

3. Further remarks upon Professor Perrier's historical errors.

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Professor Perrier's reply to the questions which I have addressed to him on the subject of his historical criticisms is of a singularly futile character. Some questions he leaves unanswered, I hope, because he means to deal with them in his forthcoming memoir. He attempts to explain one of his erroneous statements by invoking a printer's error, and by a quibble as to the meaning of the words »vertical« and »transverse«. Lastly in defence of another incorrect statement he expresses his inability to understand that the term »section of a decalcified arm« meant that the arm was decalcified before it was cut into sections. He seems to consider that these paltry excuses are sufficient to justify his repeated misstatements about my father's work to which I have drawn attention. Let us consider his reply in more detail. 1) I must again ask him to furnish a definite proof of the assertion which he has thought fit to make respecting my motives for commencing the study of the Comatulæ. I have explained already, and can refer to professor Sempër for confirmation of my statement, that my object was to reconcile his own observations on a Philippine *Comatula* with those of my father on *Antedon rosacea*. My father had described the water-vessel of the latter type as lying immediately above what is now known as the ventral canal of the arm, while Sempër (1874) found this position to be occupied by a cord which he called *x* and identified with Perrier's muscular band. My father therefore suggested that this might perhaps be the collapsed water vessel. Ludwig, writing in 1875, found, however, as Perrier had previously done, that both muscular band and water vessel are present in *Antedon rosacea*; and he therefore pointed out that the former could not be identical with the cord *x* which occupied in Sempër's section the same position as the water-

treffen, ist die factische Seite absolut eine und dieselbe und nur in der polnischen, späteren, die Deutung der Thatsachen eine etwas differente. Daß die Isopoden zu den Pseudocoeliern (!) gehören, habe ich nirgends geschrieben und Niemand ein solches Absurdum gesagt, und Herr Wassiliew, dessen Zeugnis Herr Groszlik fälschlich anführt, autorisirt mich zu erklären, daß er nie von mir eine solche Meinung vernommen hat. Zieht aber Herr Groszlik allein einen solchen Schluß aus meinen Beobachtungen über *Oniscus* (Zool. Anzeiger No. 228) heraus, so ist das nur ein Beweis, daß Herrn Groszlik die ganze neuere embryologische Litteratur (seit dem Jahre 1881, d. h. seit der Erscheinung der Coelomtheorie) fremd ist.

vessel in *Antedon rosacea*. At the same time Ludwig, as other naturalists had privately done, expressed his inability to accept my father's views about the nervous system. The result was that nearly three months afterwards (March, 1876) I wrote a short paper containing the results of my own observations on the Philippine *Comatula*, which afforded me the means of reconciling the conflicting statements of my father, Semper, and Ludwig. At the same time I brought forward important additional evidence for the truth of my father's statements about the nervous system, which had been publicly called in question by Perrier and Ludwig, and also, though I did not then know it, by Greeff. My references to Perrier were of a studiously courteous character, and he has neglected to reply to my request that he would name one single passage which can possibly justify his assertion that I had formed the deliberate intention of attacking his work. He only refers his readers again to the preface to the Challenger report, and to his own interpretation of it. Unfortunately, however, he has made yet another incorrect statement upon which the whole question turns. Were this assertion only true, there might be some reason in his interpretation of my remarks that »some of my father's statements respecting the anatomy of the arms having been called in question, I was led to reinvestigate the matter«. Perrier has thought fit to interpret this passage as applying exclusively to himself. He is of course unaware of all the criticisms of my father's views which had been made in private both to him and to myself. But he entirely ignores Semper's work of 1874, but for which I should never have commenced to study the *Comatulæ* at all, as I have already explained. He also forgets Greeff's observations at Naples in 1874, these were not published, however, till January, 1876, and that Ludwig wrote his first note in 1875; and he calmly states that he, Edmond Perrier, was »encore seul, en 1875, à avoir repris les observations de W. B. Carpenter sur les comatules«. His conclusion is that »Herbert Carpenter entrainé donc dans la carrière scientifique en fourbissant soigneusement ses armes dans l'intention préméditée d'attaquer mon premier travail sur les Crinoïdes«.

I repeat again that his statement is untrue, and I challenge him to prove it. Had I been actuated by the unworthy motive which he ascribes to me, it would have been easy to fill, not one but half a dozen pages, by exposing his blunders. Did I do so? Will he name one single passage in my first publication of 1876 or in any subsequent one up to 1883¹ which can possibly justify the statement that I have just

¹ I mention this date because Perrier now says that since 1883 I have been attacking his works. But he is silent as to 1875, the date he fixed at first.

quoted? He must prove it or withdraw it, and I leave him to decide which course he will take.

2) Let us now consider his reply on »le deuxième grief«.

He attempts to explain away his confusion of my father's observation with that of Müller by saying that one of two words (»vertical« or »transverse«) had been »défiguré à l'impression«; and he thinks it unfortunate that I did not perceive this. How should I? Which word is wrong? Does he mean to say that my father found the tentacular canal divided vertically by a vertical partition or transversely by a transverse partition? Will he quote a single passage in Dr. Carpenter's memoir of 1866 in which the tentacular canal is described as divided into two portions by any partition at all? He gave no reference in his memoir of 1873 in support of his statement, and I therefore ask him now to make good the deficiency. The only one partition mentioned by Müller is the vertical one in the tentacular canal; the only partition mentioned by my father is the horizontal one separating this canal from the coeliac canal below it. Until Perrier can bring forward previous proofs to the contrary I shall adhere to my statement that he did confuse these two structures. It is useless for him to attempt to explain the passage which I have quoted by the excuse that one of two words in it has been »défiguré à l'impression«. Whether this be the case or not, it does not alter the fact that Perrier attributed to my father a statement which he had never made, and then proceeded to criticise it unfavourably. I am quite ready to apologise to him if he can prove that he did not do so.

3) Perrier has stated as an excuse for his errors about the arm-canal that my father said nothing about having made any sections of the arms of Comatulæ. In reply I directed his attention to three figures which are described as sections of »decalcified arms«. His rejoinder is a curious one. For he speaks of them as representing »préparations de parties dures, qu'on peut supposer decalcifiées apres coup«. Why should he suppose anything of the kind when it was clearly explained that the sections in question were those of decalcified arms, while another section of an arm with the pinnules attached to which he refers as an excuse for his error was not so described? Does he really wish his fellow-workers to believe him incapable of understanding the explanation in question to mean that the arm was decalcified before the sections were cut?

It is perfectly true that Dr. Carpenter only figured the dorsal portions of these sections; but it is none the less true that he spoke of having made them, though Perrier has incorrectly stated that he did

not do so. In fact it was these very sections which enabled him to give his descriptions »de la forme et des rapports des canaux exclusivement contenus« in the soft ventral portions of the arm, descriptions of which Perrier now admits the accuracy, though he formerly denied it, owing to his not having taken the proper means to verify them.

Professor Perrier speaks of the long series of attacks which I have made upon his works since 1883. For these he has only himself to thank. Many of his observations, as no one knows better than myself, are of very great value; and I have always expressed myself to that effect whenever I have had occasion to notice them, as for example, in the January number of the Quarterly Journal of microscopical science see pp. 382, 383. My criticisms have been chiefly directed against his frequent and flagrant misstatements, and against his too hasty generalisations; and so long as he continues to commit himself in this way, so long must he expect to be criticised.

If he will take the trouble in future to understand the essential characters of existing genera before proceeding to establish new ones; if he will also take the trouble to make himself thoroughly acquainted with the observations of his predecessors before venturing to criticise them, and will refrain from giving grossly incorrect versions of their statements; if he will abstain from attributing to me views which I have never held and from making sarcastic comments upon those views; and if he will cease to make showy generalisations which are in direct and absolute contradiction to established zoological truths, without offering one particle of evidence in their favour; he will then afford no grounds for severe criticism, either by myself or by any other of his fellow-workers.

**4. Étude de la dent canine,
appliquée au cas présenté par le genre Damanet complétée par les
définitions des catégories de dents
communes à plusieurs ordres de la classe des Mammifères.**

Par Fernand Lataste, Paris.

eingeg. 5. April 1887.

I. Origine, but, méthode et plan de cette étude.

Au congrès tenu, l'an dernier, à Nancy, par l'Association française pour l'avancement des sciences, quand j'eus communiqué les résultats de mes recherches sur le système dentaire des Damans¹ et que je me fus efforcé de démontrer que la première dent

¹ Ces recherches ont été, depuis, publiées dans les *Annali del Museo civico di Genova*, s. 2, v. IV, 27 septembre 1886.

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Artikel/Article: [3. Further remarks upon Professor Perrier's historical errors 262-265](#)