LETTERS TO THE EDITOR.

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The Origin of Radium.

IN replying to Mr. Soddy's communication published in NATURE of June 13 (p. 150) I cannot refrain from expressing my regret on learning that he has apparently taken my paper in the *American Journal of Science* for December, 1906, as such a serious "criticism" and "imputation" on my part in dealing with his paper on "The Production of Radium from Uranium." It was my intention merely to point out certain conditions of experiment which appeared to me to be quite essential to the solving of the problem of the growth of radium in uranium compounds, conditions which had apparently been neglected in his own experiments, and to show that where these conditions had been fulfilled the results were not in agreement with those which had been obtained by him. I was then, and am still, of the opinion that the experimental procedure which Mr. Soddy adopted was not suited to give conclusive results of either a positive or negative character, and this opinion is certainly strengthened by the fact that, under very similar conditions of experiment, Prof. Rutherford was unable (Bakerian Lecture, Phil. Trans., 204, 218) to observe the growth of radium in a solution of Giesel's "emanating substance," although he has since concluded that radium was actually being produced in quite notable quantities.

My suggestion of the "accidental and unconscious introduction" of radium salts during Mr. Soddy's tests was only one of a number of possible sources of error to which I have already directed attention. That the other suggestions have not met with his disapproval, and that he is at least convinced of the necessity of starting with purified uranium salts, would seem probable from his statement that he is now continuing the investigation with purified uranium compounds. I am very glad to learn that the results which he has now obtained entirely confirm and extend the results which I have already published. It may be of interest to add that my original solution of one hundred grams of *purified* uranium nitrate has recently been tested and found, after a period of more than two and one-half years, to contain less than 10^{-11} gram of radium.

"The experiments described in this paper are considered to indicate that the results obtained by Mr. Soddy are without significance and that one or more products of a slow rate of change intervene between uranium and radium." This is the particular paragraph to which Mr. Soddy now raises an objection. I fully realise that this statement is open to criticism; it was an unsuccessful effort at brevity. A longer but more satisfactory summary would perhaps have been:—The results of the experiments described in this paper are in support of the hypothesis that one or more products having a slow rate of change intervene between uranium and radium, and indicate that the results obtained by Mr. Soddy are without quantitative significance in so far as they relate to the production of radium by uranium.

"Commercial salts" of uranium may contain, and usually do contain, quite appreciable amounts of every constituent of the minerals from which they have been prepared. The presence in such salts of a small proportion of the immediate parent substance from which radium is derived is therefore in itself no indication of any genetic connection whatever between uranium and radium. My observation of the growth of radium in actinium preparations, even if it has served no other useful purpose, has certainly indicated where the immediate parent of radium is to be sought. To judge from the results which I have obtained in recent experiments along the same lines it would appear that, unless the rate of disintegration of radium now assumed is greatly in error, the chemical process outlined in my "Note on the Production of Radium from Actinium" is capable of effecting the essentially quantitative separation of the radium parent from most of the other substances present in a uranium mineral.

In conclusion, it may be desirable to direct attention to the fact that the only evidence we now have that radium is a disintegration product of uranium is the constancy of the ratio between the quantities of these two elements in the natural minerals, a relation which was first pointed out in these columns by the writer. BERTRAM B. BOLTWOOD.

Yale University, New Haven, Conn., June 29.

The "Double Drift" Theory of Star Motions.

I HAVE been greatly interested in Mr. Eddington's account in NATURE of July 11 (p. 248) of Prof. J. C. Kapteyn's investigations of this subject. Although I do not quite follow his argument for the existence of two overlapping systems of stars (more dramatically termed "two Universes" by Prof. Turner), I yet venture to suggest an explanation of the apparently (perhaps really) opposite "drifts," which seems to me to agree sufficiently with the observed facts.

If we adopt Lord Kelvin's postulate of a single vast stellar universe very slowly condensing towards its common centre of gravity, we might expect that the component stars would move for the most part in ellipses or spirals of very varying degrees of eccentricity and of inclination to the mean orbit—perhaps indicated by the Milky Way. If we further postulate (what is very generally admitted) that our sun is situated towards the central rather than towards the outer portion of the whole system, then, just as the planets, through differential angular motions as regards the earth, appear sometimes to move in a retrograde direction or to be quite stationary, so a certain proportion of the stars might be expected, at any given period, to exhibit the same phenomena.

But further, considering the enormous distances that are known to separate the stars and star-groups from each other and the extreme slowness of their angular motions, there seems no reason why their respective orbits should not be almost as frequently in a right-hand as in a left-hand direction in regard to the central plane of general motion.

Our knowledge of the actual motions of the stars may not inaptly be compared to what astronomers would possess of the solar system supposing the whole of their observations had been limited to a period of about twenty-four hours, and that the sun was invisible. The motions of the planets and their satellites thus determined would seem as strange and incomprehensible as do those of the stars at the present time, our accurate observations of which have been limited to a few centuries.

It will probably be of interest to many of your readers (as it certainly will be to myself) if some of your mathematical correspondents will explain why, and in what way, some such system as is here suggested is incompatible with the facts set forth by Prof. Kapteyn and others.

ALFRED R. WALLACE.

IN the article to which Dr. A. R. Wallace refers, and elsewhere, I have confined myself to attempting to establish the result that the stars distribute themselves into two systems according to their motions, abstaining as far as possible from defining what physical connection is implied by the rather vague word "system." Whether the two systems are comparatively permanent and have come together from different parts of space, or whether they may have been evolved from a single system, is, in the present state of our knowledge, a somewhat speculative question, and it is with some reluctance that I enter upon it. Still, without asserting that the hypothesis of two *permanent* systems is the only possible one, I know at present of no other satisfactory explanation. In the system suggested by Dr. Wallace (in which the stars move about the centre of the universe in ellipses, some forward and some retrograde, with all sorts of eccentricities) the motions would be for our purposes haphazard. Thus the system would form a single and not a double drift; the extremely eccentric orbits form a perfect transition between the direct and retrograde orbits. To account for two drifts, it is not sufficient to show that some stars move forward and some backward; it must be shown that there is a concentration of the motions about two definite veloci-