. So far as concerns the description of the actual markings of the Piltdown brain cast given by my friend Prof. Elliot Smith, I am in complete agreement, but so far as concerns general mass and conformation, it is clear, from his letter in NATURE, October 2, p. 131, that I am at complete variance. How far I am right —to what extent I have made an error—remains to be seen; but the publication of these drawings and observations will show that I have made every endeavour to arrive as near the truth as is possible for me. A. KEITH.

Royal College of Surgeons, Lincoln's Inn Fields, W.C., October 4.

The Theory of Radiation.

IN his letter published in NATURE of October 9, Prof. Maclaren has referred to my use of the concept of a natural unit of angular momentum, and perhaps a few explanatory remarks may be useful, as the work has not been published in a journal devoted to physics. The concept first appeared in my paper on the constitution of the solar corona, published in the Monthly Notices of the Royal Astronomical Society in June of last year. It was found that the energy frequency ratios of the atomic systems, which were held to be the origin of the main lines in the coronal spectrum, were always simple multiples of the quantity $h/2\pi$, where h is Planck's constant. As these ratios were nothing more or less than the angular momenta of the atoms, the conclusion was forced upon me that Planck's h could only be an angular momentum.

As was stated at the time, such an interpretation removes much of the difficulty otherwise pertaining to the quanta theory, when expressed in the usual way in terms of energy. It does not, of course, explain that theory, but merely renders it more intelligible as a possibility, for it is not difficult to obtain fair mechanical models of atoms the angular momentum of which can only have a discrete set of values. Prof. Maclaren, in his letter has, in fact, indicated a very beautiful one by the help of the magneton, which has a definite unit of angular momentum. It is evidently possible to construct a system containing multiples of that unit.

The more recent work of Dr. Bohr (*Phil. Mag.*, July and September of this year) applies the same concept to series spectra, but is different in that it postulates the angular momentum of an electron in the normal state of the atom as *exactly* $h/2\pi$. For example, the whole angular momentum of a neutral atom with five electrons is, on Bohr's theory, $5h/2\pi$. But I had found it necessary in the paper cited above that the value should be $25h/2\pi$. There is in this respect a discrepancy between the two theories, which is probably not serious, as Dr. Bohr has only calculated the series lines in hydrogen and in helium with a single electron, and therefore charged. (The number of electrons and its square are then identical.) The real test of his theory will lie in its capacity to account for the *usual* spectrum of helium—a test which does not appear difficult. For Dr. Bohr has concluded that helium will not take a negative charge. The ordinary spectrum must therefore come from the uncharged atom in its passage between stationary states, which are of a limited number, as there are only two electrons. It does not appear that the helium spectrum can be obtained in this way, but perhaps further investigation will modify this conclusion. Until this is done, the point raised by Prof. Fowler in a recent discussion in NATURE, as to the apparent need for keeping the Balmer and Pickering spectra of "hydrogen" as two *distinct* series, has not been answered.

But as Prof. Maclaren states, whatever be the fate of this theory, the natural unit of angular momentum seems necessary. It is inevitably suggested by any atomic theory which now attempts to rest on a foundation of electrons and a positive nucleus; for its use is not restricted to the applications already mentioned. It is apparently the only ready means of explaining a type of spectral series which the writer has found recently to be of importance—a series in which the cube roots of the wave-lengths have constant differences. J. W. NICHOLSON.

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Science and the Lay Press.

MANY "lay" journalists will have welcomed the comments in NATURE of October 9 (p. 172) on the "sensational paragraphs to the effect that Sir Frederick Treves had announced at the Radium Institute 'a complete revolution in the future of radium.'" For the undue enthusiasm shown, the Radium Institute is partly to blame. Sir Frederick claimed credit on behalf of it for the discovery that emanation was as valuable as radium in the treatment of cancer, and when Mr. Pinch was describing the good results obtained with emanation water in the treatment of arthritis deformans he interpolated the remark that this was something new in medicine. Undoubtedly, too, the impression was created in the minds of many of those present that by utilising emanation a gram of radium could be made to do the work of several grams. While in the matter of comment several newspapers fell into gross errors, they did little more than translate into popular language the sense of what was said.

Unfortunately in compressing what he had to say