almost at once. These spread out dorsally into a fan and, running between the nuclei of the superficial ectodermal cells, end directly against the cuticle. Ventrally they terminate in a narrow bundle against the cuticle of the mid-ventral line. For a considerable time no change other than growth takes place, but ultimately, at the dorsal end, at the level of the inner face of the surrounding ectoderm cells, the fibrils lose their staining capacity and become replaced by a tendinous plate. Below this plate the fibrils now divide into segments converting the strings of cells into typical striped muscles. Above it they persist as a radiating series of "tendo-fibrils," showing no signs of segmentation and ending directly against the cuticle.

On the outer side of this series is another set of muscles having the same median ventral attachment, but having an upper attachment just below—that is, more lateral to that of the first series. These are also entirely ectodermal in origin, and are formed in the same way as the first series.

The important facts that emerge from this development are, first, that certain muscles of the Crustacea are definitely ectodermal in origin, a fact not at all in conflict with what might be deduced from the ancestry of the group; and, secondly, that the "tendo-fibrils" that run from the cuticle through the ectoderm cells to attach to endoskeletal structures may be the remains of the same originally continuous fibrils that divide up elsewhere to give the myofibrils of typical striped muscle.

H. GRAHAM CANNON.

Zoology Department,
Imperial College of Science,
South Kensington, S.W.7.

On the Absorption Spectrum of Aluminium.

IT is well known that the study of the absorption spectrum of elements provides us with the simplest method of determining the normal states of atoms. In the hands of Wood, Bevan, McLennan, and others, the study of the absorption spectrum corroborated that in the case of elements of the first and the second groups, the normal orbits are those designated spectroscopically as $1s$ and $1S$ respectively.

In the case of elements of higher groups, the experiments become more difficult, as most of them have high boiling-points, so that with the furnaces which can be commanded in a physical laboratory, very little vapour can be obtained for absorption work. This difficulty becomes more acute in the case of metals of the third group, all of which, excepting thallium, yield very little vapour up to temperatures of 1200° C.; and, in fact, we are not aware that any successful experiment has been done on the absorption spectrum of aluminium and boron, which are the least volatile elements of this group. At the same time, such experiments are necessary for determining once for all whether for these elements the $2p_1$ orbit is the normal stage, as has been obtained from the analysis of their arc spectra and corroborated by the absorption experiments in the case of indium, gallium, and thallium.

We have recently carried out successfully the absorption experiment with aluminium, using the vacuum furnace designed by Prof. Meghnad Saha for ionisation work. The furnace consists of an Acheson graphite tube heated by a battery of accumulators. The temperature was simultaneously measured by a Wanner pyrometer. We used a cadmium spark, and a copper spark under water as our sources of continuous light. The spectrum was photographed on Ilford ordinary plates sensitised by nujol, as described by Lyman. We found that no aluminium lines are obtained below 1500° C. At 1520° C., the pair $\lambda = 3961$, 3944 come out reversed. At 1650° C., the leading members of $2p_i - md$ series come out reversed. The higher members of $2p_i - ms$ series require a little higher temperature. Another curious feature is that on all plates the $2p_i - ms$ lines and $2p - md$ lines of gallium occur rather prominently. Apparently gallium occurs as an impurity in ordinary samples of aluminium.

From such experiments it is not possible to deduce whether for aluminium the $2p$- orbits are the normal orbits, or whether there is a still larger $1s$ orbit. For at the temperature at which sufficient vapour is available for absorption work, the thermal stimulus is quite sufficient to convert any lower $1s$ orbit to the $2p$-orbits. The vapour pressure of aluminium is evidently very low even at 1520° C., but we could not find any existing data on the subject, except some theoretical considerations by Grüneisen. Our apparatus is suited to the determination of vapour pressure of aluminium over a large range of temperature by using the so-called *Mitfuhrungs-methode* of Pfaundler, and we hope to carry it out at an early date. At any rate, it is quite clear that the $2p - mx$ lines require a lot of vapour for absorption, and if the $2p$- orbits turn out to be the normal orbits of aluminium atom, they do not dominate the spectrum to the same extent as the $1s$ orbits of alkalies dominate their spectra.

KANAKENDU MAJUMDER.
NALINE KANTA SWE.

Physical Laboratory,
Allahabad University, India.

The Origin of Sponge-spicules.

IT is to be regretted that Dr. Bidder (NATURE, February 28), before publishing his criticisms of my theory of the symbiotic origin of sponge-spicules, did not await the publication of the detailed evidence upon which that theory is based. I am loth to enter into controversy with him, but as his letter contains much that is misleading, I feel that I can scarcely pass it over in silence. He speaks of the observation (presumably mine) "that the first rudiment of the spicule in Stelletta is a skeleton-crystal on the tetrahedral system." I made no such observation. On the contrary, I endeavoured to show by observations on the silica pearls that the first rudiment is a minute granule resembling a Micrococcus.

Dr. Bidder's "crystallographic explanation," so far as siliceous sponges are concerned, appears to rest on the assumption that the protorhabd, or axial thread, is itself composed of silica. Otherwise it would be difficult to understand his "conjecture" as to the variation of the type of crystallisation with the percentage of water in the "spicopal." His supposed "skeleton-crystal" can be nothing but the radiating axial threads. He admits that the silica afterwards deposited upon it is "in amorphous aggregation." Unfortunately, the best observers, such as Bütschli and Schulze, are in agreement that the axial thread is composed of a protein substance, and there is nothing else that could form the siliceous skeleton-crystal which he imagines to exist. The statement that my scleroplastids "are at first gelatinous but become crystalline" is pure invention. It is well known that the axial thread, formed, as I believe, by elongation of the scleroplastid, retains its original "organic" character in the axial canal of the fully formed spicule, and, so far as I am aware, no one has hitherto ventured to suggest that it becomes crystalline.

Dr. Bidder accuses me of having "strangely changed" the name of a certain sponge to Donatia. If he will study the literature of the subject he will