LETTERS TO THE EDITOR.

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Variation of Atmospheric Absorption.

I SHOULD be pleased to know how far some observations on the change in the average absorption of the terrestrial atmosphere in this country during the last two years have been confirmed by observations elsewhere. The following table gives the mean of the best of these, made at Washington in the autumn of 1901, the spring and autumn of 1902, and in the winter, spring, and summer of 1903.

Coefficients of Transmission for Zenith Sun.

	μ	μ	μ	μ	μ	μ	μ	μ
Wave-length... ...	0·50	0·60	0·70	0·80	0·90	1·00	1·20	1·60
Mean of observations, 1901–1902.	0·765	0·769	0·857	0·897	0·910	0·921	0·933	0·930
Mean of observations during 1903	0·627	0·692	0·753	0·797	0·825	0·847	0·874	0·909
Excess of transmissibility of 1901–1902 over that of 1903	20%	10%	13%	12%	10%	8·4%	6·5%	2·3%

The decrease in the transmissibility of the air this year as compared with the last is so marked that some local effect on climate and vegetable growth might seem to be probable. Whether the unusual coolness of the summer, reported both in America and abroad, is connected with it may be a subject for speculation. S. P. LANGLEY.

Smithsonian Institution, Washington, October 22.

Heating Effect of the Radium Emanation.

THE very important and fundamental experiments described by Profs. E. Rutherford and H. T. Barnes in NATURE of October 29 will have been read with the greatest interest. Owing to the importance of the subject, I should like to direct the authors' attention to some points in their comment and explanation which do not appear to me to be quite clear, and if I can draw from them some more detailed discussion this letter will have served its purpose.

The general conclusion arrived at by the authors is that "more than two-thirds of the heating effect is not due to the radium at all, but to the radio-active emanation which it produces from itself." If I understand the description of their experiments correctly, these seem to me, however, to point to the fact that it is the "excited activity" and not the emanation that is the cause of the heating. Apparently de-emanated radium gives out an amount of heat at a rate which falls in a few hours to a minimum and then slowly recovers. Now the emanation itself begins to form again at once, so that on the authors' hypothesis the heating effect should start with a minimum and then gradually increase. The activity of the radium measured by electric methods follows the course of the heating effect, and, as Messrs. Rutherford and Soddy have explained (*Phil. Mag.*, April, *not* May as quoted by the authors), this is due to the fact that the de-emanated radium has still the excited activity attached to it, and this activity decays in the course of a few hours. When the excited activity is gone there is nothing but radium left, and the further changes are due to the re-formation of the emanation and its subsequent change into excited activity. During the course of the first few hours there is, therefore, very little emanation, but there is excited activity which falls to a minimum and then slowly grows again. Does not the explanation which holds for the activity also hold for the heating effect, and would it not follow that the parallelism of heating effect lies with the amount of the excited activity present, and should be assigned to it rather than to the emanation?

Similarly, the emanation, according to the authors, does not give its full heating power at first, but the heating effect rises to a maximum in the course of the first few hours. If the emanation is the cause of the heat, why this slow rise? Here again the effect seems proportional to the amount of excited activity present, and not to the amount of the emanation. The connection of heating power with the emission of α rays also requires further elucidation, and the information given by the authors is not, I believe, sufficient to prove their case. It is only with great diffidence that I address these remarks to you, because Prof. Rutherford knows the whole subject at first hand, and his judgment is more likely to be correct than mine. Nevertheless, one likes to know whether others have felt the same difficulty, and whether the apparent disagreement is one of misunderstanding or has some more deep-seated cause. ARTHUR SCHUSTER.

The Owens College, Manchester, November 2.

Radium and Plants.

THE sensibility of protoplasm towards the radiations of radium is a matter of so much importance that a few preliminary experiments I have carried out on plants may be of interest.

The first experiment I made in this direction was with cress seedlings. About 100 seeds were uniformly distributed over the surface of some moist sand contained in a flower saucer, and a tube containing 5 mgrs. of pure radium bromide supported at a height of 1 cm. over the centre of the sand surface. During the experiment the saucer, covered with a glass shade, was kept in the dark. It was hoped that this arrangement would show whether the radiations are harmful or not to the sensitive cells of seedlings, and at the same time indicate if they are able to act as a stimulus to evoke positive or negative curvatures.

After the germination of the seeds, which took place within two days nearly simultaneously all over the sand, the growth of all the seedlings was nearly uniform. But close comparison showed that the seedlings immediately under the radium tube were to some small extent retarded in their development. The retardation was apparent in the seedlings situated within a radius of about 2 cm. from the radium bromide. Besides being smaller, these seedlings developed somewhat fewer and shorter root-hairs than those nearer the margin of the sand.

In the subsequent growth the presence of the radium evoked no curvatures in the little plants close by it, or in those more removed. Nor did it appear to exercise any noxious effects, other than the retardation just described, on the seedlings within the period of the experiment, viz. thirteen days. The plants grew up beside it and against the glass containing it, neither influenced by it nor hurt by it, so far as one could see.

This experiment was repeated on two other occasions (one experiment lasting three days after germination and the other lasting four days) with the same result, viz. no curvature was evoked, but the seedlings close under the radium bromide were slightly retarded in their growth.

In order to determine if motile organisms are sensitive to the radiations I enclosed the radium tube in a vessel of water containing large quantities of *Volvox globator*. Extraneous light was cut off from the experiment. After twenty hours many of the Volvox colonies had sunk to the bottom of the vessel, but they were evenly distributed over the bottom, and were neither aggregated under the tube nor dispersed away from it. Those that were still swimming in the water were also uniformly distributed through it, some actually in contact with the radium tube and some far away from it, but showing no sign of being attracted towards it, or of being repelled from it.

It is apparent from these few experiments that the radiations emitted by radium bromide are not able to produce marked effects in a short time on these vegetable cells and tissues. Even the phosphorescent light (which is quite perceptible to the eye under suitable conditions) emitted by the radium bromide is too feeble to be effective in calling out a phototactic response. HENRY H. DIXON.

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