

Letters to the Editor.

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The Spectrum of Neutral Helium.

At the end of his letter to NATURE of January 13, p. 46, Dr. Silberstein appends a note to the effect that he has been able to express the diffuse series HeD' in the form

$$n = 109723 \{1/2^2 + 1/10^2 - 1/9^2 - 1/m^2\}$$

with errors of 0.7Å for the second line, and of between 0.1Å and 0.35Å for the next ten. May I be allowed to offer the following remarks:—

(1) A formula determined on a definite hypothesis, as here, ought to reproduce the wave-lengths within observation errors or at least be able to account for deviations from them. According to the data given the deviations amount to between 100 and 200 times the possible errors (700 for the second). The usual empirical formula reproduces all the lines within these limits, except the first, the O-C errors being 0.000 for $m = 2, 3, 4$ and the largest for higher values of m being 0.02. The limit is definitely within ± 0.1 of 27175.68 Å, in other words, $N(1/2^2 + 1/10^2 - 1/9^2)$ must have this value. This, of course, is possible by an empiric choice of N , but it would probably upset even the rough agreement when this is used in the last term N/m^2 .

(2) That the diffuse singlet series HeD', and indeed also the diffuse doublet HeD'', can be represented roughly in the form $A - N/m^2$, is due to the fact that for this special series the denominator in the empirical formula, $m + 0.996369 + 0.002917/m$, is necessarily very close to a whole number, and its deviations therefrom produce comparatively small effects when m becomes large. A similar arrangement in the cases of S', S'' or P'' would be found impossible.

(3) But the most fatal objection is that $N(1/2^2 + 1/10^2 - 1/9^2)$ must also be the first term of the p' sequence, which is at least numerically represented by $p'(m) = N/(m + 1.014593 - 0.004392/m)^2$. Here again the denominator is nearly an integer (though further from it than in $d'(m)$), and no doubt it could also be represented by N/m^2 , with greater deviations than in the case of d' , but the first term would then be $N/2^2$ and not $N(1/2^2 + 1/10^2 - 1/9^2)$.

It is perhaps a difference in temperament, but to me Dr. Silberstein's note appears rather to weaken than to give a "much stronger support" to his proposed theory. However, I am not here discussing his hypotheses, one objection to which I raised in a letter to NATURE on September 2 last (p. 309) which Dr. Silberstein has not dealt with.

W. M. HICKS.

January 15.

Some Experiments on Rate of Growth in a Polar Region (Spitsbergen) and in England.

In a recent paper (Journal Mar. Biol. Assoc., vol. 12, 1920, p. 355) attention was directed by me to the lack of critical evidence bearing on the theories offered to explain (a) the abundance of life in polar regions, and (b) the occurrence of several generations of a species living side by side in polar waters. Murray and Loeb and others have suggested that an explanation of these phenomena may be found in a greatly

retarded rate of growth which, it is *postulated*, must occur in the low temperatures prevailing in these regions. The present writer urges (a) that we know nothing about the rate of growth of organisms in polar regions, and (b) that the kind of metabolism of animals in polar regions—and in deep-sea situations—is not necessarily the same as that in temperate or tropical regions. A given organism may be regarded as a machine, but it is perhaps derogatory to the kind of machine one is dealing with to assume that other life-machines existing under totally different conditions are necessarily governed by identical applications of the same laws; for example, it does not necessarily follow that because the rate of metabolism in tropical or temperate animals falls off rapidly with decreasing temperatures approaching 0°C., that metabolism in polar animals is necessarily of the slow rate of temperate animals at polar sea-temperatures. No reason has yet been shown that adaptation of metabolism cannot occur; on the contrary, there is every reason to expect such adaptation.

The following experiments on the rate of growth in marine organisms at Spitsbergen—designed to obtain information on these problems—have given, however, mainly a negative result, but as in one case a positive result—yielding a much greater rate of growth than has ever been suspected—has been obtained, it is worth while recording the result now. It is hoped to write a fuller account later, giving details of the apparatus used, in the Journal of the Marine Biological Association.

In 1921 simple experiments on rate of growth were carried out in 7 fathoms of water close to Anser Island in Klass Billen Bay, Spitsbergen, by the biologists of the Oxford Spitsbergen Expedition, and mainly under the direction of Mr. Julian Huxley and Mr. A. M. Carr Saunders. The present writer had hoped to carry out the experiments under personal supervision, with the promised help of Dr. Hoel of the Norwegian Fishery Board, but circumstances nullified these arrangements.

Two pieces of apparatus were used—a galvanised iron-wire network cage of $\frac{5}{8}$ -inch mesh and 5 feet by 4 feet by 9 inches was tarred and moored to the bottom of the sea after putting a large number of dried oyster shells inside it; and a floating tarred wooden raft with strings of shells attached was anchored in the sea near the cage. The apparatus was put in the sea on June 27, 1921; the raft and shells were inspected by Mr. Huxley on July 16, and—owing to the illness of Mr. Carr Saunders—finally hauled by Mr. R. W. Segnit, geologist, and Capt. Johannsen on August 24, 1921.

On July 16 Mr. Huxley found practically no growth on the raft nor on the shells on the raft, but the cage was not hauled. On hauling the cage on August 24 the sea-urchins shown in Fig. 1 were found *inside* the cage. The door of the cage, which only covered the central portion of one long face of the cage, was found to be closed and *laced* as had been previously arranged on putting the cage in the sea. The astonishing sight of the relatively large sea-urchins inside the cage attracted attention at once, and a fruitless examination of the cage was made for any means of access greater than the mesh of the cage. The conclusion was therefore drawn that the urchins must have entered the cage while small, *i.e.* of a diameter upwards to about 1.6 cm., and grown to the size observed, *i.e.* upwards to about 2.9 cm. in diameter—excluding spines—within 58 days.

This result was regarded as very important, and a confirmatory experiment tried again at the same spot in 1922, under the direction and by the kindness of Mr. J. Mathieson, of the Scottish Spitsbergen Syndicate scientific staff. When Capt. Johannsen