LETTERS TO THE EDITOR.

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The Origin of Radium.

I CANNOT let Prof. Rutherford's letter in NATURE of June 6 pass without directing attention to one striking consequence, in which I personally am interested. During 1904 and 1905 I published (NATURE, May 12, 1904, January 26, 1905, and *Phil. Mag.*, June, 1905, p. 768) the result of an experiment which went to show that a kilogram of uranyl nitrate, purified initially from radium by precipitating barium as sulphate in its solution, and kept 550 days, generated a quantity of radium which, although only one-thousandth part of what is theoretically to be expected on the view that a direct change of uranium X into radium takes place, was still one hundred times the amount initially present. Boltwood (*Am. Journ. Sci.*, September, 1905, xx., 239), working with one hundred grams of uranyl nitrate purified from radium initially by repeated crystallisations from water, was unable to observe any detectable increase after a period of 390 days, and concluded that "the results obtained by Mr. Soddy are without significance," and averred that my results were due to the introduction of radium salts during the tests.

Now such a criticism and such an imputation on the part of one investigator dealing with the work of another surely ought only to have been made if it was the only possible explanation of the discrepancy. As it was, to me at least, it was not even the most obvious explanation. Boltwood did not give consideration to the all-important influence of the method of purification of the uranium from radium on the results obtained. My result, that the raté of production of radium from uranium was only one-thousandth of the theoretical, brought into being the present theory of the existence of several hypothetical intermediate transition forms between uranium and radium. It is obvious that, according as the method of purification employed does not or does remove these transition forms as well as the radium, so one will or will not expect to observe an initial production of radium in a solution of uranium. Now the method of precipitating barium as sulphate in a uranium solution is designed to *remove only* the radium, whereas the method of repeated crystallisation from water adopted by Boltwood is well calculated to *purify* the uranium, that is, to free it from all other accompanying substances. Hence there is no necessary discrepancy between the results of the two experiments. This view has been put forward by Rutherford ("Radio-active Transformations," p. 159).

I would not now have raised this matter had not history apparently repeated itself, and Prof. Rutherford's most recent results (NATURE, June 6, p. 126) enabled me, without making any special claim to infallibility, to exhibit clearly the real nature of Boltwood's criticism. In the *American Journal of Science* for December, 1906, p. 537 (NATURE, January 3), Boltwood published a "Note on the production of Radium from Actinium" in which evidence was given that actinium is the parent of radium. This was quickly followed (NATURE, January 17, p. 270) by some confirmatory evidence of a similar character by Rutherford, who, however, pointed out that there was no proof that actinium was itself the true parent of radium, although this parent was undoubtedly present in the actinium solutions employed. Now Rutherford shows in last week's issue that actinium *purified from radium in a different manner* yields no appreciable growth of radium. Is Boltwood's previous positive result then "without significance"? Surely not. But if Boltwood's result on the production of radium from actinium can be explained, as, of course, it can be explained, without charging him with introducing radium into his solution, so in the same way can mine with uranium. Indeed, whereas the intermediate product, which is the parent of radium, is a *necessary* companion of *any* uranium preparation which has not recently been subjected to a purification process capable of removing it, it has yet to be shown that the association of this parent with actinium is genetic and not purely fortuitous.

I hope this exposure of an old criticism, made without due consideration of the complexity of the problem, will clear the way for the publication of some further results. In the two years that have elapsed since the publication of my last paper I have had the advantage of the co-operation of Mr. T. D. Mackenzie in the steady and continuous prosecution of the work under the most favourable conditions. We have from the commencement, which dates prior to Boltwood's first communication on the subject, had as the basis of the work the all-important influence of the method of purification adopted, and we have used throughout a new method of purification, which, though not without difficulty and danger in its application to the purification of large quantities of uranium salts, was deliberately chosen as affording a reasonable guarantee that it would separate the uranium from all other substances present. Mr. Mackenzie has purified with the utmost care three separate kilograms of uranyl nitrate by this method, and I may anticipate our results to the extent of saying that, so far, they entirely confirm and extend the results obtained by Boltwood in which re-crystallisation was the method of purification employed. The first preparation, containing after purification about 500 grams of uranyl nitrate, has been kept for 600 days, and has not shown the slightest detectable increase in the amount of radium initially present. Now that these three purified preparations have been set up in a form to allow of continuous and extended observation, our attention is being directed to the residues from the three kilograms, which should contain the parent of radium, if my earlier positive result was correct. After all, it would be a little surprising if this parent of radium was entirely absent from commercial salts of uranium, for although Boltwood and Rutherford have found it in preparations of actinium, it must not be forgotten that the only source of actinium is that from which commercial uranium salts are prepared.

FREDERICK SODDY.

The University, Glasgow, June 8.

The Structure of the Æther.

IN the current number of the *Philosophical Magazine* I have given in some detail certain objections to identifying the magnetic vector with translational æthereal motion, and to a large extent these are on all fours with Prof. Hicks's objection, which is cited by Sir Oliver Lodge in the same number, and of which I had lost sight. Very briefly, thus: if bodily æther flow were (within a constant factor) identical with magnetic induction, or were even an essential feature thereof, our judgment as to whether or not a given region was pervaded by magnetic induction would depend on the arbitrary origin of coordinate axes relatively to which we chose to measure velocities, motion of bodies through the æther being physically indistinguishable from an equal and opposite motion of the æther with those bodies at rest.

Much the same difficulty (concerning the essential relativity of motion) seems to me to arise when resultant æthereal momentum is taken to correspond to the vector product of the electric and magnetic vectors; in this case, moreover, further difficulties are encountered. Consideration of a progressive train of electromagnetic waves shows that, with this æthereal-flow interpretation of the Poynting vector, we should have a resultant æthereal motion made up of a steady flow in the direction of wave propagation, together with to-and-fro motions parallel to that direction and kinematically exactly simulating the motion of a gas which is transmitting waves of sound. This clearly implies *compressibility of the aether*, not merely as a minute residual phenomenon, but as a fundamental relation of electromagnetism.

And what would happen in the case of such a body as the sun, which consistently radiates more energy than it receives by radiation? There would be a flow of æther outward in all directions, maintained throughout immense periods of time. This difficulty seems almost insuperable.

There appears to me to be much evidence in favour of the view that the resultant velocity of the æther (referred

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