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(*Hemerobid.*)

II. Wissenschaftliche Mittheilungen.

1. Professor Perrier's historical criticisms.

By P. Herbert Carpenter, D.Se., F.R.S., F.L.S., Assistant Master at Eton College.
(Schluß.)

eingeg. S. December 1886.

I have thus stated at some length the causes of my entering the field of Crinoid-research, in order to show how entirely groundless is the degrading charge which Professor Perrier, in his absolute ignorance of the facts, has thought fit to bring against me. If he wishes for any confirmation of my statements, I can refer him to Professor Semper himself, and also to my friends Prof. Max Braun of Rostock, Dr. J. W. Spengel of Bremen, and Dr. C. S. Minot of Boston, U. S., who were occupants of Professor Semper's laboratory in the winter of 1875—1876, and were fully acquainted with the causes and facts of my own work.

Will Professor Perrier name one single passage in the first paper¹¹ which I published on the Crinoids in April, 1876 which can possibly justify his assertion that I had formed the deliberate intention of attacking his work? Had this been the case I should naturally have noticed his extremely incorrect references to the results of his predecessors, one of which I have now exposed for the first time, nearly

¹¹ Remarks on the Anatomy of the arms of the Crinoids. Journ. Anat. and Physiol. Vol. X. 1876. p. 571—585.

eleven years after I first began to write on the subject. If he wishes for information respecting the others, I shall be ready to give it to him whenever he likes.

Between April, 1876 and April, 1881, a period of five years, I published eleven papers on recent *Crinoidea*. But in no one of these did I make any reference to Perrier's errors, not even in that »On the Minute Anatomy of the Brachiote Echinoderms« where I might naturally have exposed them in detail, had I so desired. In fact his name is not even mentioned in this paper.

In 1882, however, after a six years interval, I again ventured on some criticisms of Perrier's work. For I attempted to discuss some of the observations upon the vascular system of Echinoderms which had been published by the French school and by the German school respectively.

I used these names because they naturally occurred to me when I found that such apparently contradictory results had been obtained in the laboratories of the two countries. The statements of the French naturalists, led by Perrier, rested mainly on the results of injections, and those of the German authors on the section method. Having myself more faith in the latter than in the former mode of investigation, I ventured to suggest the possibility that the connection of the ovoid gland (or supposed heart) with an oral ring might »have been overlooked by the French naturalists¹². Perrier had totally denied the existence of such a connection in *Echinus*¹³. But the later researches of Koehler and Prouho, both members of the French school, have conclusively proved that he was quite wrong, and that there is not only a second oral ring (i. e. one in addition to that of the water-vascular system) which is in connection with the ovoid gland, but also a second set of radial vessels which had entirely escaped Perrier's notice. An important part of my criticisms on Perrier's researches (which were of the mildest character) has thus been abundantly justified, and I may say the same of my remarks on the earlier work of M. Koehler, a fact which will be evident to all who are familiar with the subject. It will not, I trust, be considered as any breach of confidence for me to state that in July last I had the unexpected pleasure of receiving a most courteous letter from M. Koehler, in which he acknowledged the justice of some of my criticisms on his researches on the vascular system of the Urchins. For Teuscher's pharyngeal vessels, of which he formerly denied the existence, have since been injected both by Prouho and by himself,

¹² Quart. Journ. Mier. Sc. 1882. Vol. XXII. New Ser. p. 372.

¹³ Arch. de Zool. Exp. et Gén. 1875. T. IV. p. 613.

and their results have been confirmed by the section method. But this second system of vessels, with its oral ring and radial extensions had altogether escaped the notice of Prof. Perrier, who had limited himself to the injection method without properly controlling his results by the use of sections.

So much then for Perrier's statement that I have been the opponent of all the works published in France upon the Echinoderms. I will now pass on to explain what he calls the »simple tendance qui se révèle dans les critiques d'Herbert Carpenter«.

I freely confess that (apart altogether from the question of the vascular system of the Urchins) I have published some strong criticism of Perrier's work during the last three years. But I have not done so without good reason. He has frequently committed himself to statements which he would scarcely have made, had he taken the trouble to become sufficiently acquainted with the work of his predecessors. Thus for example, early in 1883, he established a new genus of Crinoids, (*Democrinus*) on the very character which had been pointed out as distinctive of *Rhizocrinus* by Pourtalès in 1868 and 1874, and by myself in 1877 and 1882; though copies of both my own papers were sent to Prof. Perrier. He has since admitted the justice of my criticisms by tacitly withdrawing a generic (and also a specific) name with which zoological literature should never have been burdened.

From his very first essay in 1873 to his latest one in 1886, Perrier's publications on the Crinoids have contained the most remarkably incorrect versions of statements made by his fellow-workers¹⁴. I have already noticed his confusion of two entirely distinct observations by Müller and my father respectively, in 1873. I published last year a number of corrections of the blunders which he had made in an article on my Report on the Challenger *Crinoidea*¹⁵; and I now select one of many erroneous references to the writings of Ludwig, myself, and others which have appeared in his latest publication.

On p. 133 when criticising my two papers of 1876, he says of the first: »Les corbeilles vibratiles du canal dorsal et les corps sphériques des bras lui paraissent être des organes des sens;« and of the second: . . . »Les corps sphériques y sont désignés sans point de doute comme des ,organes des sens' problématiques.«

Of the three statements contained in these two sentences two are absolutely false, and one greatly exaggerated. I never said a word

¹⁴ A reference to the writings of Bell and of Sladen upon the Starfishes will show that others besides myself have been obliged to comment upon Perrier's inaccuracy.

¹⁵ Ann. and Mag. Nat. Hist. 1885. Ser. 5. Vol. XVI. p. 100—119.

in my first paper about the ciliated cups in the dorsal canal of *Comatula* being organs of sense, and in the second paper I never referred to the »corps sphériques« at all. The sarcasms in which Perrier indulges respecting my supposed views, thus lose all their point, or rather, acquire an entirely new one.

In the January number of the Quarterly Journal of Microscopical Science I have quoted the statements which I really did make, and have explained the marvellous confusion of ideas which has led Perrier to give these totally incorrect and misleading versions of them.

Not only is he quite extraordinarily careless in his references to his fellow-workers, but he has (as I remarked in August 1885¹⁶) far too strong a tendency »to make a sweeping generalisation upon data which are either altogether inadequate, or even absolutely incorrect«. An excellent instance of the latter kind is afforded by his statement respecting the presence of radiating cavities at the syzygies of the talked Crinoids. I asked him then to name a single recent talked Crinoid in which the syzygial faces are separated by radiating passages as in the *Comatulæ*, and there are pores round the outline of the syzygy.

He has given no answer to my question; so I will now repeat it, and add to it another. Can he name a single Blastoid in which there is evidence of a direct communication during life between the body-cavity and the external water? He has recently described a means by which water can penetrate directly into the coelom of a Starfish, while he also believes that in the Urchins and Crinoids its course is regulated by a complex »système de canaux d'irrigation«; and he continues¹⁷: ... »Cela autorise à diviser l'embranchement des Échinodermes en deux grandes groupes, comprenant les Cystidés, les Blastoïdes, les Stellérides et les Ophiurides d'une part, les Crinoïdes, les Échinides, et les Holothurides d'autre part.« On what observations does he rely for this statement about the Blastoidea? I know of none which can possibly justify this generalisation, and of a great many which directly contradict it.

I cannot but think it a matter for very great regret that a zoologist who is capable of the admirable work expressed in the beautiful illustrations of *Comatula*-anatomy which Professor Perrier has lately published, should be so extraordinarily inaccurate in his references to his fellow-workers as I have shown him to be; and that he should

¹⁶ Ann. and Mag. Nat. Hist. 1885. Ser. 5. Vol. XVI. p. 117.

¹⁷ Comptes rendus. T. CII. 1886. p. 1148.

lend the weight of his official position to the propagation of extensive and far-reaching generalisations which are (as yet) absolutely without any foundation of anatomical fact.

2. Die Halsgegend der Reptilien.

Von Dr. J. F. van Bemmelen in Utrecht.

eingeg. 19. December 1886.

Die Resultate meiner Untersuchung über die Entwicklung der Visceraltaschen und -Bogen bei Reptilien (diese Zeitschrift, No. 231 und 232) machten bei mir den Wunsch rege, *Hatteria punctata* auf den Bau ihrer Halsgegend zu untersuchen.

Durch die Freundlichkeit des Herrn Prof. Hubrecht, der ein Exemplar aus der Sammlung des Utrechter Universitätsmuseums mir zur Verfügung stellte, konnte ich diesen Wunsch befriedigen.

Die Halsgegend der *Hatteria* entspricht in ihrem anatomischen Baue vollständig derjenigen der typischen Saurier, besonders der Ascalaboten, mit denen *Hatteria* ja auch in anderen Hinsichten die meiste Verwandtschaft zeigt.

Die Thymus besteht jederseits aus zwei hinter einander gelegenen Stücken, zur Seite des Oesophagus in der unmittelbaren Nähe von Carotis interna, Vena jugularis, Nervus vagus und sympathicus. Der hintere Lappen ist dreimal länger als der vordere, und erreicht mit seinem Hinterende die Ursprungsstelle der Carotis interna aus dem Carotisbogen. Hier hängt er mit einem runden glänzenden Körperchen zusammen, das der hinteren Wand des Carotisbogens dicht angewachsen ist, an der Stelle wo dieser sich zur Vereinigung mit dem Aortabogen rückwärts wendet. Ein dergleichen rundes Körperchen findet sich auch an der Hinterwand des Aortabogens selbst. Mit Hinsicht auf meine Untersuchungen über die Entwicklung der Thymus und der epithelialen Derivate der Visceralspalten bei *Lacerta*, geht aus diesen Befunden hervor, daß die Thymus der *Hatteria* höchst wahrscheinlich eben so wie die der *Lacerta* aus den Gipfeln der zweiten und dritten Spalte entsteht, während der übrige Theil der dritten Spalte das Carotiskörperchen liefert. Das dem Aortabogen anliegende Körperchen darf wohl als Derivat der vierten Kiementasche betrachtet werden, so daß in dieser Beziehung *Hatteria* ein primitiveres Verhältniß zeigt als *Lacerta*, bei der die vierte Tasche schon während der früheren Stadien des embryonalen Lebens verschwindet. Einen Rest einer fünften Kiementasche konnte ich nicht auffinden, eben so wenig wie einen asymmetrischen suprapericardialen Körper. Damit ist aber das Fehlen eines dergleichen Gebildes noch nicht bewiesen, denn bei einem er-

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