

LETTERS TO THE EDITOR.

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The Origin of Radium.

IN a previous letter to NATURE (January 17) I gave an account of some experiments which I had made upon the growth of radium in preparations of actinium. The results obtained were in substantial agreement with the earlier observations of Boltwood in this Journal (November 15, 1906), but it was pointed out that there was no definite evidence that actinium itself was the true parent of radium. The experimental results could be equally well explained by supposing that the parent substance of radium was ordinarily separated from radio-active ores with the actinium, but had no direct radio-active connection with the latter.

Observations have been continued upon the growth of radium in the actinium solution prepared in the manner indicated in my first letter. The rate of growth was found to be uniform over a period of 120 days, and to agree closely with the rate of growth observed in the solid preparation of actinium which had been set aside for a period of two and a half years. Another sample of actinium was then taken and successively precipitated with ammonium sulphide in order to remove the radium from the solution. In this way a solution of actinium was obtained initially almost entirely free from radium. By examination of the α -ray activity, it was found that the actinium after this chemical treatment contained an excess of radio-actinium. This was shown by the rise of the activity to twice its initial value in about twenty days, and then a gradual decay to a steady value. Special care was taken to measure accurately the rate of growth of radium in the solution at short intervals in order to see whether it depended in any way upon the variation of the activity. No such connection was observed, for the radium was produced at a constant rate over the whole period of examination, viz. 111 days.

For equal quantities of actinium, the rate of growth of radium observed in this solution was 1.5 times greater than the normal. This indicated that only a portion of the actinium had been precipitated, while the radium-producing substance had been precipitated with the actinium in excess of the normal amount. This conclusion was confirmed by an examination of the filtrates, which were found to contain more than half the actinium. After suitable chemical treatment, a small precipitate of actinium was again obtained which was about one hundred times as active, weight for weight, as the original preparation. This actinium precipitate was dissolved in hydrochloric acid, and observations of the amount of radium in it were made at regular intervals. No appreciable growth of radium was observed over a period of eighty days. If there were any growth at all, it was certainly less than one two-hundredth part of that normally to be expected. In order to make certain that the absence of apparent growth of radium in this solution could not be ascribed to the precipitation of the radium in some non-emanating form, the solution was again chemically treated. The actinium was precipitated with ammonia and re-dissolved in hydrochloric acid. Again no growth was observed over the period of examination, viz. twenty days. The solution in its present state contains a just measurable quantity of radium, viz. about 2×10^{-12} gram.

From these observations I think we may safely conclude that, in the ordinary commercial preparations of actinium, there exists a new substance which is slowly transformed into radium. This immediate parent of radium is chemically quite distinct from actinium and radium and their known products, and is capable of complete separation from them.

It is not possible at present to decide definitely whether this parent substance is a final product of the transformation of actinium or not. It is not improbable that it may prove to be the long-looked-for intermediate product of slow transformation between uranium X and radium, but with

no direct radio-active connection with actinium. If this be the case, the position of actinium in the radio-active series still remains unsettled.

It is intended to continue observations on the growth of radium in the solutions described above. Experiments are also in progress to isolate this new substance in order to examine its chemical and radio-active properties.

Manchester, May 30.

E. RUTHERFORD.

The Structure of the Ether.

I WELCOME the interesting and helpful letter from Dr. O. W. Richardson, of Princeton, in NATURE of May 23, in which he adduces arguments against an ether flow along magnetic lines of force, and in favour of a flow in the direction of the Poynting vector EH. The result comes out much the same, but it is probably a better way of regarding the matter. Prof. Hicks also has given a simple geometrical proof that a magnetic field cannot consist solely of ether flow; and I am referring to this in a note, already printed, in the *Phil. Mag.* for June.

We shall doubtless hear in due course from the mathematical physicists to whom the first idea of a magnetic ether flow is due, whether they are satisfied with the modification of their original conception now introduced. Meanwhile, I doubt if integration of momentum, without regard to direction, can be sound.

OLIVER LODGE.

Birmingham, May 28.

Root Action and Bacteria.

THE remarkable and all but fatal effect of growing grass over the roots of freshly planted apple trees has been studied at the Woburn Experimental Fruit Farm since 1894, and formed the subject-matter of the third report of that station (1903). No satisfactory explanation of the action was obtained. Experiment showed that it could not be attributed to the abstraction of food or moisture from the soil by the grass, nor to the influence of the grass on the soil temperature or on the gaseous contents of the soil, and subsequent experiments have excluded the formation of acid or alkali from the possible causes. The conclusion drawn was that the action was probably that of a poison produced either directly by the grass or indirectly through the agency of bacteria. Since the publication of this report, further work has been done on the subject, and the view that bacterial agency is concerned has become much strengthened. The action is not confined to any particular grasses, nor to apple trees, but different grasses and different kinds of trees act and suffer, respectively, to different extents. The difference in the results, however, produced by different soils are much more conspicuous, especially in cases where trees are not grassed over until a few years after they have been planted. Though the deleterious action of grass may generally be noticed throughout the country, many notable exceptions have been met with, and these cannot be explained by any of the patent characteristics of the soils in question. Various pot experiments have been made which emphasise these observations. Trees grown in earth in pots are affected by grass in just the same way as they generally are in the field, the grass reducing the growth and vigour of the tree by at least 50 per cent.; but if the trees are grown in sand instead of earth (suitable nourishment being supplied), the grass has very little effect on them, reducing their vigour by about 5 per cent. to 10 per cent. only.

Following up this and other observations, twenty-six similar trees were planted in pots last February under various conditions; seventeen of them were in soil or sand which had not been heated, and nine of them in soil which had been sterilised, or partially sterilised, by heating to about 200° C. and to 82° C. respectively, the water lost in the process being made good. Of the seventeen in unsterilised material, all started into growth uniformly at the same time, whereas of the nine in sterilised soil two started about two days later, six did not start until at least fourteen days later, and one has not started yet.

The heating of the earth, especially to the low temperature of 82°, cannot have appreciably affected its chemical composition, and, indeed, the starting of a tree into growth is independent of nourishment supplied to it, as is shown