LETTERS TO THE EDITOR.

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Heating Effect of the Radium Emanation.

A FORTNIGHT ago I wrote to you respecting the rise of temperature observed in radium compounds. I pointed out that the experiments of Profs. Rutherford and Barnes seemed to show that the effect was largely due to the excited activity. I have since then made a few experiments confirming that view.

Air charged with radium emanation was led through a tube in which I placed a thermal junction formed by iron and nickel wires. The junction was left charged to a high negative potential during about ten minutes, then taken out and placed side by side with an unexposed junction in a metal vessel kept at constant temperature by a water jacket. The two junctions at first seemed to be at the same temperature, but the exposed one began to become warmer almost immediately, and after twenty minutes was about one-tenth of a degree higher than the other junction. The experiment was repeated with the same result. Test experiments were made in various ways to show that accidental temperature disturbances could not affect the experiment. Thus, after the wire had been treated exactly in the same way in a current charged with thorium emanation, the junction showed absolutely no change of temperature detectable by the galvanometer used.

We may draw an important conclusion from these experiments. Prof. Rutherford has given strong evidence to show that the excited activity really contains three successive stages of radio-active matter, the first changing into the second, and the second into the third; the fact that the maximum heating effect was only obtained thirty minutes after the wire was first exposed to the emanation seems to show that it is the last transformation in which the third excited activity finally disappears or becomes inactive which sets the energy free. Experiments are now in progress to test the matter further.

The experiment mentioned above with the thorium emanation was not altogether satisfactory, and I should not at present like to draw the conclusion that the excited activity due to thorium does not give a heating effect. I only mention it here to show that the treatment of the wire in the experiment independently of the presence of radium does not give rise to such variations of temperature as have been observed. Had any appreciable amount of the heating effect been due to the contact with the emanation, I should have expected the junction to show some rise in temperature when first introduced to the calorimeter. All these results should be considered as provisional only until a more detailed investigation has been made.

ARTHUR SCHUSTER.

The Owens College, Manchester, November 14.

Radium and Animals.

IN the issue of NATURE of November 5, Mr. Dixon gave a brief account of some interesting experiments with radium upon seedlings and upon Volvox, the results of which were almost entirely negative. Like Mr. Dixon, I have been investigating the action of radium rays upon living matter, but in my experiments animals of simple structure have been employed instead of plants, and my experience leads me to think that the negative result of his experiments may have been due to the distance which separated the small quantity of radium he employed from the seedlings.

In my experiments, which I have been carrying out in the university physiological laboratory, three lots of radium bromide were used, 5 mgr., 10 mgr., and 50 mgr. respectively. These were brought within 3 mm. of the cells containing the animals, the walls of which were made of thin mica instead of glass in order to lessen the absorption of the rays.

It is too soon to discuss that obscure problem, the nature of the influence of the rays upon living matter, but it is already clear that experiments with simple forms of life will furnish some data.

I have endeavoured to determine (α) whether the rays would provoke an immediate response of the nature of a contraction; (β) whether they would evoke the more generalised "tactic" response—that is to say, whether they would repel or attract the animals. Put very briefly the results are as follows:—

(α) Actinosphærium, with pseudopodia extended, exposed in daylight to 10 mgr. radium at 3 mm. did not retract its pseudopodia. In two hours, however, it was dead and breaking up. Controls were unchanged.

Stentor—a green species. Two specimens were kept in the dark for some hours to increase their sensitiveness to radiant energy. On examination with a minimum of light the animals were found extended with cilia in rapid movement. Exposed to the rays from 50 mgr. of radium at 4 mm. both slowly contracted, and slowly extended on removal of the radium. This observation was repeated three times. After the third exposure one Stentor refused to extend.

(β) Stentor. Sixteen free-swimming specimens were placed in the dark in a cell over a lead plate 3 mm. thick with a hole in the centre about 5 mm. in diameter under which was 50 mgr. of radium bromide. Next day fifteen of the animals had attached themselves clear of the pencil of β rays, and one injured specimen was in the path of the rays.

The cell was then moved so that a group of five came into the path of the β rays. In a few hours these were found to have detached themselves and moved out of the rays.

Similar results were obtained on other occasions, though it seems possible for the rays to kill feeble specimens before they respond to the repelling influence.

Hydra, both *viridis* and *fusca*, will, as a rule, detach themselves and move out of a pencil of β rays. If, however, the animal is again moved back into the rays from 50 mgr. at 4 mm. distance the third immersion is usually fatal—the tentacles drop off and the body slowly breaks up.

Perhaps the most interesting result was obtained with *Euglena viridis*. Encysted specimens under the influence of radium rays (β and γ) in the dark readily become motile and disperse without suffering any harm.

Newnham College, Cambridge. E. G. WILLCOCK.

Note on the Arctic Fox (Canis lagopus).

A RATHER peculiar error in regard to this animal seems in danger of being perpetuated in certain contemporary literature, in which it is stated that, while in the other regions of its distribution the Arctic fox generally acquires a white winter coat, in Iceland this change never takes place, but that all the foxes there are blue. As a matter of fact, this fox turns white in the Icelandic winter as elsewhere, with this reservation only, that the proportion of blue winter forms there is greater than the proportion in the Arctic regions generally, the white forms, however, probably still remaining in an actual majority. I believe this occurrence of the white phase in Iceland is so far well known that I need not dwell on the evidence for it; from personal experience, however, I can corroborate it. It is a small point, but in so far as error is abroad, it seems advisable to correct it.

In Iceland I was informed that the white form and the blue were distinct, and in his work on this island a century and a half ago, Horrebow was of the same opinion. This view is based on the fact that both Horrebow and the Icelanders had seen white foxes in full summer, and is no doubt to be explained by the fact that occasionally the white dress is not changed for the summer brown. On the other hand, I believe that some authors still maintain the distinctness of the two forms, though I am not aware how they overcome the evidence of those who have observed the phases intermediate between the two which occur at the moulting season.

In his "Colours of Animals" Prof. Poulton quotes the