

»Als ich jedoch die Gruppe der Tingiden untersuchte, fand ich bei einer ceylonischen Gattung, die ich der Güte des Herrn Prof. Haeckel verdanke, daß an der Basis des ersten Schnabelgliedes mit diesem verwachsen, zwei seitliche, articulierte Organe sich finden. Wenn diese nicht gegliedert wären, so müßte man sich der Huxley'schen Ansicht anschließen, daß die vier Borsten der Hemipteren nicht homolog den Mandibeln und Maxillen der anderen, kauenden Insecten seien. Da sie jedoch deutlich in drei Theile gegliedert sind, eine Zahl, welche genau der Zahl der Glieder der Palpi labiales bei Orthopteren entspricht, so bleibt nichts Anderes übrig, als sie mit diesen zu homologisieren und der Savigny'schen Ansicht beizutreten, wie sie Gerstfeldt nachher formuliert hat.«

Wenn nun auch H. E. Schmidt meine specielle Abhandlung nicht zur Hand war, so hätte er doch aus dem Zool. Anz. die Kenntnis von dem Vorhandensein meiner Arbeit über die Mundtheile der Hemipteren erhalten und diese berücksichtigen müssen.

Erwähnen möchte ich noch, daß ich bei meinen fortgesetzten Untersuchungen über diesen Gegenstand, im Jahre 1892 bei einer rumänischen Hemiptere die Lippentaster photographiert und im Zool. Anz. Jahrg. 1892 p. 145 veröffentlicht habe. Nähere Angaben werde ich in einer demnächst erscheinenden Arbeit mittheilen.

Neapel, den 13. September 1894.

3. The Nature of the Hermaphroditism of *Myzostoma*.

By J. Beard, D.Sc., University Lecturer in Comparative Embryology, Edinburgh.
eingeg. 27. September 1894.

In a recent number of this journal¹ Dr. Wheeler has published observations on the ovaries and »Complemental or dwarf males« of *Myzostoma*. His conclusions run directly counter to some results of mine contained in an »inaugural dissertation« published ten years ago. Respect for Dr. Wheeler's powers as an investigator would have led me to accept without challenge the accuracy of his statements on the present question, but, having previously been criticised with regard to some of the very points of which Dr. Wheeler treats, I had some time ago again taken the matter in hand and had made fresh investigations.

The manuscript of a small paper on the subject has been in my possession some five years. In that period I have, moreover, worked

¹ W. M. Wheeler, Protandric Hermaphroditism in *Myzostoma*. in: Zool. Anz. No. 447. 21st May 1894. p. 177—182.

over the ground several times and propose to publish elsewhere the evidence which leads me still to maintain my former position.

Here the question will be considered very briefly, and only in so far as my observations and conclusions contradict those of my present critic.

Dr. Wheeler has studied what Nansen termed the »problematical organs« of *Myzostoma*, and is led to confirm the latter author's supposition as to their ovarial nature. He goes further than Nansen, for he states that all previous observers, Lovén, Semper, v. Graff, Nansen, Proudho and myself, were in error in assigning an ovarial character to the peritoneal epithelium of the body-cavity, and he insists that the »problematical organs« are the only true ovaries of *Myzostoma*. Moreover finding, with Nansen, similar »problematical organs« in what I termed, and still describe as, the dwarf males of *M. glabrum* he concludes that these so-called males are merely young hermaphrodites, as von Graff originally supposed. This conclusion is, however, unsupported by any evidence that the »problematical organs« in the »males« ever give rise to ova — or to anything else!

The »problematical organs« are known to me from Nansen's description, but neither before nor since the publication of Dr. Wheeler's note have I been able to satisfy myself that they function as ovaries in the hermaphrodites. I have recently examined several fresh series of sections of specimens in which proliferating ovaries might have been expected to be present, and have also worked over all my old sections of 1884, but in none can I find evidence of the conversion of cells of the »problematical organs« into ova. Like Nansen I have seen large egg-cells embedded in or lying on the **outer cells** of the organs in question, but failed to detect any stages in the organ intermediate between such cells and the small deeply-staining cells of the organ itself.

So much in justification of myself and other previous observers. Unless Dr. Wheeler can produce strong evidence to the contrary, I certainly incline to the opinion that Nansen may have been in the right in believing his »problematical organs« to be rudimentary or vestigial. Whether male or female in character is beside the question, for an ovary or testis can only be known by its products.

My mind is quite open on the point at issue, and it does not affect my argument in the least whichever view be the right one. I have no wish to challenge Dr. Wheeler's positive statement that in the hermaphrodites these organs are ovaries, but when he insists that they are »the only true ovaries« agreement is out of question.

In another place it would be shown that ova are present in . and

take their origin from, the regular peritoneal epithelium, especially that furthest away from these »organs« i. e. near the periphery of the body, before Wheeler's ovaries begin to proliferate. The »problematical organs« may have been the original ovaries, as Nansen surmised, and they may still retain their ovarian functions, as Wheeler maintains, but they are not the sole ovaries. The body cavity is, as Wheeler points out, more extensive than formerly supposed, and its whole epithelium functions as a sexual organ.

It can be shown, and the evidence supported by figures, that the dorsal portion of the peritoneum above the gut-coeca is mainly ovarian in function, and various stages of the development of eggs from single cells of the regular peritoneal epithelium can be demonstrated. Wheeler's ovaries, if he be right in his contentions, only come into function later on, and then probably proliferate their ova directly into the uterus or near it, there to ripen.

From a study of specimens of *M. glabrum* of various sizes Wheeler is convinced that the youngest forms are those attached to the backs of the hermaphrodites, those fixed to the disc being slightly older and larger than the former.

As a matter of course we are in complete accord in holding that the so-called males on the dorsal aspect of some of the hermaphrodites have only the male organs in a high state of development²; but Wheeler is in error in his supposition that all the youngest specimens on the disc are larger than those seated on hermaphrodites. It is quite easy to find individuals on the disc of *Antedon* as small and smaller than the supposed males, and a comparison of the two, i. e. of very small hermaphrodites from the disc and of males from the backs of hermaphrodites, has furnished evidence strongly supporting my former conclusions.

For the purposes of comparison a series of males were taken and sectioned, and a corresponding set of what were presumably young hermaphrodites from the disc were treated in the same way, the size of the individual being estimated by the number of sections of a given thickness ($\frac{1}{133}$ mm). The comparison of eight males³, the sections of which range in number from 66 to 151, with twelve small forms (presumably hermaphrodites) with sections from 93 to 168 in number gave the following results:

a. The males were always full of ripe spermatozoa and sperm-

² p. 179.

³ The eight males yielded 66, 70, 78, 79, 90, 97, 128 and 151 sections respectively, while the twelve hermaphrodites furnished 93, 94, 97, 97, 100, 112, 138, 138, 140, 152, 160, and 168 sections.

mother cells (easily distinguishable from ova!) and all traces of female cells were absent⁴.

b. Every one of the forms presumably hermaphrodite contained undoubted ovarial cells, the largest ones had many large eggs, and even in the smallest specimens the peritoneal epithelium in various places was of such a character as to leave no doubt regarding its ovarial nature.

A fuller statement of these results, with figures, is reserved.

The facts just recorded take away the basis of Wheeler's hypothetical ideas of the »course of events« in the later life-history of *M. glabrum*. It becomes obvious that the »males« are not forms which, after the death of the hermaphrodite on which they sit, change into hermaphrodites by removing their quarters to a more favourable position near the mouth of their host. Their smaller fellows on the disc are already hermaphrodite while they are still male, and indeed there is not a particle of evidence to show that these males ever do become hermaphrodites.

Their position on the hermaphrodite is peculiar. It is very constant, and not as far forward as it would be if its sole purpose were to bring them near the mouth of the host. Sometimes a small form was found seated on the side wall, instead of on the back of a large hermaphrodite, and such specimens, which thus did not occupy the normal position of a male, invariably turned out to be hermaphrodite.

From a small table made ten years ago at Naples of 32 specimens of *Antedon* got on four different occasions, and all infested with *M. glabrum*, the following is gathered:

<i>Antedon</i>	♂ <i>M. glabrum</i>		males
1 had	2	and	1
7 had each	2 or more	and	0
14 had each	1	and	0
10 had each	1	and	1 or 2.

Among other things⁵ this table of the condition of things in a series of forms taken quite at random proves that the males occur far more frequently where only one hermaphrodite is seated on the host, and thus where there is least crowding on the disc. Where this latter occurs, the males are almost invariably absent, indeed I have no re-

⁴ Except the so-called ovaries of Wheeler; these may quite possibly be rudimentary or »accessory« testes in the male. At least Wheeler's observations prove nothing to the contrary.

⁵ It also disproves in another way Wheeler's idea of the manner in which the males become hermaphrodites, for on his hypothesis the »males« ought to be more abundant than is actually the case.

cord of a single case in which three or more large hermaphrodites sitting together had a single male among them. The frequency of the occurrence of a male and a hermaphrodite is very striking.

The presence of the »problematical organs« or Wheeler's ovaries in the males proves absolutely nothing, unless it can be shown that they produce ova in the males⁶.

This Wheeler admits his inability to prove, and in one of the encysted species, *M. pulvinar*, a form in which a small male and a large female jointly occupy a cyst — he confesses⁷ that he is »unable to maintain that the males are really young individuals which will ultimately develop into females after passing through a hermaphrodite stage«. He also adds »still I have seen nothing to render such a supposition improbable«. But why make any such unnecessary supposition, except to bolster up an argument which is otherwise untenable?

Many of the extreme cysticolous forms have been shown to be dioecious, and we also know that in them the male is far smaller than the female — diminutive in fact. A similar, though not quite so pronounced, minuteness is also characteristic of the male of *M. glabrum*. Does not the analogy of other cases, Rotifera, *Artemia*, *Sacculina*, Cirripedia etc., rather lead to a suspicion that the dioecious stage was here the primary one, and that the reduction in size of the males was associated with the adoption of the secondary condition of hermaphroditism? In some other cases parthenogenesis was the secondary state »chosen«. Parthenogenesis and hermaphroditism appear to be always mutually exclusive. The nature of both would be discussed elsewhere. Here it may be pointed out that in both the males tend to degenerate and disappear, but that their existence is far more endangered by parthenogenesis than by hermaphroditism.

The minute size of the undoubted males of the (female or hermaphrodite) extreme cysticolous forms is a factor of which Dr. Wheeler can no more offer an explanation than his predecessors, Nansen and Proudho. It is this minuteness of the male which strengthens me in a belief that the judgment of the nature of the hermaphroditism in these interesting animals here upheld is the only rational, the only correct one.

It is beside the point to say that the hermaphroditism is protandric, for it is invariably so, chiefly because as a rule it is the females⁸

⁶ The same is also true of Nansen's so-called oviducts in the males, as will be shown elsewhere. In my opinion the so-called lateral oviducts are nephridia.

⁷ p. 181.

⁸ A good instance of this is *Myxine* cited by Wheeler. Why is not sug-

which become converted into hermaphrodites, and therefore it must occur before the true female cells are properly differentiated.

This note has purposely been kept as short as possible in view of another publication. But perhaps it is sufficiently convincing to show that Dr. Wheeler has not quite succeeded in throwing new light on the hermaphroditism of *Myzostoma*.

4. Bemerkungen über die Nervenendigungen in den Hautsinnesorganen der Arthropoden, insbesondere Crustaceen.

Von C. Claus, Wien.

eingeg. 5. October 1894.

Die wichtigen Aufschlüsse, welche durch Anwendung der Methylenblaufärbung (Ehrlich) und der Chromsilbermethode (Golgi) für die Structur des Nervensystems und der Sinnesorgane auf dem Gebiete der Vertebraten gewonnen wurden, legten es nahe, die gleichen Untersuchungsmethoden auch auf das Nervensystem der Gliederthiere anzuwenden, um die bisherigen zum Theil auf abweichende Befunde basierten Anschauungen auf beiden Gebieten in Einklang zu bringen. Schon Retzius hat in seinen bewunderungswürdigen Arbeiten höhere Crustaceen (*Astacus*, *Palaemon*) und Gliederwürmer (*Lumbricus*, Polychaeten) sowohl auf den feineren Bau der Nervencentren als auf das Verhalten der peripherischen Hautsinnesnerven studiert, ohne jedoch auf die älteren Befunde eingehendere Rücksicht zu nehmen. In jüngster Zeit hat O. vom Rath, welchem die Wissenschaft mehrere werthvolle mit einfacheren Methoden der Untersuchung ausgeführte Arbeiten über die Hautsinnesorgane von Insecten, Myriopoden und Crustaceen verdankt, die Behandlungsweise von Ehrlich und Golgi auf eine Reihe von Arthropoden¹ angewendet und mittels derselben das, was er bereits vorher über den feineren Bau der Hautsinnesorgane ermitteln konnte, im Wesentlichen und nur mit geringen Änderungen der früheren Auffassung bestätigt.

In einer kürzlich veröffentlichten Schrift über diesen Gegenstand hat O. vom Rath zwar meiner älteren Angaben über die Nervenendigungen in den Hautsinnesorganen Erwähnung gethan, jedoch in einer Weise, welche einer bereits früher² von ihm vertretenen Mei-

gested that they have been rare males of this form derived from hermaphrodites?

¹ O. vom Rath, Über die Nervenendigungen der Hautsinnesorgane der Arthropoden nach Behandlung mit der Methylenblau- und Chromsilbermethode. Berichte der naturforschenden Gesellschaft in Freiburg i. B. Bd. IX. Heft 2.

² O. vom Rath, Über die von C. Claus beschriebene Nervenendigung in den Sinneshaaren der Crustaceen. Zool. Anz. No. 386. 1892.

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