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II. Wissenschaftliche Mittheilungen.

1. Professor Perrier's historical criticisms.

By P. Herbert Carpenter, D.Sc., F.R.S., F.I.S., Assistant Master at Eton College.

eingeg. 8. December 1886.

In the first part of a work upon the minute anatomy and organogeny of *Antedon rosacea*¹, which is destined from its extent and from the wealth and beauty of its illustrations, to become a classic at no

¹ Mémoire sur l'Organisation et le Développement de la Comatule de la Méditerranée. Nouv. Arch. du Mus. Hist. Nat. 1886. 2 Sér. T. IX. p. 53—176. Pl. I—X.

distant date, Professor Perrier has thought fit to bring a charge against me of so unpleasant a nature that I cannot allow it to pass without notice.

It will be seen subsequently from the very nature of this charge, that it relates to matters about which Perrier could have absolutely no personal knowledge whatever. I shall quote in full the statement which he has permitted himself to make, and will only say now that it is utterly untrue, and leave him to extricate himself as best he can from the unpleasant position in which he has placed himself.

The history of the matter is as follows:

In a general description of the Pentacrinoid larva of *Antedon* published in 1866, Dr. Carpenter² said:

»Beneath the tentacular canal a tubular extension of the perivisceral space passes along the ventral surface of each ray; and although this appears to form but a single canal, I shall hereafter show that it is very early divided by a horizontal partition extended from the membranous bands that suspend the digestive cavity in the perivisceral space; and that whilst the canal above the partition communicates with the portion of the perivisceral space which lies immediately round the mouth, the canal beneath the partition is extended from the portion of the perivisceral space which occupies the hollow of the calyx. The former I shall term the subtentacular, and the latter the coeliac canal; their relations will be found very remarkable.«

Dr. Carpenter thus described the subtentacular canal as separated from the coeliac canal below it by a horizontal partition.

The following version of this observation was given by Perrier³: »... Comme Müller, dont il s'est évidemment et à juste raison très-pré-occupé de retrouver les résultats, le docteur Carpenter a vu d'ailleurs, dit-il, le canal tentaculaire divisé verticalement par une cloison transversale dans certaines parties de son étendue.«

Perrier has here confounded two entirely distinct observations⁴ the one by Müller, the other by Dr. Carpenter. It is well known that Müller's tentacular canal is the subtentacular canal of later observers, and that it is frequently divided into two parts, right and left, by a vertical partition. But the partition described by Dr. Carpenter was a horizontal one, dividing the subtentacular from the coeliac canal. He spoke of these two canals on another page (702) in the following

² Phil. Trans. 1866. p. 728.

³ Recherches sur l'Anatomie et la Régénération des Bras de la *Comatula roseacea*. Arch. de Zool. Exp. et Gén. T. II. 1873. p. 36.

⁴ I would here beg the reader to notice, for reasons which will appear subsequently, that this is the first occasion on which I have referred to this error of Perrier's, nearly fourteen years after it was committed.

terms: »... It will be shown in the Second Part of this Memoir that, besides the so called »ambulacral« canal with its tentacular extensions, each arm and each pinnule contains an afferent and an efferent canal, in which the nutritive fluid is exposed to the aerating influence of the surrounding medium.« Perrier's comment on this passage is that he has been unable to find these afferent and efferent canals, and he attributes Dr. Carpenter's description of them to an error of observation; while on p. 73 he goes so far as to say of one of them that »nous demeurons convaincu que personne ne le reverra«.

He now admits his error, however, but attempts to excuse himself for having committed it in a way which only makes matters worse. He says that the thin-section method was comparatively unknown in France at the time of his work, and that Dr. Carpenter »ne disait pas, dans son mémoire, avoir fait de coupes dans les bras de la Comatule«⁵. This is a singularly unfortunate excuse; for on page 719 of the text Dr. Carpenter described the appearances presented by the horizontal and vertical longitudinal sections of the decalcified arm which he represented in pl. XLIII, figs. 6 and 7; and the explanation of fig. 2 on the same plate commences »Transverse section of a decalcified arm«. Dr. Carpenter's preparations, now over twenty years old, were not the thin transparent sections of the more recent zoological work. They were merely slices of the decalcified arm, cut with a sharp knife or a pair of fine scissors; and if Perrier had simply taken the trouble to cut a piece of arm in two with a scalpel, he could have convinced himself at once, as he has since done, that Dr. Carpenter's description of the two lower arm-canals was correct. I could mention other errors contained in his criticisms of Dr. Carpenter's work; but those which I have exposed are sufficient for my present purpose. It does not seem to have occurred to M. Perrier, who was then a young and unknown man, that Dr. Carpenter would not have been likely to make such very definite statements without having good reason for them. If Perrier had contented himself with simply saying that he had been unable to find one of the structures described by Dr. Carpenter, no harm would have been done; but with the easy confidence of his inexperience, he took upon himself to say that he was convinced that no one else would ever do so in future. This was a scarcely courteous mode of referring to the work of a man who had achieved a European reputation before M. Perrier was born; but more than two years passed before Dr. Carpenter took any public notice of these errors. On January 20, 1876 he presented to the Royal Society an abstract

⁵ Nouv. Arch. du Mus. Hist. Nat. T. IX. p. 91.

of the results at which he had arrived ten years previously; and he referred to Perrier's work as follows⁶: «... This observer has confined himself to the study of the arms, examining their terminations as transparent objects. In this manner he has added much to our knowledge of their histology; but through not having examined transverse sections of the arms and pinnules, he has not only failed to recognise the true tentacular canal⁷, but has been led to affirm that there is only one canal system in the brachial apparatus.»

For reasons which I shall explain immediately, I was led at the end of the year 1875 to make some thin sections of the arms of *Comatulæ*; and I now give verbatim Professor Perrier's account⁸ of what followed.

«A ce qu'il raconte lui-même dans le préface de son mémoire sur les Crinoïdes du Challenger, il fut conduit à s'occuper des Comatules par le désir bien légitime de démontrer l'exactitude de quelques observations contestées de son père relativement à l'anatomie des bras de ces animaux⁹. Dans le but de se mettre au courant des méthodes nouvelles d'investigation, il alla travailler à Würzburg, sous la direction du professeur Semper, qui venait de publier, en réponse à mon mémoire de 1873, les remarques qui ont été précédemment analysées. Comme j'étais encore seul, in 1875, à avoir repris les observations de W. B. Carpenter sur les Comatules, Herbert Carpenter entra donc dans la carrière scientifique en fourbissant soigneusement ses armes dans l'intention préméditée d'attaquer mon premier travail sur les Crinoïdes; dans ces conditions, il n'est pas très étonnant que je n'aie jamais réussi depuis à m'entendre complètement avec le zoologiste d'Eton, qui est d'ailleurs demeuré l'adversaire de tous les travaux publiés en France sur les Échinodermes. Ces travaux sont toujours pour lui, selon la traduction de M. Joliet, »les travaux de l'École française et de son principal membre, le professeur Perrier,« travaux auxquels sont naturellement opposés par le disciple du laboratoire de Würzburg les travaux de l'École allemande. Ce n'est évidemment pas un parti pris, c'est un simple tendance qui se révèle dans les critiques d'Herbert Carpenter.»

⁶ Proc. Roy. Soc. 1876. Vol. XXIV. p. 212.

⁷ Perrier now says that this statement is incorrect; and it is probable that he is right.

⁸ Nouv. Arch. du Mus. Nat. T. IX. pp. 130, 131.

⁹ «Voici ce passage qui met en relief d'une singulière façon, chez son auteur, cet amour-propre et cette tenacité que l'on considère à bon droit comme les qualités maîtresses de la race anglaise: 'The researches of my father, Dr. Carpenter, C.B., F.R.S., early led me to take a special interest in *Comatula* and its allies. Some of his statements respecting the anatomy of the arms having been called in question, I was led to reinvestigate the matter towards the end of the year 1875 by methods which were almost unknown during the progress of his researches, nearly fifteen years before; and I had the pleasure of verifying all those points in his descriptions of the arms of the European *Comatulæ* which other observers had disputed'. C'est dix ans après ces premiers travaux que le savant anglais éprouve encore une visible satisfaction à en rappeler l'origine!»

On a previous page¹⁰ he speaks of Dr. Carpenter »dont le fils Herbert allait la même année étudier à Würzburg, les Comatules sous la direction du Professeur Semper«. The above statements of Perrier's can have only one meaning. He asserts that I went to Würzburg in order to work at the Comatulæ under Professor Semper, with the deliberate intention of attacking his work on *Antedon rosacea*, which was then two years old.

I have only one reply to make to this charge which Perrier has thought fit to bring against me, and that is to meet it with a direct, absolute, and unqualified denial. His statement is absolutely and entirely untrue. In this case, as in so many others, he has committed himself to a generalisation which will not bear investigation.

When I first went to Germany, nearly twelve years ago, it was without the very slightest intention of taking up the Comatulæ as a subject of special study. I had not even read Perrier's memoir of 1873, and knew nothing about his criticisms of my father's work. I knew, as did most zoologists, that my father had accumulated a large amount of unpublished observations on the anatomy of recent Crinoids, and I was advised by those in whose judgement I had confidence to take up some different line of investigation from that to which he had devoted himself. I therefore went to Würzburg in February 1875, with the intention of commencing a research into the minute anatomy of certain parts of the brain, which had been suggested to me by my teachers at Cambridge as a promising field for investigation. When I arrived at Würzburg, however, I found Professor Semper much occupied with the study of the development of the urogenital system in the lower Vertebrates and its relation to that of the Worms. He suggested that I should abandon my proposed subject, and devote myself instead to the minute anatomy of the genital glands in the Crayfish. To this I agreed, and I spent some weeks cutting sections of the testis and ovary of these animals, which I still have. In May, however, I returned to England to accompany Sir George Nares's Arctic expedition as far as Disco Island, for the purpose of assisting in the dredging operations which were carried out there and in the North Atlantic by H. M. S. Valorous. Upon my return to England in September I found that a translation of Professor Semper's »Brief note upon the anatomy of *Comatula*« had been just published in the Annals and Magazine of Natural History, together with an addendum by my father. Semper's observations had been made on a new *Comatula* from the Philippines, my father's on *Antedon rosacea*, and their results were somewhat divergent. As I was about to return to Würzburg for the purpose of continuing my studies on the

¹⁰ Ibid. p. 91.

Crayfish, I suggested to my father that it would be a good opportunity for me to try and reconcile the differences between his own observations and those of Professor Semper, who would probably still have in his possession the sections which he had made in the Philippines and had used as the basis of his descriptions. The idea of making a deliberate attack on the work of M. Perrier, with which I was, even then, most imperfectly acquainted, never presented itself to me at all. For I knew that my father was preparing a memoir for the Royal Society, which would at once vindicate the truth of his previous descriptions and expose the errors into which Perrier had fallen.

Upon my arrival in Würzburg, Professor Semper at once acceded to my request for permission to see his sections, which he kindly placed in my hands for re-examination; and he was good enough also to put at my disposal various pieces of the arms of his Philippine *Comatula* which I could cut for myself. I need not go into the whole question again. It is sufficient to say that the differences between my father's results and those of Professor Semper were reconciled in a manner satisfactory to both parties. But the further observations which I was thus enabled to make upon the structure of the arms in the Philippine species seemed to indicate that it presented a most excellent field for inquiry. Professor Semper generously placed at my service all his examples of this particular type, the investigation of which occupied my whole time for many months, and led to the entire abandonment of my original plan of work for the winter of 1875—1876.

(Schluß folgt.)

2. Über den Bau und die Entwicklung von *Heterodera Schachtii* Schmdt.

(Vorläufige Mittheilung.)

Von Ad. Strubell, stud. rer. nat. aus Frankfurt a/M.

(Schluß.)

Das Ei unserer *Heterodera* besitzt die Gestalt einer Bohne oder Niere und wird von einer zarten Dotterhaut und einer derberen, structurlosen Schale umschlossen. Die Dotterelemente sind sehr groß, was den Einblick in die Umbildungsprocesse beträchtlich erschwert. Die Entwicklung hebt bereits im Uterus an, wo man Eier in den verschiedensten Furchungsstadien antrifft. Hinsichtlich einer genaueren Schilderung der Klüftungsvorgänge, wie auch bezüglich der weiteren embryologischen Details verweise ich auf die ausführlichere Arbeit. Hier sei nur bemerkt, daß ich bei *Heterodera* eine so gesetzmäßige Verlagerung der Blastomeren, die, wie Hallez neuerlich behauptete, bei allen Nematoden die gleiche sein soll, nicht beobachten konnte.

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