

LETTERS TO THE EDITOR.

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The Victoria Nyanza Jelly Fish.

IN a note occurring in your last issue (p. 348), mention is made of an interesting fact with which zoologists have for some time been familiar, namely, that the medusa characteristic of Lake Tanganyika exists at present in the Victoria Nyanza also. As it seems to be suggested that this discovery is in some way or another adverse to the theory of the origin of a portion of the fauna of Tanganyika for which I have been responsible, you will perhaps allow me space to point out that, so far from the fresh knowledge being in any way antagonistic to the view in question, the existence of the jelly fish in other lakes beside Tanganyika is exactly what one would, and did, anticipate, supposing the halolimnic theory to be correct.

The medusa in the Victoria Lake is identical with that in Tanganyika, and its presence in the former can be explained in two ways.

It may have, so to speak, existed there from all time, in which case the rest of the halolimnic fauna, or at least a part of it, should be found along with the jelly fish. In this case we should have a confirmation and extension of the view which I have already put forward, that the ancient sea from which the halolimnic "relic" sprang spread much further towards the east than was at first supposed.

It is, however, quite possible that the medusa has been recently transported to the Victoria from Tanganyika, owing to the opening up of the new trade routes, and the carriage of water in gourds and other vessels from one basin to the other.

This second view, to me, seems extremely likely, and it is certainly supported by the fact that the Victoria jelly fish is identical with that in Lake Tanganyika. Had it been long isolated in the former lake it would almost certainly now have presented specific differences, just as the freshwater shells of the Victoria differ slightly from those of the neighbouring lake basins.

J. E. S. MOORE.

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The Blondlot n -Rays.

THERE can be no doubt that the results obtained by M. Blondlot and others at Nancy are most remarkable, even if they should prove to be, as Herr Lummer's communication to the Berlin Physical Society would lead us to suppose, purely subjective, or, as he prefers to put it, "objective phenomena in the retina."

I have endeavoured to repeat M. Blondlot's experiments, but quite without effect, using calcium sulphide screens of the dimensions he suggests, that is, about 16 mm. by 2 mm.

A thin layer of gum is spread over a sheet of cardboard and the powder sprinkled over the surface until as large an amount as possible adheres to the screen. No difference in the colour or intensity of the phosphorescent glow appears to take place when a lead screen or the hand is interposed between the phosphorescent screen and an Auer burner completely enclosed in a tin-iron box with an aluminium window, nor does the interposition of a quartz lens in various places have any effect.

A very much larger screen was exposed to the Auer burner, one half being screened with lead and the other with thin aluminium, so that only the latter half was exposed to the radiation of the n -rays. The luminosity of the screen was, however, quite uniform throughout, although a sharp line ought to have separated the two parts of the screen, as the intensity of the phosphorescence should have been different in the two sides.

I am at a loss to find any other explanation of M. Blondlot's results than that he has come across a radiation to which some men are blind and others not so.

Self-hypnotism due to fatigue of the optic nerve may account for results of one observer alone if he were to manipulate the lead screens and to make observations

at the same time, but I think that M. Blondlot will have taken the precaution to get others to work the screens for him whilst he observed, and then compared results.

I may perhaps venture to note that a few years ago, in the course of some photometric work with fluorescent bodies, I was led to try whether one fluorescent body would increase or diminish the brightness of another (*Phil. Trans.*, vol. cxc. p. 92), but could not detect any such change within the errors of observation.

The fact that M. Blondlot has actually measured the wavelength of the n -rays leaves little doubt (in my mind) that what he has observed is, in the true sense, an objective and not a subjective effect, but at the same time the fact also that so many others who have tried in apparently the same way have failed, and failed deplorably, leaves still less doubt that the precise conditions upon which the effect depends yet remain to be discovered.

JOHN BUTLER BURKE.

Cavendish Laboratory, February 8.

Radiations producing Photographic Reversal.

IN a paper read before the Röntgen Society last December I pointed out that the β or γ rays from radium are capable of producing photographic reversal—a result more recently confirmed by Mr. Skinner, of the Cavendish Laboratory.

Since that particular property, shared by radiations generally, appeared likely to furnish a test of use in discriminating between rays of various kinds, I have examined also the reversing effect of polonium emanation upon photographic plates.

I find that these rays are capable of reversing pressure marks, but are unable to modify the action of X-rays or daylight upon the plate. Neither will they, even with prolonged exposure, show any tendency to reverse their own photographic effect, as happens in the case of light or rays from radium.

It is remarkable that, in their power of inducing photographic reversal, X-rays and the emanation from polonium appear to behave in a similar manner.

CHARLES E. S. PHILLIPS.

Shooters Hill, Kent, February 15.

Radium Débris.

THE valuable summary of "Researches Relating to Radium" in your issue of January 28 contained the following paragraph:—"From the disintegration theory it followed that the accumulation, during past ages, of the final products of the change of the radio-active elements must exist in the natural minerals in which these elements are found," also that helium was likely to be a product of the change.

It is the final products of the disintegration which are interesting, for if the disintegration has been going on for untold ages it is likely that other elements may have been produced from the atomic debris. It seems quite certain that radium belongs to Group ii. of the periodic classification, and no doubt to the calcium family. Is it not possible that this disintegration which has been going on for ages has given us one final "elementary" product in the shape of calcium? The close connection between the atomic weight of helium (4) and the atomic weight of calcium (40) suggests such an idea. Moreover, if one puts hydrogen in its position in the periodic classification, justified by its atomic weight, then helium comes into Group ii. along with the calcium and radium. Now, although it has hitherto been the rule to look upon the heavier elements as products of the condensation of some fundamental light substance, does not this atomic disintegration suggest that the lighter elements may be regarded as products of the breaking up of the heavier ones? When one looks at the list of elements, the small number of heavy elements impresses the mind, and the abundance of the lighter ones, those which one may regard as the final products in atomic disintegration, is well in evidence. The polymerisation of the original products of atomic disintegration may be responsible for some of the numerical relations among atomic weights.

However, attention need not be confined to the natural minerals in which the radio-active elements exist in order to