

Aphides does not seem to me to rest upon any solid basis; and the general opinion which regards the most habitual mode of reproduction of the *Aphides* as a case of agamogenesis is alone true. I have, however, no pretension to claim, by the publication of this note, any scientific rights in connexion with the embryogeny of the *Aphides*. Those who have resumed the investigation of these singular phenomena at the point where Mr. Huxley had left it, and who have caused it to advance remarkably, are at present MM. Mecznirow and Balbiani alone. If I have taken up the pen, it is because there existed between these two observers such considerable differences upon one point, and that a fundamental one, that it was necessary to test their observations. But I feel perfectly that if, by these few lines, I assist in banishing an error from science, I nevertheless introduce no new fact. Definitively I leave matters where M. Mecznirow has placed them.

LIII.—*Remarks on M. Claparède's Note on the Reproduction of the Aphides.* By M. BALBIANI*.

ALTHOUGH M. Milne-Edwards has had the kindness to communicate to me M. Claparède's note before its insertion in the 'Annales,' I do not think it necessary to reply at the moment to the objections which the author endeavours to raise against my interpretation of the mode of reproduction of the viviparous *Aphides*, or to some perfectly gratuitous allegations which his paper contains. I think this reply will be better placed in the memoir, accompanied by plates, which I propose shortly to publish upon the generation of the *Aphides*. There is only one point in M. Claparède's note which I think it essential to notice here, namely that relating to the priority which he seems to claim in favour of M. Mecznirow for all the facts upon which our observations present a more or less complete agreement.

It is certain that M. Mecznirow, three months before my communications to the Academy of Sciences, published some researches upon the embryogeny of the Hemiptera, which appeared, as a preliminary notice, in Siebold and Kölliker's 'Zeitschrift.' But in this paper, which occupies in all four pages of the journal in question, the author devotes only a little more than one page to the development of the *Aphides*; and here he omits most of the more characteristic facts in the embryogeny of those

* Annales des Sciences Naturelles, 5^e série, tome vii. pp. 30-31.

insects. It is true that in a subsequent memoir, published ten months after his first notice (December 1866) and six months after my various communications to the Academy of Sciences upon the same subject, M. Mecznirow gives a more detailed description of them, and corrects some of his previous observations; but it is difficult to admit that in the interval he was unacquainted with my investigations, published in June 1866 in the 'Comptes Rendus de l'Académie;' and yet no mention is made of them in M. Mecznirow's last memoir. M. Claparède, who must excuse me for not having mentioned M. Mecznirow's first publication, would have done an act of justice if he had indicated that the latter observer had much less reason for not citing a work which appeared six months before his own.

As regards the reproach made to me by M. Claparède of having introduced confusion into the language of histology, I believe I have not in any way contributed to increase that which already reigns in it, especially as to the definition, formerly so clear and distinct, of the word *cell*. Notwithstanding the very arbitrary acceptation that any one may now-a-days give to this term, I have not had the boldness to extend it so as to designate by it "*whatever has a certain form*," as I am accused of doing by M. Claparède in his note. When I thought it proper to give the name of cells to the histological elements which I had before me, it was because I had ascertained the presence in them of at least the two constituent parts now recognized as strictly necessary to characterize a cell, in accordance with the recent works of MM. Max Schultze, Brücke, Häckel, and others, namely a nucleus and a protoplasmic mass. M. Claparède, who can see nothing in them but globules, nowhere mentions whether he has endeavoured to enlighten himself by the employment of reagents; and I have no doubt that a drop of acetic acid would have done him more service than the objectives of Smith and Beck, or the immersion lenses of Hartnack.

I can only regret that my investigations have not received the confirmation of an observer so distinguished as M. Claparède, who has taken the trouble to test them. Perhaps the fault may be that I have presented them with insufficient details, and especially that I have omitted to mention the means that I employed for the determination of facts which, for the most part, require minute and delicate observation. These are defects which I shall endeavour to supply in a more circumstantial work on the same subject.



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