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II. Wissenschaftliche Mittheilungen.

1. On the Segmental Organs of Polynoë.

By William A. Haswell, M.A., B.Sc., Edin., Sydney.

eingeg. 16. Januar 1885.

Mr. Bourne seems to misunderstand altogether the object of the note on the above subject which he criticizes in No. 178 of the *Zoologischer Anzeiger*. The object was, not to assert that I had first discovered the external openings of the segmental organs of *Polynoë* but

to shew that certain observations of mine had priority of publication over certain observations of Mr. Bourne's. I say priority of publication, for that is the only sort of priority we can argue about; the fact that Mr. Bourne conducted his investigations in Naples in March, 1882, that he submitted his report to the British Association Committee in June, etc. etc., has nothing to do with and merely tends to confuse the main issue. The fact remains that the first publication of the results of Mr. Bourne's work was in September 1883, and that of mine in Sydney in August 1882 (equivalent to a publication in London six to eight weeks later) and in the *Zoologischer Anzeiger* in October 1882, the latter date being more than two months before Mr. Bourne's paper was read at the Linnean Society. Whatever then the respective merits of the papers in question I contend that to notice in a foot-note with the preface »Since the above was written« (as if it had just been seen in time for the press) a paper published in a widely circulated periodical a considerable time before, is scarcely the sort of treatment calculated, as Mr. Bourne says he intended it, to »encourage the meritorious activity of a colonial naturalist.«

Mr. Bourne states that I wish to claim priority of discovery of the external openings of the segmental organs of *Polynoë* and that I am very bold in doing so. Now, as I pointed out in a note on the subject of those organs also published before Mr. Bourne's original paper was read, that the openings were known as regards the genus *Aphrodita* to Treviranus and to Quatrefages and doubtless to many between them, and as regards *Hermadion fragile* to Claparède, this statement must be untrue.

But Mr. Bourne also alleges that I did nothing more than to serve the external openings (which had been done before), gave no figure »because I had seen nothing that could be figured« and merely expressed the »pious opinion« that the openings were the openings of segmental organs, and that, therefore, there was nothing that Mr. Bourne need have noticed in the body of his paper. Now all this is not only unfair, but untrue. I do not think that either Mr. Bourne or myself have added greatly to the facts known about the segmental organs of the *Aphroditea*, but, as anyone who reads my papers can readily ascertain, Mr. Bourne is taking quite the wrong way to justify himself. I not only pointed out the interpretation of the statements on this subject of Williams and of Ehlers and described the position of the efferent duct of the organs, but I also traced the ciliated tube inwards into the body and towards the middle line, observed the presence of concretionary matter in its walls, found in several instances spermatozoa passing naturally out through it (a fact which

runs counter to the opinion as to the sole functions of the organ expressed by Mr. Bourne in his paper); pointed out that (as might have been expected if the organ serves as the efferent duct of the reproductive organs), the orifice is wider and the papilla on which it opens shorter in females than in males; described in the females of species in which the eggs are hatched under the elytra the arrangement of long cilia on the parapodia by which the ova might be carried upwards to this position, and pointed out this absence of these cilia in forms in which the eggs are not received under the elytra. These were all facts which are of some importance in determining the functions of the organs in question; yet, according to Mr. Bourne they amount to no more than the expression of the »pious opinion« that the openings are the openings of segmental organs.

But I have something more against Mr. Bourne which, as it is concerned with the acceptance as an ascertained fact of which is simply a new form of an old error, it is necessary I should allude to here. Mr. Bourne's chief claim to originality of discovery in connection with this subject, as contrasted with my expression of pious opinion, is his discovery of the character of the internal openings of the nephridia, and he gives a figure of the entire organ which he states is a diagram compiled from observations on specimens under the compensorium, teased specimens and sections: in this he represents the organ as opening into the perivisceral cavity by a wide, funnel-like, ciliated mouth.

Now Mr. Bourne has here fallen into an error which, in other forms, has been committed before, and which is perhaps excusable enough, but it would be a great pity should the error come to be re-accepted as an ascertained fact of science as might very well happen. The segmental organ of *Polynoë*, as of *Aphrodita*¹, does not end in any such dilated internal opening as Mr. Bourne describes, and the error into which he has fallen has sprung from a neglect to study carefully the whole structure of the animal. The arrangement of the intestinal caeca in particular he does not seem to have understood; he alludes to the ciliated funnels described by Ehlers, and merely adds that Grube was unable to trace the connection between these and the external openings. Now I shewed in my paper that these ciliated funnels, as can be seen with the utmost distinctness in transparent forms, such as *Antinoë praeclara*, are the openings of the narrow ciliated necks of the caeca into the intestine. When the caeca are drawn upon they are readily torn away and separated from the intestine, leaving on the one

¹ For an accurate drawing of the general form of the organ in the latter genus see Selenka, »Das Gefäßsystem der *Aphrodita aculeata*«. Nied. Arch. Zool. Bd. II. Taf. III.

hand in the wall of the intestine a row of apertures and on the other a series of free ciliated funnels which under those circumstances have very much the form and exactly the position which Mr. Bourne figures as possessed by the supposed internal opening of his segmental organ, with the openings directed inwards. In specimens which are »teased« or roughly dissected this is particularly likely to happen, and the inference that this free-opening, inwardly-directed, ciliated funnel is the internal termination of the segmental organ is a very natural one.

I have no fondness for »controversies« and cannot see how I can benefit by »seeking« one with Mr. Bourne whom I only know of as a rising young zoologist. I have simply stated the facts as they appear to me. But I cannot help in conclusion expressing the hope (in which I am sure I shall have the sympathy of all who have read Mr. Bourne's very injudicious note) that he will in future bear in mind that »Colonial Naturalists« (among whom I can scarcely yet rank myself) require no other treatment, no more patronage and no more neglect, than naturalists at home.

University of Sydney, November 27th 1884.

2. Artificial Fecundation in the Mollusca.

By William Patten, Ph.D., from Boston U.S.A.

eingeg. 27. Januar 1885.

During a short stay at the Zoological Station at Trieste my attention was called to the development of *Haliotus* and *Patella*. In September and early October many specimens of *Haliotus* were found containing either ripe ova or active spermatozoa. All attempts, however, to procure fertilized ova were fruitless and toward the end of October no more ripe ova or spermatozoa could be obtained.

I have been unable to find in the literature upon the subject any reference to the external appearance or deposition of the ova of either *Patella* or *Haliotus*. The absence of any external sexual organ or any gland secreting a substance for attaching the eggs to foreign objects or for holding them together, and the fact that the eggs had not been observed by any one led me to the conclusion that they were probably deposited singly in the water and there underwent an external fecundation.

As the animals would not deposit their eggs in confinement I determined to try artificial fecundation and was greatly pleased to find on the first trial that after about four hours quite a number of the ova experimented with were in the first stages of segmentation. This was of special interest to me, as I know of no instance of artificial impreg-

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Artikel/Article: [1. On the Segmental Organs of Polynoe 233-236](#)